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Introduction: The Method.

The problems for the present investigation have been selected with reference to the need of data, their interrelations, and the adaptation of methods and apparatus. Some are involved in standard experiments, some have been developed by other investigators and are here developed a step further, and some are new. Likewise the apparatus consists of some standard pieces, some adaptations and improvements, and some new pieces. Five independent studies are reported together in this article. There is no other reason for grouping them under a common head than that they were made upon the same class of persons and are in the nature of statistics.

The experiments have been made upon the members of the introductory classes in psychology in the University during the academic years '97–8 and '98–9. The students were Juniors and Seniors, and both sexes were represented. These students were fairly prepared for observing and yet did not know what results to expect in any particular test, and where

necessary they could be kept ignorant of the objective relations. All the members of the classes volunteered to make individual appointments for the tests, and the test was omitted upon no person except for such reasons as would be considered valid excuse for absence from the class. In every case we aimed to obtain the most favorable conditions for observation. Therefore the data to be presented are obtained from a well defined class of persons under similar conditions. They represent the best efforts of the university students.

As a rule the problems were not discussed in class prior to the testing. Careful precautions were taken to obviate any suggestion of what might be expected in the results. As the courses were elective, the students were naturally interested in the study of mental phenomena. After the test, copies of the records were furnished to the class, the results were discussed, and each individual could compare his or her record with the records of others. This added materially to the interest in the experiments, which is so essential to painstaking observation. The records often express striking peculiarities of sensibility and judgment and, as it is important that no one should be in danger of having his feelings injured by exposure before class-mates, no names have been given with the records. The students passed by assigned numbers which have been retained in this report as in the original report to the class, so that each student can identify his own but no other record. By means of these numbers, one individual's record may be traced through records of the various tests for which individual records are given. The determination of variations of mental processes with sex constitutes an important feature of this investigation, in which the opportunities and conditions for comparison have been specially favorable.

None of the following problems are treated exhaustively. All the time and energy might well have been devoted to one of the problems or a part of one. But most of the problems, e. g., those on illusions, are of such a nature that the best results can be obtained upon the first trial; then, the students would

have tired of repeated tests of the same kind and the experience would not have been so valuable to them as beginners; and, furthermore, most of the development of a problem can be carried on to the greatest advantage by the ordinary method with trained observers.[1] It is mainly when we wish to determine the nature of the naive experience or the uniformity of a particular tendency that we find it profitable to appeal to the statistical method.

These statistics that have been obtained in the laboratory have been supplemented by some tests made upon school-children. As the account of the latter occurs in fragments under the appropriate topics, it is necessary to state, in this introduction, how the data were obtained.

Six students in a university course in genetic psychology, under the direction of the writer, made these tests upon children in the public schools of Iowa City during the spring term of 1898. These students had previously pursued courses in psychology and pedagogy and undertook the investigation mainly for the sake of the training.

Ten boys and ten girls of each age from six to fifteen, inclusive, were selected from a grammar school and a primary school. Mr. G. C. Fracker, the principal of the grammar school, selected and graded the children. A fair distribution of the children was obtained by selecting, from the registers, those whose birthdays fell nearest the day of the appointment. The respective teachers invited them to come to the school rooms at different periods on Saturday mornings. They were received by a teacher who registered and guided them.

The registers called for the child's name, assigned number, age, sex, grade, standing according to the last monthly report, mental ability as estimated by the teacher, and remarks about known peculiarities. The last three items were obtained at the teacher's leisure. Each child carried a pass-card contain-

[1] The normal person experimented upon in a psychological laboratory is an "observer." He observes under the conditions imposed upon him by the experimenter. Psychologists are in danger of adopting the inappropriate term "subject" from the language of the pathological clinic.

ing the name and the assigned number. The results were classified directly according to problem, individual, sex and age. The experimenters occupied different rooms and the children passed singly from room to room as they were called. The children were called one period in advance of their respective turns so that each one had an opportunity to see another perform the test. Six minutes were allotted for each test; thus each child was required to work thirty-six minutes in all.

The six students pursued different investigations. Five of the six problems which constituted the series are reported in this article, namely: (1) Hearing-ability, (2) Discriminative sensibility for pitch, (3) Illusions of space, (4) Illusions of weight, (5) Illusions of time. In the preliminary work they studied their problems, tested their apparatus, and practiced until they became expert in their respective measurements. The writer is responsible for the method, the apparatus, and the final statement of the results.

All the sections of this report are condensed into a compact form in order that the results may be accessible. Historical discussions are avoided as far as possible. Likewise all discussion of theories is excluded, except in so far as it is necessary for the statement of the problems or follows directly upon the conclusions drawn from the experiments. It is earnestly hoped that this procedure will not be construed as indicating a lack of recognition of what the writer owes to previous investigators.

The writer herewith extends his sincere thanks to all the students who have contributed to the success of these experiments by acting as observers. The names of those students in the laboratory who have assisted by performing certain series of experiments are appended to the corresponding sections of this report, in grateful acknowledgement of the services rendered. The writer most cordially acknowledges his obligations to Professor Patrick for constant promotion of the work.

I. Visual Perception of Interrupted Linear Distances.

In this investigation an attempt is made to determine the extent and the nature of some of the variations of the Müller-Lyer illusion for untrained observers to whom the illusion has not been explained. Other illusions are dealt with incidentally. The tests were made upon the students in the introductory class in psychology before they had studied the subject of illusions. It was, however, apparent to all that some illusion was involved in the test.

First Series: List and Explanation of the Forms.

(*Miss Mabel Williams assisted in making this series of tests.*)

The numbers in this list refer to the corresponding numbers in the reproductions of the forms on page 9-13. The positions of coins are represented by circles. Where coins were not used the figures are copies of the original figures. The forms in the plates are reduced to one-third of the original size. They are drawn so as to express the results of this test; i. e., they are drawn according to the averages of the estimates recorded in Table I. The short vertical marks indicate the true distances. The distances between these marks and the limiting edges of the end-figures represent the amount of the illusions.

The description of the following forms of figures involving the illusion may be facilitated by observing two fundamental types.

Type A. This is the type of Form 6. Two silver dollars are placed in a horizontal line and separated by a distance equal to the diameter of a dollar. A third dollar is placed so far to the right of these, in the same line, that the distance between the left edge of this and the right edge of the middle dollar shall appear to be equal to the distance between the right edge of the middle and the left edge of the first dollar. That is, the distance between the adjacent edges of the second

and the third shall appear to be equal to the distance between the remote edges of the first and the second. This is the double form of the Müller-Lyer figure. In the standard member the angle-lines point inward and in the other member they point outward. Form 6 shows graphically the average amount of the illusion as determined by the present experiments. The mark to the right indicates where the inner edge of the third dollar should be in order to make the compared distance equal to the standard distance.

Type B. This is the type of Form 22. Two silver dollars are placed in a horizontal line and separated by a distance equal to twice the diameter of a dollar. It is required to indicate the middle point between the extreme left-hand edges of the two coins. The total distance here to be divided is equal to the standard distance in Type A. The division mark is placed according to the present estimates. The actual middle point is not indicated.

Form 1. Five dimes[1] are used. This form varies from Type A in that the standard member is composed of four coins instead of two. The four dimes are placed in a line and separated by inter-spaces equal to the diameter of a dime. The fifth dime is to be placed so that the distance between the adjacent edges of this and the fourth shall appear to be equal to the distance between the remote edges of the first and the fourth. Standard, 124 mm.

Form 2. Four dimes are used. This form differs from Form 1 in that the standard member is composed of three coins instead of four. Standard, 89 mm.

Form 3. Three dimes are used in the form of Type A. Standard, 54 mm.

Form 4. Type A. Silver quarters. Standard, 72 mm.

Form 5. Type A. Silver half-dollars. Standard, 91 mm.

Form 6. Type A. Silver dollars. Standard, 114 mm.

[1] The actual diameters of the coins are as follows: dollar 38.10 mm.; half dollar, 30.48 mm.; quarter, 24.13 mm.; and, dime, 17.78 mm. Sufficient allowances were made in the interspaces of the standard members to eliminate the fractions. The coins were new.

Form 7. This form differs from Type A in that the two members are separated by the duplication of the middle coin. The standard member is composed of two silver dollars as in Form 6. The compared member is formed by placing two dollars in the same line, to the right, at such a distance apart that the distance between these two shall appear to be equal to the distance across the other two.

Form 8. The standard member is retained as in Form 6. A blank paper is placed immediately to the right. It is required to indicate the apparent distance between the two coins by two dots on the blank paper. Standard, 38 mm.

Form 9. This form differs from Form 6 in that the two dollars of the standard member are placed edge to edge. Standard, 76 mm.

Form 10. Type A. This is composed of plain circles, each having an outside diameter equal to the diameter of a dollar. Standard, 114 mm.

Form 11. Type A. The circles are filled with concentric circles which constitute an attractive design. Standard, 114 mm.

Form 12. Type A. This form consists of Form 10 with the addition of a base-line.

Form 13. Type A. The end-figures consist of plain squares. Standard, 114 mm.

Form 14. Type A. The limiting figures consist of isosceles triangles, placed as represented in the cut. The horizontal distance through each triangle is 38 mm.

Form 15. This form differs from Form 14 in the omission of the vertical sides of the triangles. Standard, 114 mm.

Form 16. This differs from Form 10 in that semicircles are used instead of circles. Standard, 114 mm.

Form 17. This consists of Form 16 with the addition of horizontal lines, 19 mm. long, to each end of the semicircles. Standard, 114 mm.

Form 18. This and the next two forms differ from Type A in that the two members are placed in different directions. The standard member consists of two silver dollars as in Form

6. It is required to place a third dollar at such a point above these two that the distance between the adjacent edges of this and each of the other two shall appear to be equal to the horizontal distance in the standard member. Standard, 114 mm.

Form 19. This form differs from Forms 6 and 18 in that the second member is formed by placing the third dollar vertically over the second. Standard, 114 mm.

Form 20. This form differs from Form 6 in that the standard member is placed in a vertical position. The second member is produced by placing the third dollar to the right of the lower dollar in the standard member. Standard, 114 mm.

Form 21. A silver dollar is placed on a blank paper and the observer is required to indicate its diameter by two dots on the paper, to the right of the coin.

Form 22. Type B. Silver dollars. Standard, 57 mm. In this and the following four forms the measurement is made upon the section to the right of the middle point.

Form 23. Type B. Squares, with sides 38 mm. long, constitute the limiting figures. Standard, 57 mm.

Form 24. Type B. The limiting figures consist of angle-lines of the same length and forming the same angle as in Form 15. Standard, 57 mm.

Form 25. Type B. The diameter of the large circle is 114 mm; that of the small circle, 19 mm. They are 114 mm. apart. Standard, 114 mm.

Form 26. Type B. The diameter of the large circle is 152 mm. The inside circle, whose diameter is 19 mm., is 114 mm. to the right of the left hand edge of the large circle. Standard, 57 mm.

Apparatus and Method.

The twenty-six forms were put on separate cards, of a silvery tint, 57 cm. long and 38 cm. wide. Where the coins were not used the moveable end-figure was drawn on a carefully trimmed card of the same color as the background. The adjustment was made by moving the adjustable coin or

PLATE I.

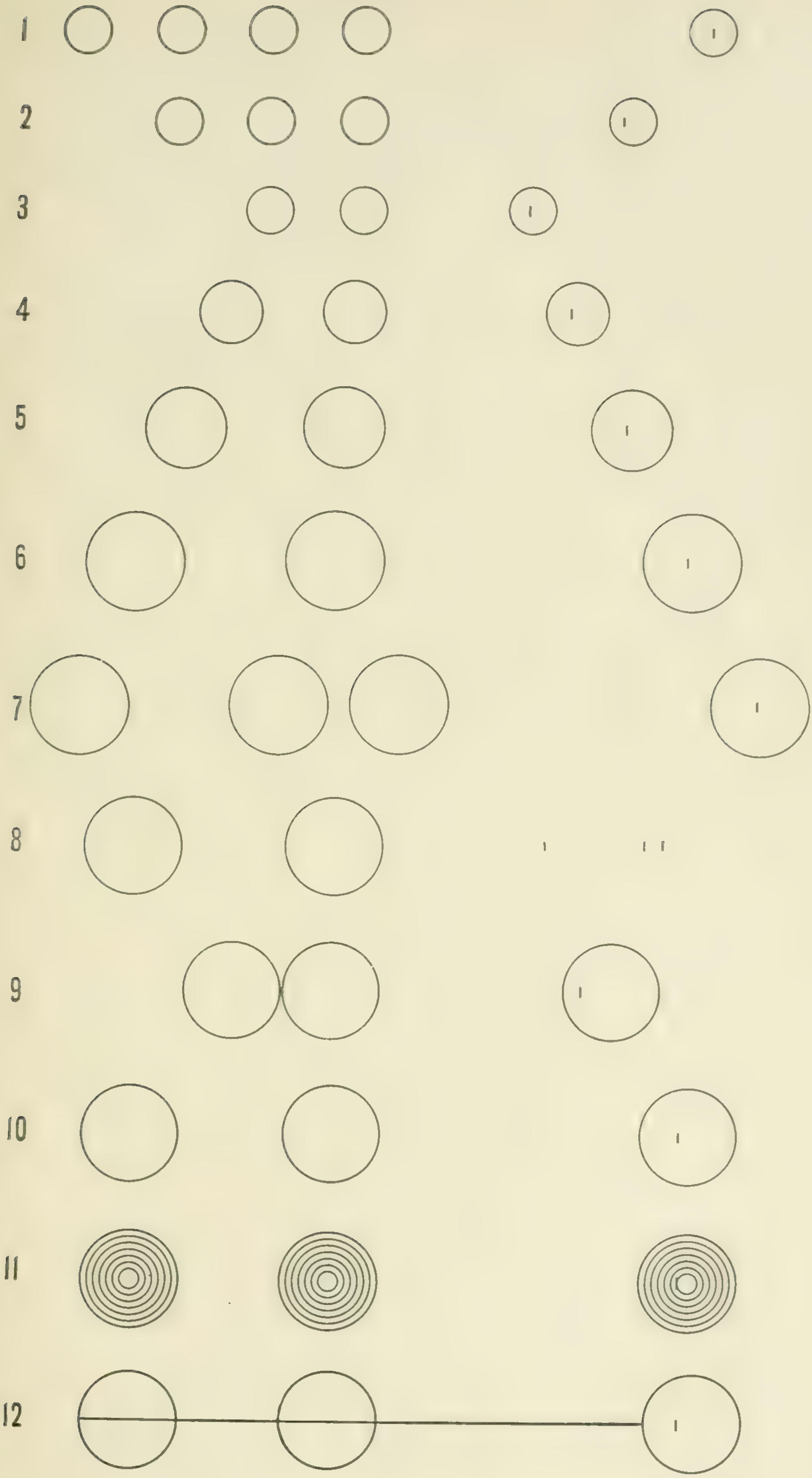


PLATE II.

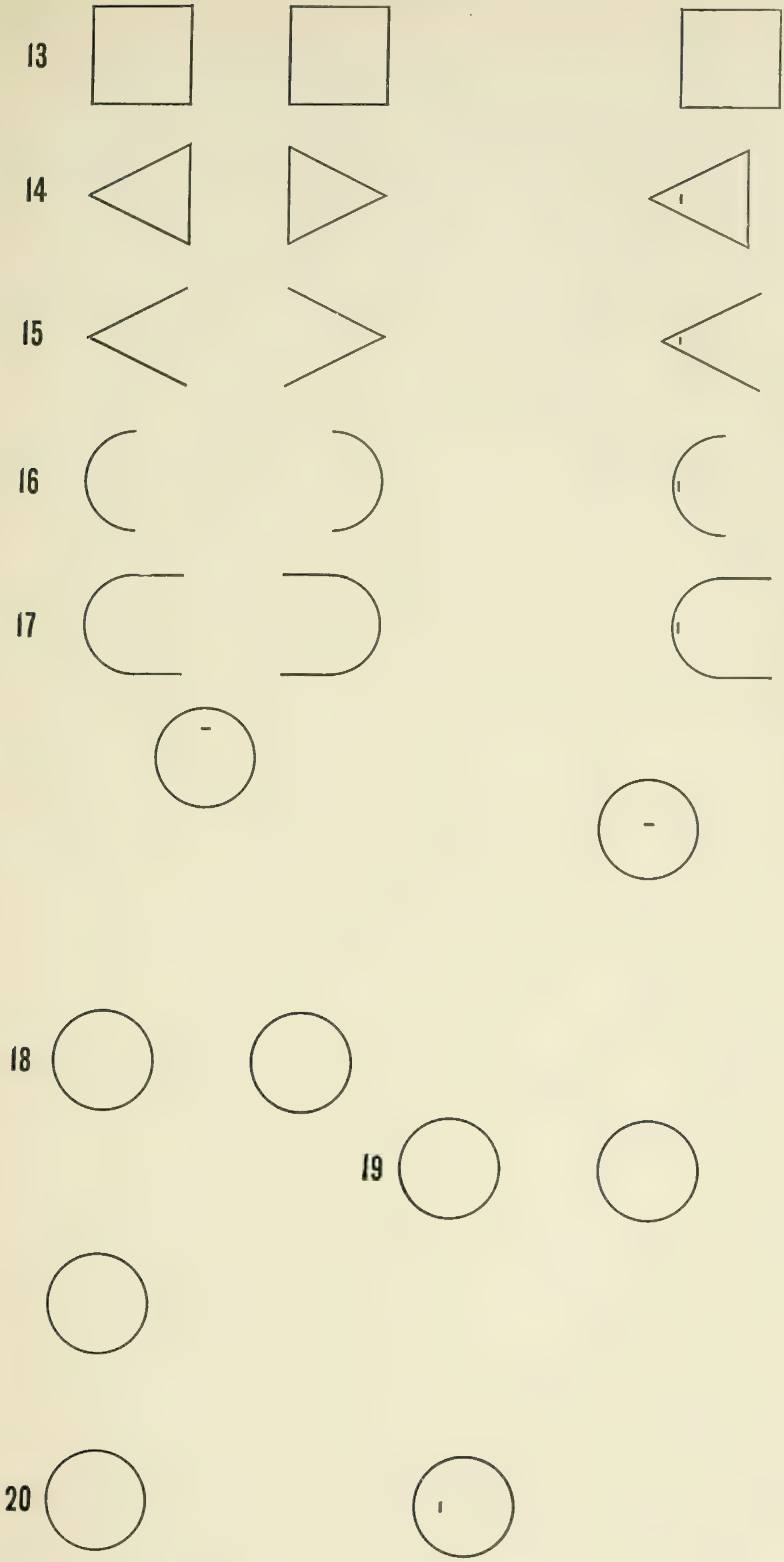
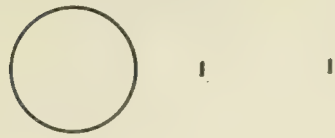
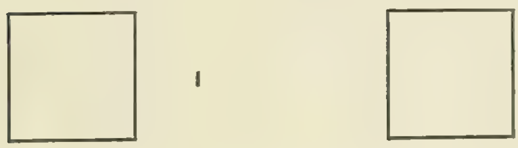


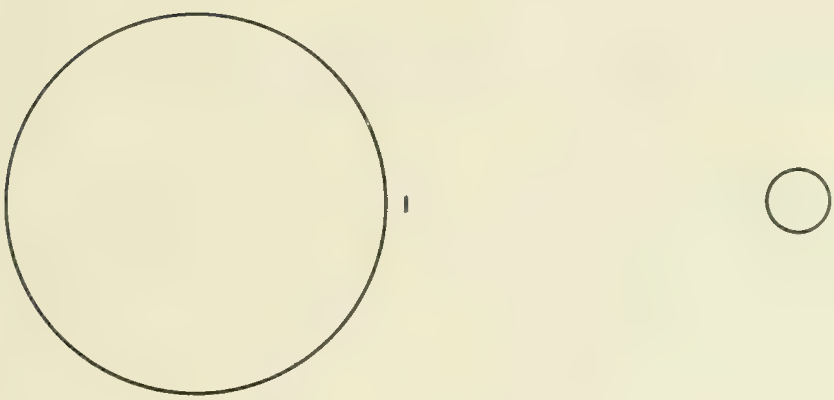
PLATE III.

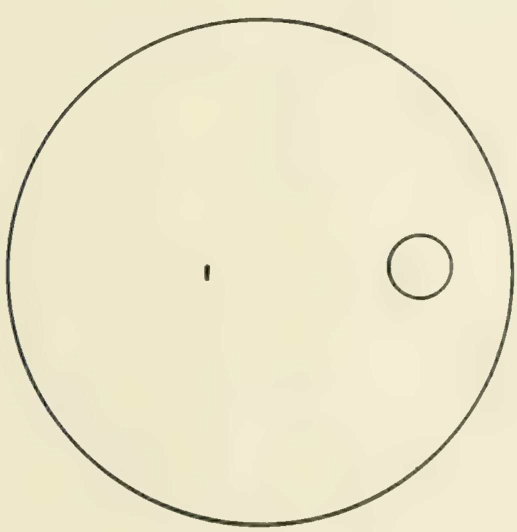
21 

22 

23 

24 

25 

26 

card with the rubber end of a pencil. In the trials on the forms of Type B, the bisection was made by placing the point of a triangular limb from a pair of fine compasses at the middle point. The observer was seated at a table, 70 cm. high, in such a way that he looked down upon the cards which were laid down flat. The cards were presented in order without any preparatory statement about the nature of the series. The standard member was always kept to the left. Four trials were made for each form, in the double fatigue order; i. e. two trials were made on each form in passing over the series the first time and then two more in returning over the series in the reverse order. The forms are so arranged in order as to eliminate the error of sequence as much as possible. Discussion of the illusion was evaded and no records were given out until the test had been completed.

The following conditions must be remembered in the interpretation of the results as expressed in the accompanying figures.

First, the figures are reduced in size and are therefore not supposed to bring out the original effect. Second, there are individual variations. These results are averages and do not represent fully any one of the individuals tested. Third, pointing out an illusion weakens it. In making the tests we aimed to obtain the conditions of every-day perception. The reader has the advantage of having the illusion pointed out. And, fourth, the majority of normal illusions cannot be discovered by unaided observation; they must be discovered by experiment, just as the micro-organism must be found with a microscope. They are nevertheless there and are stern realities. The proper way to verify these records is to make the test upon a person to whom the illusion has not been explained. Forms 6 and 18 are convenient for this purpose. The prime condition in the study of illusions is that the naive judgment shall be obtained without any disturbing suggestion or warning.

Statement of the Variations of the Illusion in the First Series of Forms. (*See Table I and Plates I, II, and III.*)

The forms of the illusion in this series are so selected and arranged as to bring out the effect of variations in size, relief, attractiveness of design, presence or absence of base-line, shape of the limiting figures, number and mode of interruptions, direction of the members, mode of division, and relative position of the limiting figures. There is also a basis for observation upon the influence of practice and the difference in the force of the illusion for males and females.

1. Size of the Coins. Forms 3, 4, 5, and 6, composed respectively of dimes, quarters, half-dollars, and dollars, produce different degrees of illusion. The force of the illusion decreases with the size of the coin. The illusions are: for dimes, 12 per cent; for quarters, 13 per cent; for half-dollars, 14 per cent; and for dollars, 15 per cent of the respective standard distances.

As the accompanying reproductions of the forms are only one-third of the original size, they do not convey the original intensity of the illusion.

2. Relief and Attractive Contour. The part that relief plays in this illusion is brought out by a comparison of Forms 6 and 10. The circles in Form 6 indicate the relative size and positions of the coins, while in Form 10 the diagrams themselves constitute the form. In the former case the illusion is 15 per cent, and in the latter, 13 per cent. The relief increases the illusion by 2 per cent.

That this is not due to the attractiveness of the surface of the coin is demonstrated by a comparison of Forms 10 and 11. The design of the limiting figures in the latter is more attractive to the eye than the face of a dollar, but the illusion in this form is the same as for the plain circles, 13 per cent. The outline is clear and well defined in all the forms of this experiment. Other tests indicate that clearness in the outline does not add to the strength of the illusion. Complexity of outline, as in the design of a wall paper, increases the force of the illusion. It also appears that the fainter the outline is the more the eye strives to follow it.

Table I.

The Illusions in Forms 1-26.

		1st Estimate		*4th Estimate*		*Ave. Estimate*				*Ave. Illusion*	
Form	*Standard*	*M*	*W*	*M*	*W*	*M*	*ad*	*W*	*ad*	*mm*	%
1	124	115	105	117	120	117	7	112	8	9½	8
2	89	83	77	90	86	86	5	83	6	4½	5
3	54	45	44	50	50	46	3	47	5	7½	12
4	72	61	58	66	65	62	4	60	4	9	13
5	91	76	73	80	81	79	5	78	5	12½	14
6	114	97	93	104	98	100	6	94	4	17	15
7	114	97	91	103	98	99	5	94	6	18	16
8	38	46	45	45	45	47	2	44	3	7½	20
9	76	65	64	71	74	68	4	71	6	6½	9
10	114	97	90	104	103	100	5	98	7	15	13
11	114	98	96	101	101	100	5	99	5	14½	13
12	114	96	98	105	101	102	4	101	5	12½	11
13	114	112	110	114	120	113	5	113	4	1	1
14	114	101	102	103	104	100	4	102	8	13	11
15	114	106	104	108	109	107	4	106	6	6½	6
16	114	112	110	109	115	111	4	112	4	2½	2
17	114	111	112	110	115	111	4	113	5	2	2
18	114	83	78	91	87	87	3	81	6	30	26
19	114	91	88	98	95	94	5	92	4	21	18
20	114	101	99	107	101	105	5	101	5	11	10
21	38	40	37	39	38	39	1	38	1	½	1
22	57	55	54	58	57	56	2	55	2	1½	3
23	57	56	56	58	58	57	2	57	2	0	0
24	57	52	54	53	53	53	1	53	1	4	7
25	114	108	106	107	107	107	1	109	2	6	5
26	57	52	53	54	53	54	1	54	1	3	5

M, men; *W*, women.

ad, average of the mean variations. (This is the average of the variations found for each observer by taking the average of the variations of each record from the average of the four records on each point, regardless of sign).

mm, the number of millimeters by which the compared member was overestimated. This is the mean between the men's and the women's records, but there were twenty-eight male and only eighteen female observers.

%, the per cent of the standard distance that the illusion represents.

The "Average Estimate" is the average for the four trials of which the first and the fourth are recorded separately.

Compare the data of this table with the diagrams in Plates I, II, and III.

3. *Introduction of a Base-line.* The base-line lessens the illusion. The illusion is 2 per cent less in Form 12 than in Form 10. The line makes the distances to be compared more definite and tends to keep the eye from wandering over the outline of the limiting figures.

4. *Form of the Limiting Figures.* The variations are represented in the figures of Forms 10 and 13–17. The illusion is greatest for the circles (Form 10, 13 per cent) and smallest for the squares (Form 13, 1 per cent).

In Form 8 of this series it is demonstrated that the middle distance in the standard member is greatly overestimated when viewed by itself. When the distance across the whole standard member is viewed the case is reversed. The standard member is perceived to be shorter than it really is, not so much on account of the underestimation of the horizontal diameters of the circles as on account of the generous contraction of the middle space. This paradox is not involved in Forms 13, 14, and 15, where the suggested form of the middle space is a square. It seems strange that the strongest illusion should appear in the form that presents such conflicting conditions as Form 10.

At first sight it would appear that there is practically no illusion in Form 13, and it is generally so stated. The case is an interesting one because it is typical of what we find in many other conditions that are generally overlooked. The relative absence of any illusory effect is due to the conflict of two or more illusions which tend to cancel each other. The width of the space between the standard members is underestimated because the upper and lower limiting lines are absent and the eye sweeps freely out over the adjoining space with the result that the width of this space is perceived as the distance across the neck of a dumb-bell figure. The conditions are similar for the long space constituting the second member. It appears to be shorter than it really is, while the corresponding distance in Form 10 appears to be longer than it really is. And the standard member in the square-form appears to be longer than the standard member in the circle-

form. Therefore in placing the third square, there should be a tendency to place it too far out. As a matter of fact this does not occur, but it is placed a trifle too far in. The conditions are quite complicated. There are probably four illusions involved. These are arranged in pairs and the two opposing pairs practically balance each other. On the one side is the tendency to underestimate the distance through the open space in the second member. This is represented by (1) the tendency just stated as due to the absence of horizontal limiting lines, and (2) the well known tendency to underestimate open space as compared with filled or interrupted space. Both of these are undoubtedly present. On the other hand these are counteracted by (1) a vague effect of the Müller-Lyer illusion (Perhaps the eye sweeps over the body of the figure instead of along the limiting lines.), and (2) the tendency to overestimate a long distance which is compared with a short distance as in the comparison of the two sides of a double square.[1] To illustrate the last case, which may be doubtful, the perception of the linear distance in question depends upon the conception taken of the area which it runs through. If this area is thought of as a dumb-bell figure, the distance will be underestimated; but if it be thought of as a

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[1] The reversal of the tendency to overestimate the vertical under certain conditions was discovered in some of our experiments in another investigation. A wide, white celluloid frame was made to surround a pink colored surface 101 mm. wide and 270 mm. long. The length of this area could be readily changed by pushing in white celluloid slides from the ends. The frame was placed in a horizontal position, at arms-length from the observer's eye, and perpendicular to the line of vision. The observers, thirty-seven males and twenty-five females, were students of psychology who had not yet studied illusions. They were first asked to form a perfect square. Seven made it right, within 1 mm., eighteen made the horizontal measurement too short by an average of 3 mm., and thirty-seven made the horizontal too long by an average of 5 mm. The total average overestimation of the horizontal is 2 per cent of the standard. They were then asked to form a double square, longer horizontally. Forty-eight made the horizontal line too short, by an average of 15 mm. (mean variation, 8); and fourteen made it too long, by an average of 10 mm. (mean variation, 6). On the whole the horizontal distance was made 4½ per cent *too short*. (See same for children, below.)

rectangle determined by the limiting squares, the distance will be overestimated. These two tendencies are then reciprocal.

The illusion for the triangle-form is 11 per cent, which is 2 per cent less than the illusion for the circle form. It would seem that the directing force of the triangle should be greater than that of the circle. This is apparent to every observer, and becomes a plain cause for conscious or semi-conscious reaction. Most of the observers remarked that the sharp angles are very delusive. No one gave evidence of having an equal apprehension in regard to the circles. This apprehension of strong illusion may be even greater for Form 15, where the illusion amounts to 6 per cent only. However, when compared directly as in Plate II, the standard member in Form 14 appears to be as much shorter than the standard member in Form 15 as is necessary in order to account for the difference in the illusion. The difference in the apparent length of the standard members obtains also when base-lines are added in these two forms.

There is an illusion of only 2 per cent in Form 16. The extension of the semicircles in Form 17 does not change the illusion.

5. *Direction of the Members.* A vertical distance is overestimated when compared with a horizontal distance. In Forms 18, 19, and 20, this illusion is combined with the Müller-Lyer illusion. The resulting illusion is strongest in Form 18, where it amounts to 26 per cent.

In Form 20 the tendency to overestimate the length of the standard member on account of its vertical direction counteracts the Müller-Lyer illusion, making the resultant 5 per cent less than the simple Müller-Lyer illusion as in Form 6.

But in Form 19 the Müller-Lyer illusion is re-enforced by the illusion of the vertical in the second member. The resultant illusion is 3 per cent greater than the single Müller-Lyer illusion as in Form 6.

When a line compared with a horizontal line inclines from the vertical position, the overestimation of this line decreases with the increase of the amount of inclination. If the inclina-

tion of the compared member were the only condition for variation of the illusion in Forms 18 and 19, the illusion should be weaker in the former, where the member slants. But it is much greater. There are probably two new elements that contribute toward the total illusion in Form 18. There is a double estimation to be performed, and the position of the third dollar suggests the bisection of the standard member.[1] On account of the middle position of the third dollar, there is a tendency to converge all eye movements toward a point just above the middle of the horizontal line that joins the diameters of the two dollars. In so far as this takes place, it introduces the effect of bisecting the horizontal distance. The two-fold estimation requires greater effort than the single comparison and it is probable that the increase in effort also tends to strengthen the illusion.

6. Number of Interruptions. The illusion of filled as compared with empty space probably enters into every form of the Müller-Lyer illusion. As a general rule filled space or interrupted space seems greater than open space, but the rule suffers many apparent exceptions. The exception may be due to a counteracting or to an overshadowing illusion. In Forms 1 and 2 the overestimation of the interrupted and filled space is increased by the insertion of more coins in the standard member than are required for the regular Müller-Lyer figure. The Müller-Lyer illusion is thereby weakened. The standard distance is increased without increasing the span of the limiting figures. The recorded illusion for these two forms represents the Müller-Lyer illusion minus the illu-

[1] Anticipating the need of a special explanation of the extraordinary force of the illusion in Form 18, I inserted an extra test in this series. It came between Forms 20 and 21 in order. A horizontal line was drawn 114 mm. long, and a long perpendicular was erected at the middle of this. The observer was directed to cover the upper part of the vertical line with a strip of paper so far that the exposed part of the vertical line should appear to be equal to the whole length of the horizontal. The average estimate of the whole class makes the vertical line 9 per cent too short. This is a double illusion. We shall not be far wrong if we attribute one half of it to the comparison of a vertical line with a horizontal and the other half to the bisection of the horizontal.

sion of filled space. This is 5 per cent for Form 1, and 8 per cent for Form 2, as compared with 12 per cent for Form 3. Theoretically the resulting illusion should have been smaller for the five dimes than for the four. This reversal is due to a constant tendency found in all our experiments upon normal illusions, namely, the tendency to give the illusion in the first trial fuller sway. There is, of course, some margin for chance variations. With the introduction of more coins in the standard member, the ratio between the two illusions would undoubtedly vary in a series so that the illusion of filled space would soon be the stronger.

In the following forms of this section it was found more convenient to use dollars than dimes. The variation between Forms 6 and 7 is practically immaterial, the illusion in 7 being only 1 per cent greater than in 6. Psychologically these two forms are alike, but Form 6 is more convenient and the conditions for comparison are there more favorable.

The elimination of the middle space in the standard member of Form 9 changes the proportions of the form, makes the standard more definite, and facilitates the perception of the compared distance. These conditions coöperate in reducing the illusion from 15 per cent in Form 6, to 9 per cent in Form 9.

This effect becomes quite paradoxical in view of the fact that the eliminated distance is greatly overestimated. This is demonstrated in Form 8. The compared member involves no illusion by the method employed, therefore the 20 per cent illusion expresses the actual overestimation of the middle distance of the standard member in all these circle-forms.

It would be natural to look in Form 21 for the reciprocal of the effect of the middle space in Form 8. But in Form 21 there is no illusion. It is generally known that the effect of diverging angle-lines is greater than the effect of converging angle-lines. The influence of the edges of the coins is also reduced because these are arcs of a circle, a perfect figure. The diameter of a circle is not elastic under the illusory influences.

The consequences of placing an inelastic figure in juxtaposition with an elastic figure will be noticed later.

Thinking that this non-elasticity found in Form 21 was due to the relief or to some special associations with the coins, we made some trials with a plain circle which was of the same diameter as the dollar. But the diameter of the circle is also estimated correctly by the same method.

7. *Mode of Division.* The second type of the illusion is represented in Forms 22–26. In the first three the shape of the limiting figures is varied. The dollars produce an illusion of 3 per cent, the squares apparently no illusion, and the angle-lines an illusion of 7 per cent.

The conditions for the perception and comparison of distances are here essentially different from those in the previous type. The middle space is overestimated in Form 22, and this tends to counteract the illusion. The converging forces of the two sides of the left-hand dollar nearly balance each other and the illusion depends mainly upon the inner side of the right-hand dollar.

The middle distance in Form 23 may be underestimated or overestimated according as the middle space is considered as unlimited or limited by parallel lines above and below. The Müller-Lyer illusion is probably not involved here at all, because the sections of the divided distances are so small in comparison with the size of the squares.

In Form 24 the force of the angle-line illusion is increased by the relative shortness of the standard distance. Shortening of the standard distance has the reverse effect in Form 22.

The size of the end-figures is next varied. In Form 25 the middle point is placed 5 per cent too near the small circle. The distance between the two circles is overestimated. In making the comparison the diameter of the circle is first compared with the whole distance between the two circles and then the apparent difference is distributed. No one placed the middle point inside of the large circle. If a direct comparison were made as in the previous forms, the right-hand edge of the large circle would not be clearly apperceived, and an element of contrast would also enter.

The illusion persists with equal force when the small circle is placed inside a large circle as in Form 26. The apparent middle point is placed 5 per cent of the standard distance too near the small circle. Here the illusion is due to the combined angle-line effect and the contrast of the limiting arcs.

Practice. The influence of practice is eliminated as far as possible in this series. It was, however, unavoidable in two respects, namely, (1) through the four trials upon each form, and (2) through the succession of twenty-six similar forms. The difference between the estimates for the first and the fourth trials may be seen in the table. Much of the decrease in the average of the last estimate for the class is due to a rather excessive reaction of a few observers. The illusion is strongest at first glance, before the observer has become fully orientated. The most important element in the practice is the gaining of knowledge about the presence and force of the illusion. The effect of practice is evidently the same here as in illusions of weight,[1] for which I have proved that so long as the observer remains ignorant in regard to the presence of the illusion, it will not decrease by practice. Practice tends only to make it more uniform.

Variation with Sex. Compare the average estimate for men and for women. The men are nearer right in the greater number of cases and by the greatest amounts.

Compare the records for men and for women in the first and the fourth estimate. There is a greater difference between the first and the fourth estimate for women than for men. This reaction is also a sign of suggestibility. The women start with a strong illusion and then respond to the growing suggestion that some allowance should be made.

Compare also the average mean variations. The men have the smaller mean variation. The variations with sex are all small and would be insignificant were it not that they seem to be fairly constant.

[1] Stud. Yale Psych. Lab., 1895, III. 1.

Second Series: Influence of the Magnitude of the Angles and the Length of the Angle-lines.

In our preliminary work we repeated a part of Heymans' experiments[1] upon the influence of variations in angles and the length of the angle-lines. These tests were made in connection with the study of the formation of the square and the double square. (See note p. 19.) A simplified form of Heymans' apparatus was obtained by drawing one member of the figure on each end of the slides in the frame employed in the formation of the squares. The slide that went under the other did not have more than one end-figure, as the middle angle-lines were common to both members.[2] The adjustment was made by pushing in one slide until the two members appeared to be equal. The forms were all of the same type, being the double Müller-Lyer figure, composed of a base-line and straight angle-lines. (Form 15 with base-line.)

Thirty-one male and twenty-six female students were tested; only one trial was made on each point. In the records of the males and females, there is no difference that is worth recording; the average for all is therefore stated.

The constant base-line was 95 mm. Calling the angles A and the angle-lines L, the variations in the figures and the results may be stated as follows:

(1) A 30°, L 30 mm., estimate 75 mm., illusion 21 per cent.
(2) A 15°, L 30 mm., estimate 73 mm., illusion 22 per cent.
(3) A 60°, L 30 mm., estimate 79 mm., illusion 16 per cent.
(4) A 30°, L 45 mm., estimate 78 mm., illusion 18 per cent.
(5) A 30°, L 15 mm., estimate 81 mm., illusion 15 per cent.

As far as they extend, these results agree with those found by Heymans. The illusion is strongest with the angle of 15°,

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[1] Heymans, *Quantitative Untersuchungen über das optische Paradoxon*, Zeitschr. f. Psychol. u. Physiol. d. Sinn., 1895, IX. 221.

[2] Placing the joint of the slides at the vertex of the middle angle, as Heymans has it, cancels a part of the influence of that angle. It is better to place one-third of the adjustable member upon the slide that contains the standard member.

and for angle-lines of 30 mm. It is remarkable that the illusion here found is no stronger than the illusion found for the trained observer by Heymans. Yet the illusion in this series is stronger than the illusion in the preceding series. This difference is due partly to the difference in knowledge or suspicions about the illusion, and partly to the smaller number of trials. (Compare the first of these forms with Forms 12 and 15 in the first series.) The tests were not made in the order in which they are described here; this "Second Series" was the first general test upon visual illusions made in this laboratory. The "First Series" of tests were made a year later and, although they were made upon a different class, many of the observers were prejudiced on account of information received from the foregoing class. While this test is not so reliable as the other, I think that it well represents the force of the illusion in the naive state of mind, when a single, unbiased estimate is made under its influence.

Third Series: Elasticity of the Apparent Distance through Open Space.

Comparison of the results obtained in the tests with Forms 8 and 21 in the first series brings out the fact that the illusion of a distance through open space has free sway, and that it is checked in a distance through a figure of known form. According to the results for Forms 19 and 20 above, the mean overestimation of the vertical is 4 per cent of the standard distance. That is a probable amount for the total overestimation of the whole vertical member regardless of any other illusion. If this error were evenly distributed among the three sections of the standard member in Form 19, the circles should appear ellipsoid, with a longer vertical axis. This they do upon very close examination but, in the ordinary methods of estimating, the circles do not appear to be distorted, and yet the overestimation of the vertical takes place. If this is true the whole distortion is forced into the distance through

the space between the circles which is unlimited laterally. That would make the illusion of the vertical, that is admitted through the middle distance in Form 19, about 12 per cent, which is too much for the simple illusion of the vertical.

If four circles of the same diameter are arranged in a rectangular form so that the vertical and the horizontal distances between the circles are equal to the diameter of a circle, the elasticity of the middle distance may be seen quite readily. From the foregoing we know that both the vertical and the horizontal members appear to be shorter than they really are (counting a member to consist of the two circles and the intervening space). The vertical members appear to be longer than the horizontal members. The circles do not appear to be distorted unless the distortion is looked for specially. When a vertical middle space is compared directly with a horizontal middle space the former appears to be the longer, and the difference is at least twice as apparent as the difference between the whole members. And, if the vertical and the horizontal middle distances are compared indirectly, in terms or by means of the diameter of the intervening circle, the difference becomes still greater.

The following is an attempt to determine this difference quantitatively. Two sets of twelve cards each were made exactly alike. Each card contained two circles, 10 cm. in diameter, and the distances between the circles varied in a series of 1 cm. steps from 5 cm. to 16 cm., inclusive. The cards of one set were placed in a vertical position and those of the other in a horizontal position. They stood upon an easel, at right angles to the line of vision and three meters from the observer. To avoid, as far as possible, the error of succession,[1] they were arranged in the following irregular order (where h denotes horizontal, and v, vertical, and the middle distances are given in centimeters): 16 v, 15 v, 6 h, 5 h, 10 h,

[1] The test began with two vertical figures, and it was not announced beforehand that there would be any horizontal figures. This precaution was taken because the method of estimating that is followed in the first trials is liable to be continued.

9 h, 5 v, 6 v, 9 v, 10 v, 14 h, 13 h, 14 v, 13 v, 7 v, 8 v, 12 h, 11 h, 8 h, 7 h, 12 v, 11 v, 16 h, and 15 h. The cards were 57 cm. long and 36 cm. wide and had a light blue tint. The lines were 1 mm. wide.

This test was made upon twenty-four of the gentlemen who had taken part in the First Series. They were aware of the disturbing influence of the interrupting lines, but they were requested not to make any theoretical allowance for that; e. g., if they surmised that there might be an illusory effect, of say 10 per cent, they should not take that into account but estimate the distances as they actually appeared upon the very closest inspection. These are the particular instructions: Consider the diameter of a circle to consist of ten units. How many such units are there in the distance between the two circles? They had only one trial on each point. The records are contained in Table II.

TABLE II.

Forced Overestimation of the Vertical.

St	*HE*	*d*	*VE*	*d*	%*HI*	%*VI*	%*VI*-%*HI*
5	5.3	0.8	5.7	1.0	6	14	8
6	6.5	0.8	7.0	1.0	8	16	8
7	7.0	0.7	7.8	0.9	0	11	11
8	8.0	0.7	9.0	0.9	0	13	13
9	9.6	1.2	10.7	1.3	7	19	12
10	10.7	1.3	11.8	1.1	7	18	11
11	11.5	0.9	12.6	1.0	5	15	10
12	12.6	0.7	13.1	1.2	5	9	4
13	13.6	1.0	14.6	1.6	4	12	8
14	14.5	1.8	15.9	1.5	4	14	10
15	15.7	1.2	17.6	2.0	5	17	12
16	16.3	1.5	18.7	2.1	2	17	15
			Average		4	15	11

St, standard distance between the circles.
HE, average estimate of the horizontal distance.
VE, average estimate of the vertical distance.
d, mean variation of the records for all the observers.
%*HI*, per cent of overestimation for the horizontal.
%*VI*, per cent of overestimation for the vertical.
The unit of measurement is one centimeter.

The data are not sufficiently numerous to enable us to establish any law for the variation with the length of the middle distance. As the standard distance, 10 cm., is in the middle of the series and there is no regular variation, it may be permissible to apply the average of the illusions for all the distances to that one for convenience in a crude comparison. The horizontal middle distances are overestimated by 4 per cent, on the average; the vertical, by 15 per cent. The difference between these two, 11 per cent, is due to the difference in direction. This is not the ordinary overestimation of the vertical because the whole figure was in a vertical position.

The figure in Plate IV is drawn according to the data obtained in this test. To the persons that were tested this figure should appear perfect in arrangement; i. e., the middle distances should appear to be equal to the diameter of a circle. The horizontal middle distance is 4 per cent shorter than the standard, and the vertical middle distance is 15 per cent shorter than the standard distance. Of course, no two persons will see it alike and much depends upon the relative size of the figure, its distance from the eye of the observer, and the fact that the illusion is pointed out.

In making the comparison for the vertical figure there seems to be a tendency to estimate the horizontal diameter of the circle first and then assume that, as the circle is a perfect figure, this is also the vertical diameter. The middle distance is therefore virtually compared with the horizontal diameter in the perception of which there is perhaps no illusion (See Form 21 above). There is then no overestimation of the supposed vertical diameter of the circle but there is nothing to check the overestimation of the vertical middle distance. This may account for a normal overestimation of the middle distance, but 11 per cent is more than the normal overestimation of the vertical. The excess may be accounted for by the combination of the illusion of the vertical with the illusion of angle-lines, upon the hypothesis that the illusion of the vertical is augmented by increased effort in the estimation. It requires greater effort in estimating the length of the middle

distance in this figure than in estimating the length of a plain vertical line.

The illusion is inhibited in the circles because we know that the circle is a perfect figure. Any other figure which possesses an equal symmetry will produce a similar effect. If squares are substituted for the circles, the diagonals of the squares lying in the same straight line, the effect is even greater than for the circles, on account of the decreased limitation of the middle space.

A visual illusion of distance perhaps occurs wherever lines, real or imaginary, are interrupted by lines forming angles other than right-angles with the base-line, but in most cases it is difficult to determine it on account of the want of a suitable unit of measurement. A simple case is that in which distances appear to stand in the ratio of 1:1, as in a single vertical or horizontal member in Plate IV. It is a favorite type of designs in wall papers, carpets, silks, etc. The section of a design on a wall paper frieze seen in Plate V is an illustration. The horizontal diameter of the incomplete circles in the upper part of the figure is 125 mm. and the distance between the figures is 112 mm. in the original. These distances appear to be equal. One of the most pleasing effects is the ratio of 1:1. This is obtained by making proper allowance for the illusion. To show how common this apparent ratio is, I may mention that I have found, in the same room, more or less complex patterns of this type on the wall papers, the table cloth, and the carpets. The middle spaces were approximately nine-tenths as long as the diameter of the limiting circular figures, but they appeared to be equal.

The same forces that cause the Müller-Lyer illusion upon plane surfaces also influence the perception of geometric forms. To mention a homely illustration, the difference between the height and the diameter of a flour barrel appears to be much greater than it really is. In any cylinder whose length is equal to its diameter the length appears to be decidedly greater than the diameter, especially if the actual proportions are not known. The illusion is still greater if the

PLATE IV.

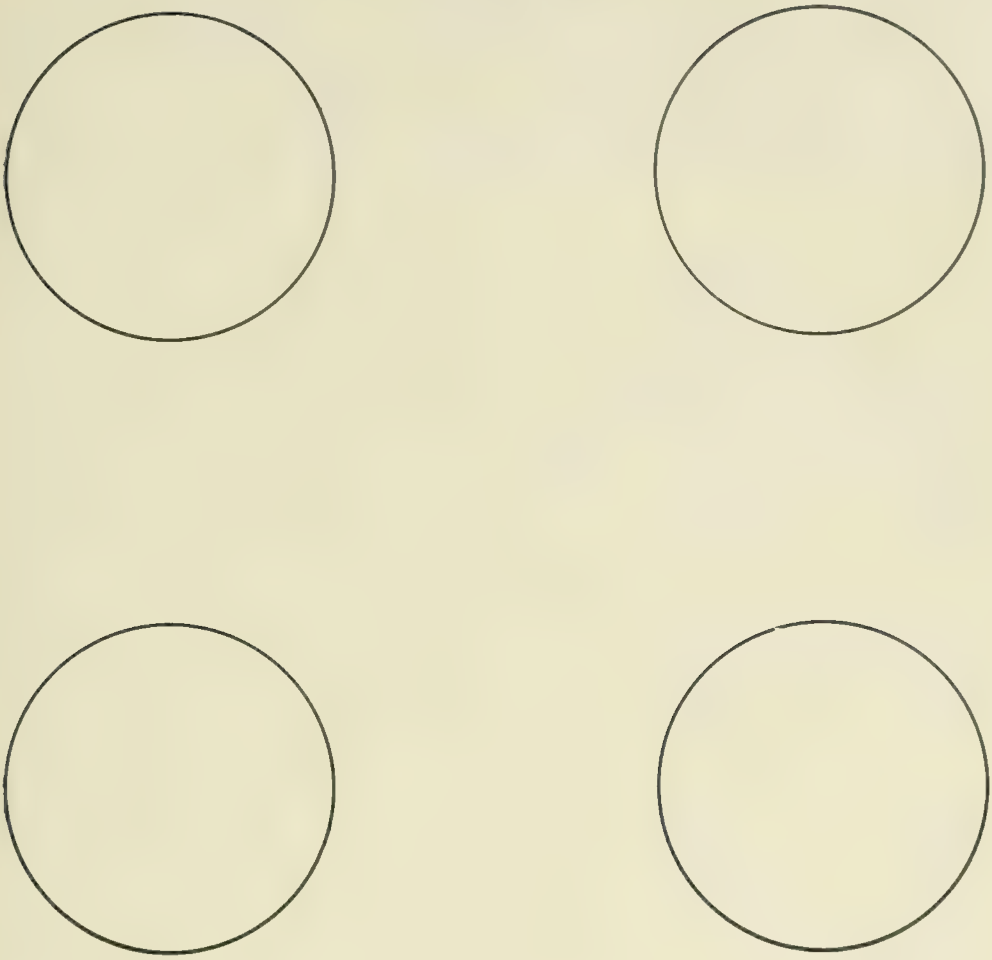

PLATE V.

length is actually greater than the diameter. Much of course depends upon the point of view and combinations with other illusions. The problem is now being investigated. This illusion is an extremely important factor in the æsthetics of geometric forms.

Fourth Series: Variation with Mental Development.

(*These tests were performed by Miss Eva M. White.*)

The following tests were made upon school children of the ages from six to fifteen, inclusive. (See description of method, p. 3, above.) The results are contained in Table III. The headings of the columns in the table refer to the corresponding paragraphs in the following statement.

A. The Müller-Lyer illusion with three silver dollars as in Form 6 of the First Series. The illusion amounts to 31 per cent for girls and 30 per cent for boys. It is 15 per cent for the university students.

B. The conflict of the Müller-Lyer illusion with the illusion of filled space. This is like Form 1 of the First Series (p. 6), but instead of dimes brass disks were here used. The standard member consisted of five disks which, when arranged like the dimes in Form 1, made the standard member 114 mm. The disks were fixed on a wire which also formed a base-line. A sixth disk was adjustable like the fifth dime in Form 1. The illusion amounts to 11 per cent for the girls and 15 per cent for the boys. Only a general comparison of this can be made with the 8 per cent illusion for the university students in Form 1.

C. The Müller-Lyer illusion—the same as the first form described on p. 25. (Base-line 95 mm., angles 30°, angle-lines 30 mm.) The same apparatus was used as in the test with the students. The illusion amounts to 22 per cent for the girls and 21 per cent for the boys. This happens to be exactly the same as was found for the students in that test. The students had more nearly the same conditions as the children in this test than in any other. I consider the comparison much fairer in this case than in the case of Form A.

TABLE III.

Force of the Normal Illusions of Sight in Children.

GIRLS.

Age	*n*	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*E*	*d*
6	10	90	28	107	12	80	14	86	6	141	25
7	9	72	21	94	8	76	7	84	8	148	16
8	10	75	14	103	8	76	4	84	6	157	19
9	9	66	11	98	8	79	8	83	6	177	15
10	9	78	7	99	6	67	3	83	4	180	7
11	10	68	10	104	6	73	5	85	7	180	16
12	10	77	10	102	6	74	10	84	4	178	10
13	10	84	7	104	6	74	10	84	4	178	10
14	10	87	5	103	4	73	9	86	4	183	13
15	7	90	8	106	6	74	6	88	5	189	13
Average		79	12	102	7	74	7	85	6	171	14
Standard		114		114		95		101		202	

BOYS.

Age	*n*	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*E*	*d*
6	9	76	18	89	20	74	9	82	14	147	21
7	10	74	23	95	8	81	13	86	6	161	23
8	9	65	11	97	6	80	5	87	5	145	32
9	7	69	17	89	15	71	5	85	7	171	17
10	6	90	12	96	5	80	6	87	6	187	13
11	10	85	9	99	5	71	6	84	5	177	11
12	10	79	9	96	4	68	5	82	4	180	9
13	8	87	5	106	8	68	7	88	4	184	7
14	9	90	11	100	11	73	6	86	9	190	20
15	6	80	3	97	3	85	12	89	6	191	10
Average		80	11	97	9	75	7	86	7	173	16

n, number of children tested.
d, mean variation.
Unit of measurement, the millimeter.
The forms are designated by the capitals as in the text.

D. The vertical, bisecting-line illusion. A perpendicular 125 mm. long was erected at the middle of a horizontal line 101 mm. long. The test consisted in making the two lines appear equal. The adjustment was made by passing a card down over the upper part of the vertical. The illusion amounts to 18 per cent for the girls and 17 per cent for the boys. This may be compared with the same test made, though with a standard of 114 mm., upon the students (p. 21) in which the illusion averaged 9 per cent. The comparison is not direct because the students had good grounds for supposing that there was an illusion, but the children had not.

E. The double square. The method and apparatus described on p. 19 were employed. The girls make the apparent double square 9 per cent too short, and the boys make it 8 per cent too short. The average for the students is 4½ per cent. Here the comparison between the children and the students is perfectly legitimate. The records for the children clearly confirm the principle that the overestimation of the vertical depends upon the ratio between the horizontal and the vertical. For this illusion there is a decided improvement with age in the children. The improvement of the students over the children is also certain.

The illusion has practically the same force for girls and boys. For the first four forms of the illusion there is no decrease in the illusion with development of the children. The only improvement is in uniformity, which is seen in the mean variation. In comparing the records of the students and the children in Forms A, B, and D, it must be borne in mind that the superiority of the students is not all in greater accuracy of perception, but often in the possession of some sort of advantageous caution against illusion.

These data upon the illusions in children were collated and compared with the classification of the children according to "general mental ability" by the method that is illustrated for hearing-ability in Table VII. The comparison reveals no constant tendency for the illusion to vary with the "brightness" of the children.

II. The Material-weight Illusion.

A few years ago I called attention to the existence of a negative illusion of weight which is due to the appearance of the material in the object that is lifted. The original statement[1] was made upon the basis of a small number of experiments. I have here undertaken to verify those observations and to study the variations in the illusion for age and sex. I suggest the name "material-weight" for this illusion because that coördinates it with the "size-weight" illusion and indicates its nature.

First Series: Illusion Unknown to Observers.

Three blocks were made of wood, iron, and cork, respectively. They were all of the same size, cylindrical, 31 mm. in diameter, and 40 mm. long. They were also of a uniform weight, 55 g. The wood and the cork had been filled and the iron had been made hollow. A standard set of blocks to be used as a means of measurement was made of hard rubber and polished. The standard blocks varied in weight from 15 g. to 90 g., by five-gram steps. But they were of the same size and shape as the other blocks.

The size-weight illusion was introduced in the same series in order to obtain a comparison of the new illusion with this well known illusion. Two blocks like the blocks of the standard series in every respect except in length and weight were used. They were of the same weight, 55 g., but one was 12 mm. long and the other was 140 mm. long. They may be designated as the small and the large.

The blocks were kept behind a screen and were handed to the observer in order. The wooden block was first presented together with the 55 g. block from the standard series and the observer was requested to compare them according to the following written directions.

[1] Stud. Yale Psych. Lab., 1895, III. 18.

"*Test of discriminative sensibility for weight.* Compare the weight of the presented blocks by lifting them two or three times in rapid succession, being careful always to keep the blocks in the same position, grasp them in the same manner, lift them about one inch at a uniform speed, and replace them gently and quickly. State whether they are equal or different in weight and if different state which is the heavier."

If as usual the wood was pronounced heavier, heavier standard blocks were presented in order until the block from the standard series had been pronounced heavier in two consecutive judgments. Then lighter standard blocks were presented in order until they had been pronounced lighter in two consecutive judgments. This method was pursued until four upper and four lower determinations of the region of equality had been made. In the case of a few persons who were very slow in making the comparisons, only two such complete determinations could be made. The same mode of procedure was followed for the iron and the cork blocks. The same method was also followed for the large and the small blocks, except that the 65 g. block was handed first with the small block, and the 45 g. block first with the large block, in order to save time. This method is somewhat arbitrary but it is time-saving and excludes the danger of suggestion from the observer as the order of the blocks depends upon the observer's judgments. It is entirely satisfactory. The trials were made in the double fatigue order. In the statement of the results, the mean between the upper and the lower limits of the estimated weight is taken as the apparent weight of the block that has been tried. The mean variation was obtained by taking the average of the variations of the individual upper and lower limits from the average of their means.

I place the entire table of the results of these measurements on record in order to establish the fact of the existence of the illusion, and show its extent and individual variations. The cork and the wooden blocks are overestimated and the iron block is underestimated. The essential condition of the illu-

sion is that the preliminary estimate of the weight of the object shall be wrong. In this case that is brought about by making the objects appear to be solid and made of materials of different weight. Before lifting an object we normally estimate the approximate weight by sight, and the effort to be exerted in lifting is adjusted semi-automatically upon the

TABLE IV. (*A*). *Men.*

The Material-weight and the Size-weight Illusions When They are Unknown to the Observers.

N	*Wood*	*d*	*Iron*	*d*	*Cork*	*d*	*Small*	*d*	*Large*	*d*
2	55	3	57	5	61	3	70	2	48	0
4	57	1	54	2	59	3	76	4	35	0
6	61	2	49	2	58	0	58	0	45	2
8	63	3	46	2	60	4	69	1	24	6
10	66	1	44	4	62	3	73	2	31	3
12	56	2	55	3	59	1	61	2	48	0
16	59	4	46	4	58	9	71	10	37	2
18	65	2	46	4	61	5	83	0	31	4
20	55	4	55	3	64	2	75	3	38	0
22	63	5	45	5	63	0	68	0	50	3
24	61	1	48	0	65	3	66	1	43	1
26	56	2	50	3	58	2	58	5	53	0
28	58	0	48	0	61	3	73	0	45	3
30	63	0	43	5	68	0	83	0	30	3
32	57	2	58	1	58	2	69	4	44	1
34	58	2	46	3	55	3	66	1	45	6
36	62	1	48	0	58	5	90	0	33	3
38	64	3	47	2	60	1	64	2	41	6
40	57	2	49	2	55	2	80	2	35	2
42	64	0	55	2	68	0	82	7	48	0
44	60	2	50	1	63	2	70	2	54	3
46	63	0	50	2	58	3	73	0	40	2
48	58	3	53	0	59	3	69	2	43	5
50	62	3	49	1	57	2	77	2	43	0
52	53	0	51	1	55	2	78	0	35	0
54	60	2	48	5	60	2	72	0	40	2
56	60	2	49	1	58	1	86	1	30	0
58	60	2	53	2	55	3	62	1	45	5
Average	60	2	50	2	60	3	72	2	41	2

The measurement is in grams. The standard is 55 g.
N, the observers by number.
d, mean variation.

TABLE IV. Continued. (*B*). *Women.*

N	*Wood*	*d*	*Iron*	*d*	*Cork*	*d*	*Small*	*d*	*Large*	*d*
1	56	2	51	4	65	2	70	4	33	3
5	59	2	50	0	63	2	82	4	35	0
7	63	5	51	2	63	4	73	5	38	5
9	64	9	47	3	65	3	79	8	34	2
11	58	0	45	2	62	1	80	2	39	4
17	58	0	49	6	65	2	83	4	30	2
19	55	3	54	1	54	1	70	3	35	4
21	62	4	48	3	64	2	81	2	31	1
23	57	1	51	3	61	1	86	1	30	3
25	39	1	46	3	59	1	78	7	39	3
27	59	3	50	1	59	4	73	3	34	1
29	50	1	53	5	66	3	78	4	36	1
31	59	3	50	4	68	4	83	2	35	0
33	65	8	47	2	57	1	85	4	45	4
35	60	3	51	3	56	5	76	1	33	2
37	58	0	49	6	59	1	83	6	39	3
39	57	3	53	2	64	2	80	0	29	1
Average	59	3	49	3	62	2	79	4	35	2

basis of this preliminary estimate. If insufficient effort is put forth at the beginning of the lifting the weight of the object will be overestimated. If too great effort is put forth the weight of the object will be underestimated.

It is not necessary to state the illusion in relative terms of the standard series. As this is a common scale having the same unit of measurement as the illusion blocks, it may be eliminated and the results may be expressed as an absolute difference in the apparent weight of two illusion blocks. This has been demonstrated for the size-weight illusion[1] and the principle of the measurement is the same. Thus, the average estimate by the men indicates that the cork and the wooden blocks each seem to weigh 10 g. more than the iron block although they are actually the same weight. The illusion is 18 per cent of the actual weight. The women estimate the difference between the wooden and the iron blocks the same as the men, but they estimate the difference between the cork and the

[1] SEASHORE, *Weber's Law in Illusions*, Stud. Yale Psych. Lab., 1896, IV, 62.

iron blocks to be 13 g. which is 24 per cent of the total weight.

These results may be compared with the results for the size-weight illusion which are contained in the same table. The comparison is necessarily relative to these particular sizes, materials, etc. In a general way it is apparent that the material-weight illusion is not as strong as the size-weight illusion, but practically as constant.

The illusion is not limited to these particular artificial conditions. It occurs whenever the appearance of the material which constitutes the object leads us to think that the object is heavier or lighter than it really is. The erroneous estimate may be due to false appearance of the material, erroneous associations of weight, or failure to acquire a true conception of the normal weight of the material. Thus, if a breakfast roll is heavier than it appears to be when looked at, we judge it to be still heavier than it actually is when we lift it. The weight of a metal tube is ordinarily underestimated, not only because it is larger than a solid piece but also because it is associated with a solid rod of the same material and dimensions. The weight of aluminium is generally underestimated and the weight of mercury is generally overestimated because we are very slow in learning that these are actually so different from other metals. It is important to notice that no one thought that the blocks in this test were actually solid, of a uniform material. It is at once apparent that the cork block is too heavy to be all cork and the iron block too light to be solid iron. After the first trial the observers knew what to expect but the illusion did not disappear. This shows that the illusion rests upon a subliminal or automatic process which tends to continue when once established, despite the opposing knowledge.

The best direct demonstration of the cause of the illusion and its persistence is obtained by lifting the blocks gently and observing that although the cork has its natural roughness and the iron has a polished surface the cork generally tends to slip, as the iron does not. Although we know all about the conditions, we automatically grasp the iron with greater force than we grasp the cork.

This test was made before the subject had been discussed in the class. A few of the observers may have been aware of the illusion and it is possible that they may have made conscious or unconscious corrections. Upon inquiry it was found that no one knew the exact extent of the illusion and only a small per cent knew anything about its nature. The illusion is therefore nearly the maximum for these particular conditions.

Second Series: Illusion Known to the Observers.

(These tests were made by Miss Anna Kierulff.)

The same test was made upon the other class, with this variation that the illusion was demonstrated and fully discussed in the class before the test was made. The following was added to the above written directions: "Do not guess or make any allowance for possible errors due to the difference in the materials of the blocks." The method described above was employed.

In addition to the first three blocks a lead block similar to the iron block was also included, the object being to determine whether the illusion would be greater for lead than for iron, as it tends to be greater for cork than for wood. In this respect the test cannot be said to be successful because it was difficult to notice any difference between the lead and the iron blocks. The fact that there is only a small difference between the illusion for the wood and the cork may be taken to indicate that for many the maximum illusion is reached with wood and that additional increase in the illusory appearance fails to increase the illusion.

As the records are of the same nature as those contained in Table IV, it will suffice to quote the final averages with the corresponding averages of the mean variations, which may be designated by ad. For the twenty-nine men the records run thus: wood, 57 g., ad., 3 g.; iron, 53 g., ad., 3 g.; lead, 53 g., ad., 3 g.; and cork, 59 g., ad., 3 g. For the twenty-six women: wood, 56 g., ad., 3 g.; iron, 53 g., ad., 2 g.; lead, 53 g., ad., 2 g.; and cork, 58 g., ad., 3 g. The illusion persists even when understood in detail but is not so strong as when unknown.

After the regular trials the observers were requested to arrange the blocks in the order of their apparent weight, knowing that they were equal. Ten declared that the four blocks appeared equal. That such assertions are vague and uncertain is demonstrated by the fact that four of these ten showed more than the average illusion according to the regular measurement. The judgments of those that noticed apparent differences are exhibited in the following tabular form where the Roman numerals indicate the order of weight, beginning with the heaviest.

	I	*II*	*III*	*IV*
Cork	34	8	3	0
Wood	8	28	8	1
Iron	2	4	17	22
Lead	1	5	17	22

Third Series: Variation with Mental Development.

(These tests were made by Mr. I. I. Dalbey.)

Similar tests were made with the same apparatus upon the school children (see p. 3). The method was necessarily simpler and less reliable. The blocks of the standard series were arranged in a line on the table in order of weight. The children were first required to make sure that they understood this order. Then they were required to match each of the illusion blocks in weight with a block in the standard series by comparing them under the most favorable conditions. The results are given for the different ages in Table V. As there is no great difference between the girls' and the boys' records they are stated together in the body of the table and only the averages are given separately.

There is a constant tendency to select a block that is too light. This may be accounted for by the circumstance that although the children were allowed to proceed up and down the series at pleasure there was a tendency to come to a decision oftener and more readily when passing in the direction of the heavier blocks. In such cases the records may represent one limit of the region of equality instead of the middle or average. In the two foregoing series of measurements it

has been demonstrated that the cork and the wood are overestimated and the iron and the lead are underestimated, and in the study of the size-weight illusion it was shown that the results may be stated in terms of the illusion blocks instead of in the terms of the standard series. The result may therefore be stated in terms of the apparent difference between two illusion blocks. The average apparent difference between the cork and the lead block is thus 11 g., or 20 per cent of the actual weight; between the cork and the iron, the same; and between the wood and the iron or lead, 7 g., or 13

TABLE V.

The Material-weight Illusion for Children.

Age	*n*	*Wood*	*d*	*Iron*	*d*	*Lead*	*d*	*Cork*	*d*
6	19	56	10	48	7	46	10	60	13
7	19	53	9	44	10	41	8	49	8
8	19	55	6	49	7	51	7	59	8
9	16	54	5	48	5	49	7	60	6
10	18	53	3	46	5	47	8	57	7
11	20	56	6	48	4	44	5	61	5
12	20	54	3	47	7	49	7	60	6
13	18	55	3	44	6	46	4	57	7
14	19	55	4	48	5	48	6	60	5
15	13	52	4	45	6	49	4	61	7
Average		54	5	47	6	47	7	58	7
Boys		52	5	45	6	47	7	58	8
Girls		56	6	48	7	47	6	59	6

n, the number of children for each age.
d, mean variation.

per cent of the actual weight. This is virtually what was found for the university students. This illusion does not vary with age. On the whole there is no noticeable variation with sex among the children.

The children were classified according to the degree of illusion that they showed, and this classification was collated with the classification according to "general mental ability" by the method illustrated in Table IX for hearing-ability. The comparison revealed no functional relation between "brightness" and suggestibility in this particular case.

The small variation with sex that we find is significant. These results are respectfully submitted for comparison with those published by Dr. Wolfe,[1] in which he finds the size-weight illusion twice as strong for women as for men. The material-weight illusion is too mild to form a good basis for the discussion of this. The question can best be discussed on the basis of the size-weight illusion. The data in Table IV represent as fairly as we can determine the relative susceptibility of men and women to this illusion, when a careful judgment is made under the most uniform conditions.

The Transition from Negative to Positive Illusions of Weight.

The material-weight illusion here discussed is a negative illusion because it is contrary to the suggestion. The commonest illusions in all senses are positive; we realize what we expect or what is suggested. By analogy we may infer that the material-weight suggestion may also produce positive illusions. The negative illusion is produced because the suggestion is too violent to be accepted. The very reaction of the mind against the violent suggestion intensifies the attention to the sensory signs of effort which are misleading. But if the suggestion is made mild and reasonable so that it does not surprise the observer, it will be the determining element in the perception and the sensory elements will be relatively overlooked. If this hypothesis is true we may expect to find an uninterrupted gradation from the positive into various degrees of the negative illusion. Some preliminary trials on this were made upon the children in connection with the above test.

Two Florence flasks were filled, one with unground roasted coffee and the other with common white beans. Some lead was introduced into the center of the coffee and some cotton into the beans. Neither cotton nor lead appeared at the surface. The two flasks were thus reduced to a mean weight of 63 g. A

[1] Wolfe, *The Effects of Size on Judgments of Weight*, Psych. Rev., 1898, V. 26.

similar but larger pair of flasks were filled in the same way and reduced to a mean weight of 244 g. The children were asked to compare them, by lifting, and tell which was the heavier. The experiment is based upon the general knowledge that beans are heavier than coffee. The judgments ran as follows for the small flasks: coffee heavier, 86; beans heavier, 89; and, "no difference perceived," 10. For the larger flasks the result was as follows: coffee heavier, 119; beans heavier, 59; and, "no difference perceived," 9. Thus, there is no illusion for the small flasks and for the large ones there is a balance in favor of the negative illusion.

Two cylinders were turned out of pine, 30 mm. in diameter, one 30 cm. long and the other 29 cm. long. They were reduced to a mean weight of 83 g. It was supposed that the clearly perceptible difference in size would act as a suggestion and that the illusion would be positive because the suggested difference was so small that sensory evidence to the contrary would not be detected. The children judged them as follows: long heavier, 61; short heavier, 110; and, "no difference perceived," 10. The conditions are not simple, but the negative illusion still obtains.

The "Color-weight" Illusion.

It is generally thought that dark colored objects appear to be heavier than light colored objects. If such an illusion exists it would be involved in the coffee and bean tests. A test for this was included in the Third Series. Two blocks were made of the same size and shape as the standard rubber blocks. One was painted white and the other black. They were weighted to 55 g. What material they were made of could not be seen. The children matched them in the standard series in the same way that they matched the blocks of different material. The results show no illusion.

Energy Economized by the Illusion.

In this connection it may be of interest to mention a new problem in the study of illusions of weight. While studying

the above illusion, Miss Kierulff suggested that it might be possible to increase the efficiency of the muscular effort by means of the illusion. So long as the lifter is not exerting the maximum effort, there is no doubt but what he lifts the object which seems lighter with greater ease than the one which seems heavier. What would be the effect of the illusion upon the fatigue if a man would lift a ten-pound object fifty times in succession, being fully convinced that it did not seem to weigh more than five pounds? Many conditions must be considered in answering that question, and we have no experiments, but it is probable that with a moderate weight like this for a strong man he would feel at the end of the trial more as if he had lifted five pounds than ten pounds.

But can the maximum effort be increased by the illusion? Theoretically there should be no illusion in the maximum effort. We have arranged an experimental test but have only carried it far enough to find that there are several interesting problems involved. The maximum lifting ability is tested with objects in which the conditions of the size-weight illusion are present. A flour barrel and a half-peck measure are used. Nearly all who have tried it can lift more in the barrel than in the half-peck measure. Even when the maximum weight is lifted, there is a tendency to judge the barrel to be lighter and to try to account for the failure to lift more by fatigue or unequal distribution of effort.

III. Localization of Sound in the Median Plane.

(These tests were made with the assistance of Miss Mabel Williams.)

The general problem was to determine some of the constant tendencies in the localization of sound.

The sound was produced by a 100 v. d. electric fork in a distant room. The fork interrupted the primary circuit of an

induction coil in whose secondary circuit three telephone receivers were inserted. One receiver (R) was placed seven feet to the right of the observer's head and another (L) seven feet to the left, both in the aural axis. The third receiver (C) was placed two feet vertically overhead. Two different intensities of the sound were used for each receiver. The fainter was just clearly perceptible, and the stronger was such that the normal ear could detect it at a distance of about one hundred feet. In the following the strong sounds are designated by capitals and the weak by small letters. The trials were always made in the following order: R, L, C, c, rl (together), RL, RL, rl, c, C, L, R, RL, rl.

Thirty-nine students of psychology whose hearing-ability had been previously measured, were experimented upon. There were twenty-five men and fourteen women. They were blindfolded before entering the experiment room and told that the position of the experimenter and the shape of the room did not determine the location of the sounds. The experimenter turned on all the sounds noiselessly, remaining seated in the same position. The students were asked to determine the distance in feet and the angular direction by degrees in the horizontal and the vertical planes. The distance was required merely as an aid in the angular localization. The variation in intensity was introduced in order to call forth renewed effort in each trial. As the influence of variation in intensity is discussed in the sequel it may be neglected in the present qualitative statement.

We may consider the results with reference to three sources of sound:

(1) The lateral sounds—those that originate in R and L acting singly;

(2) The fused median sounds—those that originate in RL and rl, i. e., the resultant of two symmetrically located, simultaneous sounds; and,

(3) The single median sounds—those that originated in C and c.

Lateral sounds can be localized with reference to two planes.

Median sounds can be localized with certainty in one plane only. It is well known that if two sounds in the positions of R and L are sounded simultaneously they are perceived as one and this resultant is located in the median plane.

1. *Is there any constant tendency to localize a median sound in any particular section of that plane?*

(a) The single median sounds.

Twenty-two persons always locate these sounds in front of a vertical plane through the aural axis, three always locate them back of this plane, and fourteen vary.

Of all these sounds, 77 per cent are located in front of the vertical plane, 20 per cent back of it, and 3 per cent in it.

Twenty-two persons always locate these sounds above the horizontal plane through the aural axis, one always below, and sixteen vary.

The same sounds are located, 78 per cent above the horizontal plane, 11 per cent below, and 11 per cent in it.

If the median plane be divided into four quadrants, by two lines that cross the vertical diameter at an angle of 45°, the same records will be distributed as follows: 52 per cent in the front quadrant, 38 per cent in the upper quadrant, 10 per cent in the back quadrant, and none in the lower quadrant.

Therefore, there is a decided tendency to locate the single median sound, that is produced directly overhead, above and in front of the ears, i. e., upward and forward.

(b) The fused median sound.

Three persons locate all of these in front of the vertical plane, eight persons locate all behind this plane, and twenty-eight vary.

Of all the fused median sounds, 25 per cent are located in front of the vertical plane, 73 per cent back of it and 3 per cent in it.

Eighteen persons locate all these sounds above the horizontal plane, one locates all in it, and twenty vary.

Of all these sounds, 72 per cent are located above the horizontal plane, 12 per cent below, and 16 per cent in it.

Thirty-nine per cent are located in the front quadrant, 21

per cent in the upper quadrant, 39 per cent in the back quadrant, and 1 per cent in the lower quadrant.

Therefore, there is a tendency to place the fused median sound, that is produced in the binaural axis, above the ears. This tendency is virtually as strong in this case as in the case of the single median sound that actually came from above.

If we consider the front and the back quadrants, we find no tendency in the fused median sound to favor either one of these. But if the vertical line through the head be considered the dividing line, there will appear to be a decided tendency to place the fused median sounds back of this.

2. *Is the median sound localized on the side of the stronger ear?*

3. *Is the lateral localization of a fused median sound as definite as the lateral localization of a single median sound?*

(a) The single median sounds.

Three groups of ten persons each were formed upon the basis of the comparative keenness of the two ears.[1] Group 1 comprises the ten who have the greatest difference in the acuteness of the two ears, Group 2 those whose ears show the next greatest difference in acuteness of hearing, and Group 3 those whose ears have nearest the same degree of acuteness.

Group 1 located 57 per cent of these sounds in the median plane, 38 per cent on the side of the stronger ear, and 5 per cent on the side of the weaker ear.

Group 2 located 45 per cent of these sounds in the median plane, 33 per cent on the side of the stronger ear, and 22 per cent on the side of the weaker ear.

Group 3 located 58 per cent in the median plane, 20 per cent to the left, and 22 per cent to the right of this plane.

Therefore, there is a tendency to locate the single median sound on the side of the stronger ear.

(b) The fused median sounds.

Group 1 located 36 per cent of the fused median sounds in

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[1] For test of the comparative acuteness of hearing in the two ears, see Section IV, p. 55.

the median plane, 32 per cent on the side of the stronger ear, and 32 per cent on the side of the weaker ear.

Group 2 located 19 per cent in the median plane, 41 per cent on the side of the stronger ear, and 40 per cent on the side of the weaker ear.

Group 3 located 45 cent in the median plane, 22 per cent on the right, and 33 per cent on the left side of that plane.

Therefore, in the case of the fused median sound, a correction is made for the difference of the two ears in acuteness ot hearing, or else this sound is not located laterally with sufficient definiteness to reveal small variations. It is probable that the latter is the case because the angular variation of the fused median sounds is much greater than for the single median sounds. This could of course be given quantitatively, but a qualitative statement is sufficient for the present purpose. The lateral localization of a fused median sound is less definite than the lateral localization of a single median sound.

4. *Is there any constant tendency in the misplacement oj a lateral sound?*

This is determined with reference to the sounds R and L.

Six persons located all the lateral sounds above, and four located all in front of the true source. No one located all the lateral sounds back of the true source or below it.

Fifty-two per cent of the lateral sounds were located above, 23 per cent below, and 25 per cent in the true plane.

Fifty-four per cent were located in front, 31 per cent back of, and 15 per cent in the true plane.

Therefore there is a tendency to locate sounds, that lie in the aural axis, above and in front of the axis. This is like the tendency for the single median sound, but it is not quite so strong.

It is well known that it is difficult to localize sounds at all. As all the observers were untrained, there was much uncertainty exhibited in these experiments, and the results are very fluctuating. It will, however, be seen that I have based no conclusion upon any small variations.

5. *Can the unpracticed observer localize the single median*

sound radially at all? And what are some of the constant tendencies in this effort?

This problem was taken up in a separate series of tests. Two telephone receivers were placed seven feet apart, eighteen inches above the floor. Another receiver was placed vertically over each of these. All four receivers pointed toward the center of the square thus formed. The observer was seated on a high office stool in such a position that all the receivers were in the median plane of his head, and the center of the square formed by the receivers fell at the center of his head. The sound was produced as before and all the switches were manipulated noiselessly from one position. The quality of the tone was not exactly the same in all the receivers, but the difference was so small that few of the observers could detect it. The intensity was varied systematically between relatively weak and strong sounds. The strong sounds were of the same intensity as in the preceding test, and the other sounds were made so much weaker that the difference was clearly discernible. The test was divided into four parts. In each part two weak and two strong sounds were given through each receiver. The sounds were arranged in such sequence with reference to intensity and direction, that the observer could not guess it or get any help from it. The same order was followed in all the parts and for all the observers.

In the first two parts of the test, the observer remained blindfolded and did not know the number of receivers nor the location of any, except that they were in the median plane. He was placed on a high stool so that he would know that there was ample space for instruments below as well as above. The conditions were the same for the first two parts except that the observer turned around on the stool and faced in the opposite direction in the second part. This was done to eliminate disturbing associations that might accumulate with one position. The last two parts of the test were similar to the first in every respect except that the observer was here shown the location of the receivers and was permitted to keep his eyes open.

This test was made upon twenty-four of the gentlemen who had taken part in the previous test, the results of which had been explained to them. They were blindfolded before entering the room and told that all the sounds would be produced in the median plane, that there would be more sources of sound than in the previous test, that they should not judge by differences in intensity, and that they need only tell which of eight points the sound appeared to be nearest to. These eight points were designated as the four cardinal points, front, down, back and up, and points radially midway between these, counting from the center of the head.

Table VI.

Localization of Sounds.

	I		II		III		IV	
	st	*wk*	*st*	*wk*	*st*	*wk*	*st*	*wk*
Up front	37	20	29	26	43	28	43	26
Front	16	8	15	15				
Down front	6	6	4	6	23	22	25	26
Down	2	3	2	2				
Down back	4	10	4	8	9	26	10	23
Back	4	17	11	15				
Up back	15	19	17	17	25	24	22	25
Up	16	17	18	11				

The Roman numerals designate the four parts of the test.
st, strong; *wk*, weak.
The figures in the table give the average per cent of sounds located at the points named. There were twenty-four observers. Each observer had sixty-four trials, distributed equally among the four parts, among the four sources of sound in each part, and between the weak and the strong sounds.

(1.) *Degree of success in localizing the sounds.* In the first two parts of the test, where the observers had no knowledge of the number or the direction of the sources of sound, 12½ per cent of the sounds would be located correctly by chance. These observers get 22 per cent of the strong and 18 per cent of the weak sounds right. The mean residual is 7½ per cent.

In the last two parts of the test, where the observers knew the positions of the four receivers and kept their eyes open, 25 per cent of the sounds would be located correctly by chance.

These observers get 33 per cent of the strong and 33 per cent of the weak sounds right. The residual is 8 per cent.

No observer showed any *special* ability in localizing the sound. There is a remarkable uniformity and the average represents the individual records well.

According to these figures more success is obtained than can be accounted for strictly by chance. But the data are not sufficiently numerous to demand exact conformity to the law of chance. Grant, however, that the residual represents some degree of success and is an approximate measure of it, this small fraction does not correspond at all to the degree of success that most of the observers feel themselves capable of. Most of the observers thought that they had been right very much oftener than this indicates. This does not apply to all, because a few seemed to think that they had to guess all the time. In trying this test myself, I indicated the degree of sureness that I felt by corresponding intensities in the voice when pronouncing the decision. I was about as liable to be wrong when I gave the strongest evidence of sureness as when I gave evidence of uncertainty. The feeling of ability to locate median sounds, that the unpracticed observer has, is almost entirely an illusion. In ordinary experience we have a sort of feeling that somehow we locate sounds that fall in the median plane by direct hearing, while in reality the localization is a matter of inference based upon other experience. Students always show great surprise when the uncertainty of this localization is demonstrated before them in class.

We tried the same test upon a blind man, who walks around in the city and country without any guide. He located no more sounds right than chance requires.

The degree of success is greater when the observers are blindfolded than when they see, practice notwithstanding to the contrary.

(2.) *Constant tendencies.* Some of these may be seen in Table VI. In parts I and II the tendency to misplace the sound upward obtains to about the same extent as in the foregoing test. There is also a tendency, though not so strong,

to misplace the sounds forward. This is like the tendency for all single sounds in the foregoing test.

In ordinary experience we hear more sounds from the region above the horizontal plane through the ears, for the reason that this space is larger and contains more sources of sound than the lower region. We also hear more sounds from objects that we pay attention to, i.e. face, than from objects that we do not attend to, i.e. those behind us. Hence we normally expect more sounds to come from "up front" than from any other direction. Such a tendency ought to reveal itself in the present test. Expectant attention would be focused semi-automatically in this direction, and since there is no clear direct sensation of direction, there should appear a tendency to realize the expectation or to perceive as we habitually perceive. The test was planned to throw some light upon this theory. In Parts III and IV we should expect the constant tendency to decrease because the observers know the positions of the receivers and naturally expect a fair distribution of the sounds.

The results support the theory. The tendency to misplace the sounds in the direction of "up front" is considerably smaller when the positions of the receivers are known than when they are not known. In making a comparison of the figures in Parts I and II with those in Parts III and IV, the average of the weak and the strong sounds must be taken because the weak and the strong sounds followed in the same series without any order apparent to the observer, and inspection of the figures shows that the localization depends upon the differences in intensity to some extent.

In the statement of the degree of success, it has been seen that the success is practically equal for the weak and the strong sounds. That the localization was influenced by the variation in intensity is clearly indicated by the figures which show that there is a tendency to place the weak sounds behind and the strong sounds in front of the observer. The influence of intensity was, therefore, not successfully eliminated and there is nothing to show that the partial success is not due to judgment upon variations in intensity.

IV. Hearing-ability and Discriminative Sensibility for Pitch.

The tests on hearing-ability and the tests on discriminative sensibility for pitch were made upon the same persons and at the same time, and are here reported together for convenience in determining whether any functional relation exists between the two processes.

Hearing-ability of Students.

The original model of the audiometer that is described in a special notice in this volume was employed. (See notice under New Psychological Apparatus.) It was essentially like the one that is described. Slight changes have been made in the standard of intensity and the division of the scale of intensities, therefore no exact comparison can be made between the results obtained with the original model and those obtained with the final model. In this report the intensity of the stimulus is expressed directly in terms of the number of coils in the secondary circuit, that were required to produce the sound. The smaller the number, the more acute is the hearing.

The upper and the lower limits of the threshold of hearing were determined by five trials for each point and the mean of the averages for these is recorded as the threshold of hearing. Both ears were tested in the same way and all the trials were made in the double fatigue order. The stimulus, a double click in the telephone receiver, was always preceded by the usual warning and a sufficient number of control trials were introduced to eliminate the error of fallacious perception. As we had no quiet-room, some of the variations may be due to distracting sounds in the environment. The records for the students that were tested are contained in Table VII.

The first noticeable feature is the great variation in hearing-ability among normal individuals. The difference in the

TABLE VII.

Hearing-ability and Discriminative Sensibility for Pitch.

MEN.

N	R	d	L	d	P
2	23	1	18	6	12
4	27	1	25	1	12
6	48	7	37	5	12
8	48	11	29	6	12
10	73	3	145	4	30
12	39	6	74	14	30+
14	31	12	43	6	8
16	36	5	68	3	23
18	34	5	21	2	8
20	45	10	50	6	5
22	36	7	50	5	8
24	36	7	56	17	5
26	71	7	37	6	17
28	22	2	40	12	5
30	†				5
32	38	4	28	2	8
34	20	6	22	4	8
36	18	8	16	3	17
38	25	6	18	3	30+
40	†				23
42	56	10	71	4	30+
44	28	6	86	16	3
46	58	4	54	4	12
48	59	13	42	6	3
50	39	8	32	4	8
52	19	6	49	6	5
54	42	10	73	9	5
56	39	9	27	5	30+
58	58	8	38	3	3
	—	—	—	—	—
Average	39	7	46	5	13

WOMEN.

N	R	d	L	d	P
1	32	6	27	2	8
3	30	9	20	4	8
5	35	8	32	4	8
7	55	9	58	5	3
9	35	6	24	9	8
11	55	7	74	6	12
13	61	8	55	6	23
15	24	6	43	4	5
17	105	9	*		3
19	18	2	14	3	5
21	26	6	33	7	8
23	43	11	49	17	8
27	48	8	87	8	8
29	82	8	69	12	5
31	24	4	33	3	5
33	50	9	38	6	8
35	19	3	16	4	2
37	15	3	24	6	12
39	17	3	17	3	30+
	—	—	—	—	—
Average	41	7	40	6	9

N, the observer's number.

R, hearing, right ear; *L*, hearing, left ear; *d*, mean variation.

Unit of measurement for the sound, the energy of one coil in the induced circuit of the audiometer.

P, the least perceptible difference in pitch. (See p. 59.)

Unit of measurement for pitch, 1 v. d.

*Deaf in the left ear.

†Missed test on hearing-ability.

The + signifies that those observers were not tone deaf, but could not perceive differences of 30 v. d.

keenness of the two ears is also conspicuous. Few of the students were aware of the existence of any such difference. The hearing-ability of the men and the women seems to be about equal. These experiments are only preliminary. They have served, at least, to demonstrate the successful operation of a new, convenient, and exact instrument—the audiometer.

Hearing-ability of Children.

The same apparatus and method[1] was employed as with the students; but only from two to four records were made for each limit of the threshold. Sufficient practice was given to

TABLE VIII.

Hearing-ability of Children.

Age	*A*	*B*	*C*	*D*	*E*
8	0	2	6	5	7
9	0	3	5	5	1
10	0	3	7	2	1
11	3	4	10	3	1
12	0	6	10	3	0
13	5	8	5	0	0
14	7	8	4	0	0
15	4	6	3	0	0

The figures give the number of cases that come in each group as specified in the text.

enable the children to understand the nature of the test and recognize the sound. The records of the six and the seven year old children have been discarded on account of the presence of a source of error in the measurement. The results may best be set forth by dividing them into five grades on the basis of keenness in hearing, as follow: Grade A, 1-25; Grade B, 26-50; Grade C, 51-100; Grade D, 101-200; and Grade E, 201-500. The numerals express the intensity of the lowest audible sound as in the foregoing section. The grading is made for the best ear. Table VIII contains the results distributed according to age.

[1] For the general conditions of these tests upon the children, see p. 3.

The records of the boys and the girls are grouped together because there is no noticeable variation with sex. The table shows that hearing-ability improves with age up to the age of 12. Acuteness of hearing depends partly upon general mental ability as shown in ability to comprehend the conditions of the test and concentrate attention upon the particular stimulus, but the test was made so simple and brief that this factor was reduced to a minimum. The variation here shown is therefore probably due chiefly to the development of the sense organ.

The individual differences and the differences between the two ears for the children are as great as for the students. Very few children were aware of the existence of any difference in the keenness of the two ears.

Do bright children hear better than dull children? Before answering this question we must explain the use of the term "general mental ability" in this connection. The children are divided into five grades according to the data furnished by the teachers. The mean per cent of the class standing and the "teachers' estimate of ability" is used as a basis for this division and is here called "general mental ability," for convenience. The designation is open to valid criticism, but may here serve provisionally as a connotation of what is popularly called brightness and dullness.

In Table IX the classification according to general mental ability, as defined, is collated with the classification according to hearing-ability. The same designations, A, B, C, D, and E, are used in both cases. In case of an affirmative answer to the above question, the children of Grade A in general mental ability should come in the higher grades (A or B) for hearing-ability. They do not. The figures in parenthesis show the probable distribution of the cases if there were no connection between hearing-ability and general mental ability. The records follow this distribution pretty closely. Therefore there is here no indication that the bright children hear better than the dull children. There may be cases of children who are dull or are counted dull because they do not hear

well, but such cases are not common enough to be revealed clearly by our method although there may be some indication of them, as in the last figures of Table IX.

TABLE IX.

Comparison of Hearing-ability and General Mental Ability.

Hearing-ability.	*General Mental Ability.* *A*	*B*	*C*	*D*	*E*
A	5 (4)	3 (6)	7 (6)	4 (3)	0 (2)
B	5 (7)	8 (11)	14 (10)	9 (6)	2 (3)
C	11 (9)	18 (13)	12 (13)	3 (7)	1 (3)
D	3 (4)	7 (5)	3 (5)	1 (2)	4 (1)
E	2 (2)	2 (3)	2 (3)	2 (1)	2 (0)

Students' Discriminative Sensibility for Pitch.

(The Measurements were made by Miss Mabel Williams.)

A simple and satisfactory apparatus for this purpose was obtained by tuning a set of forks for increments in height of pitch above international A, 435 v. d., as follows: ½, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v. d. At this point 1 v. d. is equivalent to 1-54 of a tone. The forks are of the best quality and of uniform shape and size—11.5 cm. long. Precautions were taken to keep the temperature constant as that is the greatest source of error. In making the test the standard fork and one of the differential forks were sounded in rapid succession by striking them uniformly and holding them up close to the ear. Each tone was sounded about three seconds, and an interval of about three seconds was allowed between the two tones. The observer was required to state whether the second of the two tones was higher or lower than the first. No other choice of answer was given. The test began with the largest interval, and only one trial was made for each step until the region of uncertainty had been reached. Here ten trials were made for each interval, and the lowest interval for which eight of the ten answers are correct is considered the threshold of discriminative sensibility. This is recorded in the column headed P in Table VII.

The records show the ability of the individual students. The best observer recognizes a difference of two fifty-fourths of a tone, while five are unable to perceive a difference of thirty fifty-fourths of a tone. The superiority of the women's records over the men's may be accounted for by the difference in musical education. There is no marked functional relation between a keen sense of hearing and discriminative sensibility for pitch.

Ability to detect difference in the pitch of tones is a fundamental factor in the appreciation or execution of music. This fact would be better known if all people who pretend to appreciate music had as good power of introspection and were as honest as one of the men tested, who asserted that he appreciated the music of the snare drum as much as any other form of music. The measurement of this ability has not only psychological value but is of pedagogical interest as well. Those who by natural defect of the ear cannot perceive differences between tones cannot appreciate harmony and melody based upon such differences.

There is a peculiar illusion in the discrimination for pitch. Even the smallest intervals appear, to some persons, to be perceptible and recognizable, although they may be below the threshold of discriminative sensibility. The writer experimented with the increment of ½ v. d., having tested the forks with great accuracy. This interval is below his threshold, and yet it appeared to be perceptible in varying degrees. This may be a common illusion among musicians. It seems to be due to the manner of directing the attention in the effort to determine whether the second tone is nearer to a higher or a lower tone that is clearly distinguished from the first and is used as a standard of reference.

Children's Discriminative Sensibility for Pitch.

(*This test was made by Miss Della Northey.*)

The apparatus and method employed were here essentially the same as those described in the foregoing section. There were two minor differences in the size of the intervals and the

tests were necessarily less complete.[1] The records are divided into five grades according to the magnitude of the least perceptible difference in pitch, which is expressed in terms of the number of vibrations per second, as follows: Grade A, 1–2; Grade B, 3–5; Grade C, 6–10; Grade D, 12–30; and Grade E, those who could not perceive a difference of 30 v. d. For those in Grade E, qualitative tests were made with a chromatic pitch pipe to determine whether there was any case of total tone deafness; no such case was found. Sufficient preliminary

TABLE X.

Children's Discriminative Sensibility for Pitch.

Age	*A*	*B*	*C*	*D*	*E*
6	0	0	6	7	3
7	0	0	6	3	5
8	1	1	9	6	2
9	1	4	0	4	3
10	3	6	3	0	1
11	4	13	4	0	0
12	3	14	3	0	0
13	4	12	4	0	0
14	3	8	7	0	0
15	1	5	6	1	0

The figures give the number of cases that came in each group as specified in the text.

practice was given to teach the child the distinction between higher and lower in pitch. Where this could not be accomplished within the limited time, no record was taken. Still I think that the records for the ages of 6–9 are vitiated by the failure to convey to the child an adequate conception of the nature of the test, within the period of time that was allowed. These records are however included because they show what could be accomplished.

[1]In the tests upon the children the forks were rested upon the table instead of being held up to the ear. We have determined by a special series of tests that, for continuous experiments, this introduces a source of error. The discrimination is much easier if the fork is rested on a sounding board than if it is held in the hand near the ear. This is due to the help that is obtained from differences in timber that are intensified by the sounding board. The tone is purest when the fork is held in the hand.

The record for the boys and the girls are given together in Table X because there is no noticeable variation with sex. For the reason stated, we are not justified in concluding on the basis of these records that there is such development with age as the records for the first four ages may indicate. The reliable records begin with age ten. From ten to fifteen, inclusive, there is no sign of improvement with age in the discriminative sensibility for pitch. The average for all the children for these ages is practically the same as the average for the university women. The answers obtained upon inquiry in regard to the musical training of the children show emphatically that the individual differences are not due principally to training. Many of those who have a high grade of tone sensibility have had no musical education, and the reverse was also found to be true. A child of eight never failed to perceive a difference of two fifty-fourths of a tone. Only a small per cent of adults could reach that limit, even with the

TABLE XI.

Comparison of Hearing ability and Discriminative Sensibility for Pitch.

Discriminative Sensibility for Pitch.	*Hearing-ability.* *A*	*B*	*C*	*D*	*E*
A	2 (3)	4 (6)	12 (7)	1 (2)	0 (1)
B	11 (8)	22 (19)	22 (24)	7 (7)	0 (4)
C	6 (5)	10 (10)	13 (12)	2 (4)	3 (2)
D	0 (2)	3 (4)	3 (5)	3 (2)	4 (1)
E	0 (1)	1 (1)	0 (2)	2 (0)	1 (0)

most persistent practice. The large individual variations, independent of age and sex, must be accounted for chiefly by structural differences in the sense organs.

It is probable that the organ of Corti reaches its maximum efficiency at the age of about ten, and that it then begins to deteriorate, especially if it is not called into systematic activity. This would be analogous to the known fact that the range of perceptible pitch early reaches its maximum extent in children and then gradually narrows down, so that adults do not perceive as high or as low pitch as children.

Do those who have keen sense of hearing tend to have good discriminative sensibility for pitch? The question is answered affirmatively by Table XI in which the data on the two processes are collated. The deviation from the most probable distribution without any functional relation, which is indicated in the parentheses, is in the direction of a correlation between hearing-ability and discriminative sensibility for pitch. But this correlation is not strong.

Do the bright children have a keener discriminative sensibility for pitch than the dull? This is answered by the collating of the classification according to the discriminative sensibility for pitch with the classification according to the general mental ability, in Table XII. There is no functional relation; the distribution of the results practically coincides with the most probable distribution according to chance, which is indicated in the parentheses. This is the strongest

TABLE XII.

Comparison of Discriminative Sensibility for Pitch and General Mental Ability.

Discriminative Sensibility for Pitch.	General Mental Ability. *A*	*B*	*C*	*D*	*E*
A	5 (5)	8 (6)	3 (7)	5 (4)	1 (1)
B	16 (14)	18 (16)	18 (17)	7 (10)	2 (3)
C	6 (8)	7 (10)	15 (10)	5 (6)	3 (2)
D	6 (7)	7 (8)	8 (9)	8 (5)	2 (2)
E	4 (3)	3 (3)	3 (4)	2 (2)	1 (1).

evidence in favor of the theory that the discriminative sensibility for pitch depends principally upon the natural structure of the end organ and is subject only to small variation with education.

Miss Mabel Williams has found, in some experiments now in progress in the laboratory, that there is a natural limit to discriminative sensibility for pitch, which may be reached with little or no practice. Some persons experimented upon have been given twenty periods of practice on twenty successive days. Of the three who have had some musical education,

two failed to lower their thresholds at all in that time, and one made some improvement but had a relapse in the trials that were continued after the twentieth day. Of the two who had no musical education, one, a girl aged twelve, could not perceive an interval of 30 v. d. at the beginning, but gradually improved up to a threshold of 5 v. d. in the twentieth trial. The other, a university student, remained at the same threshold, 12 v. d., during the twenty periods of practice.

V. Motor Ability, Reaction-time, Rhythm, and Time Sense.

The following is a statistical study on the time of mental processes.[1] It is an attempt to bring together some of the approved experiments and reduce them to a reliable statistical form. The measurements are made upon a well defined group of persons, under uniform conditions, and with reliable apparatus and methods. The student observers are designated by the same numbers in all the tables of results.

1. *Time of Action, Simple Reaction, Discrimination, and Choice.*

The following written directions to the observer indicate the nature of the tests in this series:

"There are five sets of experiments in this series and all should be made under the same general conditions. The warning 'Now' will be given from two to five seconds before each stimulus. React as quickly as possible and do not allow yourself to be disturbed by any form of distraction. In Sets A, B, and C, do not direct your attention toward the stimu-

[1] It is perhaps unnecessary to state that the expression "time sense" does not imply that we possess a special sense of time. And the separation of the processes in complex reaction-time, does not imply that any abrupt psychical divisions exist.

lus, but concentrate all thought and effort upon the movement to be made by the finger.

A. Simple reaction to touch. Press the button as soon as you are touched on the forehead.

B. Simple reaction to sound. Press the button as soon as you hear the click in the receiver.

C. Simple reaction to light. A light will appear; press the button as soon as you see it.

D. Reaction after discrimination in sight. Either one or two lights will appear; press the button as soon as you have distinguished whether there is one or two, and immediately call out the appropriate number.[1]

E. Reaction after discrimination and choice. Either one or two lights may appear; if one, press the button; if two, do not.

F. (Special) Greatest rapidity and regularity of action. Press the key, with a small movement of the index finger of the right hand, as rapidly and regularly as possible."

All the records were taken by the graphic method. A Deprèz marker was so connected that it recorded the application of the stimulus and the following reaction. The records were read on a 100 v. d. time-line. Although the records were made in such a way and under such conditions that they could have been read in thousandths of a second, they are given only in hundredths of a second. The reaction was made with a break key, except in Set A where it was necessary to use a make key. The excursion of the key was reduced to a minimum. Latent times have been eliminated. Special reference must be made to the stimuli and the conditions of each test.

A. The Scripture touch-key[2] was used by an assistant in touching the forehead of the observer. A light tap with the hard rubber point of this produces a distinct sensation of impact. The missing records in the accompanying tables were thrown out because in obtaining them the make-contact of

[1] This is a satisfactory control method. The observer will call out the number for which he reacted whether the reaction is right or wrong.

[2] See Stud. Yale Psych. Lab., 1895, III. 107.

this key was used, and that introduced a too great source of error. With the light break, that was used afterward, the record was made upon the beginning of the impact.

B. A telephone click served as sound stimulus. Its intensity was such as to make it clear and yet not disagreeable.

C. The sight stimulus consisted of a light produced by the fluorescence of a Crooke's tube, seen through an aperture, 10 mm. in diameter, at the back of a dark-cabinet, 1 M deep. The tube was illuminated by one hundred flashes per second. This is a more accurate sight stimulus than can be produced by any form of a shutter, and obviates the sound accompanying the spark.

D. Here the same stimulus was used as in Set C, with the only variation that a second aperture could be left open at the control of the experimenter, thus producing two similar lights. The two apertures were 20 mm. apart. The adjustment for one or for two lights could be made noiselessly between the trials. Eight single and seven double lights were used.

E. The stimulus was the same as in Set D. Ten double lights were interspersed in irregular order with fifteen single lights.

F. The button of the key made an excursion of about 3 mm. and required a pressure of nearly 100 g. The record was taken at the end of seven seconds of tapping. The observer did not know that the record for the first seven seconds was not taken. The period recorded includes a complete movement of the finger.

About eight trials were given for practice in each of the reaction tests. The trials were made in the double fatigue order for the series of tests. No clear distinction can be made between sensory and motor reactions for untrained observers, but the directions aimed to make the simple reactions of the motor type. All the conditions were reduced to such as were simplest and most favorable for quick and accurate response. The results are contained in Table XIII.

The relative length of the simple reaction-times varies in

the generally accepted order. Reaction to hearing is the shortest and most uniform, reaction to touch upon the forehead is next in order of length, and reaction to sight requires the longest time.

The discrimination-time is a more definite quantity than it has generally been considered to be. The average time for the men is 0.08 sec., and for the women 0.07 sec. The mean variation is no greater for the gross discrimination-time than for the gross time which is terminated by choice.

The simple choice-time is a trifle longer than the simple discrimination-time. It is 0.10 sec. for the men and 0.08 sec. for the women.

Erroneous reactions may operate in one of two ways: some persons shorten the average complex reaction-time by taking the chance of incurring some errors, and others are hampered by the occurrence of an error so that they become over-cautious and lose time in hesitation. Thus premature reactions may signify a gain to some and a loss to others.

The fact that a proportionally smaller number of errors are made in Set D than in Set E indicates that the premature choice-reactions are not due to a failure to discriminate before acting, but to a failure to inhibit the habitual action.

While in general there seems to be a fairly constant ratio between the different simple reaction-times, and between the simple and the compound reaction-times, there are some conspicuous exceptions that point to individual peculiarities in the attitude toward the different senses and peculiarities in the relative readiness of thought and action. Thus there are indications of ear-mindedness and of eye-mindedness, so also of the deliberate and accurate observer and of the erratic and impulsive actor. These signs are most interesting to one who is acquainted with the observers.

The complete time for the most rapidly and regularly repeated movement (F) is equal to the shortest reaction-time (B); i. e. the double motor process in Set F requires the same length of time as the combined sensory and motor processes in Set B. It may be that the repeated movements would have

TABLE XIII. (*A.*) *Men.*

Time of Action, Reaction, Discrimination, and Choice.

N	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*e*	*E*	*d*	*f*	*F*	*d*
2			13	2	21	2	35	9	0	38	7	0	14	1
4			16	2	20	2	29	3	1	39	7	2	18	1
6			15	3	22	8	26	3	0	32	9	0	16	1
8			13	2	20	2	31	7	0	34	3	1	12	0
10			17	2	19	3	27	4	0	51	7	2	17	1
12			13	4	23	2	25	4	0	27	7	2	18	1
14			13	2	17	2	25	4	2	32	5	1	14	1
16	21	7	16	3	21	3	25	3	0	33	4	2	12	0
18	18	1	15	2	22	1	28	3	1	41	7	1	17	1
20	15	1	12	1	18	2	24	4	0	38	5	1	13	0
22	20	3	17	4	21	4	25	6	1	51	14	1	14	1
24	18	3	14	3	25	4	37	8	0	45	8	2	15	1
26	19	2	17	4	22	3	23	3	0	32	5	3	12	1
28			14	2	22	3	31	5	0	46	8	2	17	1
30	19	2	14	1	23	5	26	4	0	43	6	1	12	1
32	19	2	16	4	20	4	24	5	0	39	9	2	16	1
34	23	3	15	2	23	4	47	10	0	46	9	0	15	1
36	24	2	15	1	19	2	28	5	0	37	4	1	15	1
38	17	2	13	1	21	3	24	3	0	39	6	0	13	1
40	17	2	13	1	20	3	27	3	0	30	4	1	12	2
42	15	2	13	1	22	1	32	8	1	39	5	2	14	0
44	17	2	11	1	15	2	23	4	1	28	5	2	14	1
46	15	2	12	1	19	3	33	6	1	44	4	1	14	1
48	16	3	12	1	21	2	41	1	0	60	6	0	15	1
50	17	1	14	1	23	3	29	6	0	33	2	1	16	1
52	17	1	13	2	20	4	44	6	0	42	5	1	16	3
54	21	2	16	3	23	2	31	8	0	36	4	3	14	1
56	19	4	15	3	19	2	24	4	0	34	4	2	18	1
58	16	3	13	1	20	5	28	7	0	38	5	0	11	1
Average	18	2	14	3	21	3	29	5		39	6		15	1

Unit of measurement, 0.01 sec.

A, B, C, D, E, and F, the tests as designated in the "directions."

d, mean variation for the fifteen trials on each point. This is the average of each individual's variations, regardless of sign, from the average record for all the observers.

e, number of premature reactions. This represents half the probable number because half of the premature reactions would be right by chance.

f, number of errors made (in ten control trials) by premature reactions, i. e. by reacting to two lights.

TABLE XIII. Continued. (*B.*) *Women.*

N	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*e*	*E*	*d*	*f*	*F*	*d*
1			21	4	31	3	35	6	0	42	3	0	17	1
3			14	2	19	4	38	11	0	44	13	1	13	1
5			15	2	18	2	28	3	0	34	5	2	13	0
7			13	1	24	3	36	5	0	44	4	1	13	0
9			16	1	23	2	39	6	0	45	5	1	20	1
11			26	4	27	7	33	7	0	39	4	1	22	1
13	17	3	12	1	22	2	30	4	0	39	4	3	13	1
15	17	2	12	2	22	4	30	5	0	37	6	1	12	2
17	16	3	14	1	16	3	23	5	0	45	5	1	15	1
19	25	8	15	2	22	2	26	3	0	39	4	1	14	0
21	19	2	15	1	22	2	29	6	0	32	4	0	18	2
23	18	2	14	2	20	7	31	4	1	36	4	2	14	1
25	19	3	18	2	22	3	47	10	0	45	10	1	15	0
27	13	4	13	2	16	4	22	2	0	37	5	2	15	1
29	23	5	19	1	25	4	32	7	0	43	11	2	16	0
31	17	1	15	3	17	3	23	4	1	40	8	0	16	1
33	16	1	12	1	18	2	23	5	1	29	6	3	12	2
35	21	2	13	2	26	2	33	4	0	41	5	3	12	0
37	22	2	15	1	26	3	27	3	0	34	5	2	14	0
39	14	3	14	2	21	3	31	11	0	37	4	0	13	1
41	18	2	18	2	22	3	36	11	0	43	3	1	16	1
43	15	3	13	2	25	4	29	6	0	42	9	2	16	1
45	18	2	13	2	23	2	27	5	1	31	5	2	11	1
47	15	1	13	2	20	3	28	3	0	38	8	1	12	1
49	18	3	14	2	19	1	29	5	0	30	2	1	16	1
51	15	1	13	1	20	3	27	5	0	34	4	2	14	0
53	16	2	15	2	24	5	29	3	0	39	9	0	14	1
Average	18	3	15	2	23	3	30	6		38	6		15	1

been shorter in some cases if the regularity had not been demanded, but it is not probable that many sacrificed speed for regularity. There is a tendency for those who have good voluntary motor ability to have short reaction-time, both simple and complex.

There is no remarkable variation with sex in time, uniformity, or reliability. Although it is not necessary to do so, the small variations that do exist may be accounted for by the normal fluctuations in the records.

2. *Free Rhythm in Action.*

The purpose of this test was to determine the most natural rhythm of action and its characteristics in free, simple, and

small movements of a limb in its most natural position. It was required to repeat a light pressure rhythmically by the tip of the first finger.

The Verdin capsule[1] for the study of small movements consists of a metal lever, 20 mm. long and 2 mm. in diameter, mounted vertically on the membrane of an ordinary capsule for the transmission of air pressure. This capsule was connected with a Marey recording air capsule which was mounted on a kymograph. By this means a tracing was obtained showing the form, amount, and duration of the successive pressures that were exerted upon the end of the lever of the Verdin capsule by the observer's voluntary action.

The word rhythm was not mentioned and precautions were taken to prevent the suggestion of any particular rate. The observer was required to press with a force of about 8 g., which required an excursion of about 5 mm. by the finger. The standard was indicated to the observer in some single preliminary trials. During the test the eyes were kept closed. The specific instructions were given in writing, as follows:

"Rest your hand on the table and press this point with the first finger, at regular intervals, always in the same way. Choose any length of interval you like, but retain the same throughout the experiment. The regularity of the interval and the uniformity of the pressure will be recorded."

As the records do not manifest any peculiarity in the mode of pressing, that aspect is disregarded and the results are stated with reference to the length of the periods and the degree of pressure. The former is recorded in columns *A*, *B*, and *C* and the latter in columns *D*, *E*, and *F* in Table XIV. The length of the periods of the rhythm was determined at the beginning, the middle, and the end of each test, by measuring, with a time-line, the distance from crest to crest in five successive waves. The time-line scale was divided into twentieths of a second. The records of the pressure have not been converted into units of weight, but are retained in terms of the original measurement of the amplitudes of the graphic

[1] See Catalogue of Ch. Verdin, Paris.

TABLE XIV.

Free Rhythm in Action.

(*A.*) *Men.*

	Period			*Pressure*		
N	*A*	*B*	*C*	*D*	*E*	*F*
2	18	18	18	11	14	14
4	18	16	15	10	22	27
6	11	11	11	14	24	29
8	22	22	22	7	22	31
10	12	11	10	14	32	34
12	28	28	28	6	6	6
14	26	26	26	9	16	24
16	25	25	25	7	18	43
18	14	13	12	10	13	19
20	17	17	17	4	5	9
22	13	13	13	11	14	20
24	34	34	34	6	22	29
26	28	28	28	10	17	21
28	15	15	14	7	6	10
30	14	13	12	7	11	22
32	24	26	28	8	14	19
36	28	28	28	7	15	20
38	12	12	12	4	5	4
40	11	11	11	6	8	15
42	25	25	25	2	4	5
44	13	13	13	7	8	15
46	6	6	6	12	23	47
48	13	13	13	7	8	15
50	30	30	30	9	23	22
52	17	16	14	6	10	13
54	14	16	26	2	3	6
56	16	16	16	6	9	18
58	14	14	14	6	17	30
			Average	8	14	21

(*B.*) *Women.*

	Period			*Pressure*		
N	*A*	*B*	*C*	*D*	*E*	*F*
1	15	15	15	5	6	6
3	8	8	8	4	15	16
5	11	11	11	6	18	26
9	15	15	15	14	20	22
11	8	10	10	5	6	7
13	10	10	10	14	17	34
15	12	12	12	7	10	13
17	16	15	14	9	32	38
19	18	16	15	8	14	17
21	12	12	12	10	42	51
23	70	70	70	11	30	41
25	40	40	40	4	15	16
27	18	20	24	16	14	11
29	60	60	60	10	19	25
31	10	13	14	8	13	18
33	36	44	48	12	18	38
35	12	12	12	5	5	5
37	9	9	9	8	10	17
41	22	22	22	8	12	16
43	23	23	23	8	8	8
45	20	21	24	8	16	18
49	9	8	7	7	8	9
51	10	10	10	9	13	16
53	8	8	8	5	6	7
55	32	32	35	8	6	6
57	13	13	13	9	14	22
59	10	12	14	8	16	18
			Average	8	15	19

N, the observers by number.

Length of the intervals in the chosen rhythms: A, at the beginning; *B*, at the end of 45 sec.; and *C*, at the end of 90 sec. The figures give twentieths of a second which was the unit of measurement.

Pressure: D, at the beginning; *E*, at the end of 45 sec.; and *F*, at the end of 90 sec. The figures give the amplitude of the graphic waves in millimeters.

waves, because the conversion would not be reliable and the record here given is expressive enough for the present purposes.[1]

The rhythm of these free movements seems as a rule to be determined by the periodicity of the processes of circulation and respiration. The most frequent rhythm is that of the pulse. There may be three cases. (1) The period of the complete movement may be synchronous with the period of the pulse beat. In conditions like those under which this test was made, the pulse may vary, at least, from sixty-six to ninety-two beats per minute in different persons. That is equivalent to pulsations of from 18 to 13 twentieths of a second in length. Nineteen of the observers chose rhythms that fall within these limits. (2) The period of action may be a multiple of the pulse beat. The downward and the upward movements of the finger may be considered as separate movements and each made to coincide with a pulse beat. According to the above estimate of possible pulse rates, periods of action of from 36 to 26 twentieths of a second would correspond to double periods of the pulse. Eight of the records fall within these limits. (3) Two complete movements may coincide with one beat of the pulse. Five of the records fall within such limits, i. e. 8 to 7 twentieths of a second. Observers 23, 25, 29, and 33, were seen to follow the

[1] The recording of pressures by air transmission is not satisfactory, as the functional relation between the pressure and the amplitude of the curve is too uncertain. The recording may be made directly with a very simple device. Take a light wooden lever and support one end of it on pivot bearings and the other from a suspended coil spring. Let a spring pointer project from the side of the lever and trace on a kymograph drum, and suspend a pressure button below the lever or in any other direction by means of pulleys. The amplitude of the graphic record may be varied by placing the tracing point at different distances from the bearing of the lever. The force of the pressure and the amplitude of the movement may be adjusted by changing the point of suspension of the pressure button in a similar manner. Large variations in pressure must be adjusted for by substitution of springs. The friction on the drum will be negligible for most purposes if the tracing point is long and flexible laterally. This dynamograph makes an excellent ergograph. If a constant pressure is desired, a weight acting over a pulley may be substituted for the spring.

rhythm of the respiratory movement. In many other cases it was evident that the respiratory periods were regulated so as to form some multiple of the periods of action. In that way the rhythm of action may have been correlated with both the circulation and the respiration. None of the observers were aware of having followed the pulse or the respiration.

Is the rhythm of action correlated with the pulse at the moment of action or with the average normal pulse? That is, is the subconscious correlation of the two processes direct, or is the chosen rhythm that of the habitual movements whose rhythmic character has been gradually determined by the subconscious influences? That important question is merely suggested by these experiments. The problem is worth studying quantitatively by means of synchronous records of the action, the pulse, and the respiration. But this cannot be done by means of the ordinary sphygmograph and pneumograph, because these instruments suggest special rhythms and, by pressure, bring the pulse and the respiration into the focus of attention. It is necessary that the record of the organic processes be made without the knowledge of the observer.

The regularity with which the chosen rhythm is adhered to is remarkable. If a person be required to act with a given rhythm, e. g. two second intervals, there will appear a strong tendency to acceleration. (See next section.) The absence of such a tendency here may indicate that the regularity of the rhythm was determined by some present gauge like the processes that have been mentioned.

There is a strong and constant tendency to increase the degree of pressure during the free, rhythmic activity. As may be seen in columns *D*, *E*, and *F* in Table XIV, there are only four exceptions to this rule in the fifty-five cases. The amount of increase is remarkable. The figures in the tables that record the amplitude of the waves in the graphic tracings do not fully represent the amount of increase because the deflection of the tracing pointer is relatively less for the greater pressures. The average pressure is at least three times as great at the end of the ninety seconds as at the

beginning. No reference was made to the standard of pressure after it had been once indicated in the preliminary trials. This accounts for the difference in standard that is chosen in the beginning by the various observers. All were astonished at the amount of increase shown by the graphic records.

There is a slight tendency for the women to choose faster rhythms than the men. Thus eight of the women chose periods of 10 twentieths of a second, or less, while only one of the men chose a rate as fast as that. On the other hand the women also chose the longest periods. Five women and only two men chose periods of 30 twentieths of a second, or more. This is in accord with the frequent observation that the actions of a given class of men are more uniform than the actions of a corresponding class of women. This difference appears in a similar way in other experiments, as may be seen from the tables.

3. Regulated Rhythm in Action.

The purpose of this test was to obtain a measurement of the ability to reproduce or follow a fixed rhythm, and to determine some of the constant tendencies in such action.

The latest model of the Meumann time sense apparatus was used on a Zimmermann kymograph. The rhythm was marked by a sounder connected with this apparatus. The records were traced by a Depréz marker in circuit with a telegraph key with which the observer indicated the rhythm. All latent times have been eliminated.

Three rhythms were chosen to represent respectively the slowest, the most favorable, and the fastest movements that could be performed by all the observers within the limits of accuracy to be specified. A period of 2.80 sec. was adopted as the longest that all could reproduce with less error than plus or minus the reaction-time; a period of 0.48 sec. was taken as the shortest that all could reproduce without confusion; and a period of 1.08 sec. was selected as perhaps the most favorable. These three rhythms constituted one series. Sufficient practice was given in which to become familiar

with the action of the apparatus and acquire the rhythmic movement. Then, without stopping, forty records were made in succession with the same standard. The same procedure was followed for each rhythm, the tests being made in the order the records are given in the tables. During the test the observers kept their eyes closed. The written instructions were given as follows:

"Hold the key like a telegrapher, observe the regular beat of the sounder, and mark this time by pressing the key so that the click of the closing of the key coincides with the click of the sounder."

Table XV sets forth the individual and the general characteristics that are exhibited in these forms of regulated rhythmic action. It gives a sort of birds-eye view of the class. The records show tendencies to anticipate, to lag, to fluctuate, and to be exact; and these tendencies are indices to the temperament, and the habitual modes of perception, attention, and action.

In comparing the degrees of success for the different rates, a standard is chosen approximately proportional to each period. Thus, the trial is considered a success if the period is estimated correctly within ± 0.05 sec. in rate I, ± 0.02 sec. in rate II, and ± 0.01 sec. in rate III. Considering both the standard of success and the number of successful estimates, we find that the ratio of success is about the same for all three rates, i. e. the degree of success is proportional to the length of the period.

There is a strong tendency to underestimate the period, i. e. to anticipate the regulating click in the fast and the medium rates. This is due to the tendency to accelerate a given rhythm. With many of the observers there was a noticeable rhythm in this acceleration itself. Some observers would gradually accelerate until it dawned upon them that they were constantly anticipating; they would then make a fresh start and soon find that they were again accelerating. Others plainly made a uniform effort to check the tendency to accelerate in the latter part of the test. Several protested

TABLE XV. (*A.*) *Men.*

Regulated Rhythm in Action.

	I. Slow: 2.80 sec.					*II. Medium: 1.08 sec.*					*III. Fast: 0.48 sec.*				
N	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*
2	32	60	21	8	9	8	92	7	0		30	45	3	5	3
4	0	100	32	0		36	49	7	15	7	34	19	2	47	3
6	41	46	16	13	10	35	65	9	0		37	41	2	22	2
8	15	23	22	62	18	38	25	4	37	7	66	11	3	23	3
10	39	36	13	25	15	20	72	7	8	5	5	95	5	0	
12	23	49	14	28	11	13	74	16	13	13	18	72	6	10	3
14	15	60	23	25	13	20	67	8	13	5	22	66	4	12	3
16	24	52	13	14	7	8	92	9	0		24	66	5	10	3
18	18	32	17	50	10	31	56	7	13	4	14	76	5	10	3
20	28	59	17	13	9	19	43	6	38	5	30	66	8	4	2
22	18	46	22	36	15	23	72	9	5	3	23	64	3	13	3
24	35	23	37	42	28	15	80	8	5	4	23	64	3	13	2
26	21	64	14	15	11	33	67	6	0						
28	22	53	29	25	12	31	36	7	33	7	18	46	8	36	8
30	0	0		100	17	23	0		77	7	13	5	2	82	3
32	17	35	17	48	12	26	59	11	15	11	10	90	6	0	
36	21	37	14	22	15	47	27	5	26	5	47	38	3	15	2
38	42	11	9	47	13	45	22	5	33	5	42	51	3	7	3
40	39	35	14	26	11	40	38	5	22	5	19	81	4	0	
42	0	0		100	31	5	8	5	87	13	29	23	3	48	4
44	24	38	14	38	9	35	44	5	21	6	64	12	3	24	2
46	33	59	24	8	10	18	68	6	14	5	27	68	3	5	3
48	21	44	22	35	16	21	57	5	23	4	47	27	3	26	3
50	15	60	26	25	17	38	62	6	0		39	53	3	8	3
52	5	8	15	87	23	27	73	8	0		50	33	3	17	2
54	0	0		100	14	20	5	5	75	10	11	89	4	0	
56	20	30	18	50	22	46	30	3	24	5	59	8	2	33	2
58	29	55	20	16	10	21	74	8	5	5	38	62	3	0	
62	23	63	12	14	9	13	87	10	0		5	95	6	0	
64	28	22	28	50	25	23	71	5	6	5	2	98	6	0	
66	26	50	19	24	14	0	100	18	0		0	100	6	0	
	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Av.	22	40	19	38	15	26	55	7	19	6	29	55	4	16	3

Unit of measurement, 0.01 sec.

Number of trials, 40 for each rate.

N, the observers by number.

%*R*, per cent of cases right within ± 0.05 sec. in rate I; within ± 0.02 sec. in rate II; and within ± 0.01 sec. in rate III.

%*U*, per cent of cases in which the observer underestimated the period.

%*O*, per cent of cases in which the observer overestimated the period.

AU, average time of underestimation, i. e. anticipation.

AO, average time of overestimation.

TABLE XV. Continued. (*B.*) *Women.*

	I. Slow: 2.80 sec.					*II. Medium: 1.08 sec.*					*III. Fast: 0.48 sec.*				
N	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*
1	32	34	20	34	10	0	100	28	0		0	100	7	0	
3	33	8	12	59	12	33	27	6	40	8	33	41	3	26	3
5	4	50	20	46	12	0	100	14	0		0	100	14	0	
9	13	50	19	37	16	20	36	7	44	6	3	97	8	0	
11	27	24	14	69	12	2	0		98	9	10	71	5	19	4
13	25	16	16	59	16	12	82	10	6	4	20	67	4	13	4
15	25	44	15	31	12	19	66	6	15	7	23	70	5	7	6
17	3	3	7	94	16	42	44	7	14	5	8	80	6	12	3
19	15	10	15	50	29	0	100	18	0		5	95	5	0	
21	5	25	27	70	12	10	87	11	3	8	11	78	3	11	3
23	23	8	10	69	13	9	60	9	31	8	59	27	4	14	3
25	28	37	19	40	21	19	56	6	25	6	53	31	3	16	2
27	6	14	11	80	13	31	9	4	60	7	38	15	2	47	2
29	11	31	20	58	14	33	35	8	32	7	25	50	3	25	3
31	11	8	23	7	6	13	82	9	5	5	0	100	5	0	
33	14	67	53	19	18	29	53	12	18	7	27	66	3	12	3
35	20	35	20	45	20	38	26	5	38	4	43	57	3	0	
41	13	53	62	34	21	5	92	12	3	3	15	82	4	3	3
43	20	22	18	58	23	28	63	6	9	6	51	35	2	14	2
45	10	70	32	20	15	38	51	6	11	4	20	80	4	0	
49	15	10	27	75	30	17	83	8	0		25	68	4	7	3
51	52	37	13	11	9	50	36	5	14	6	21	68	6	11	4
53	17	83	21	0		7	93	8	0		25	48	4	27	3
55	0	5	24	95	27	20	74	9	6	5	32	42	3	26	3
57	18	45	21	37	22	44	40	5	16	4	20	80	4	0	
59	23	20	17	57	17	58	30	5	12	4	46	37	3	17	3
Aver.	18	31	21	51	17	22	59	9	19	6	24	64	5	12	3

that the standard rhythm had gradually become slower; they considered their action uniform and ascribed the variation to the regulating apparatus.

The men have somewhat better records than the women in all three rhythms, both in regard to the per cent of successful trials and in the degree of approximation to the standard.

4. *Time-estimate.*

The following is an attempt to determine some of the constant tendencies in the appreciation of intervals of time. The investigation is limited to one kind of interval, namely, the

so-called "empty" interval. The following lengths of interval were chosen: ¼ sec., ½ sec., 1 sec., 2 sec., 5 sec., 10 sec., 20 sec., and 40 sec. These intervals were divided into two series, the first four being in one and the last four in the other.

First Series: Short Intervals.

The Meumann time sense apparatus was used to produce clicks in a sounder. By this apparatus time intervals of the lengths here required could be marked off with uniformity and precision. The latent time of the sounder was eliminated. The records were made on the kymograph by the usual graphic

TABLE XVI. (*A.*) *Men.*

Time-estimate: Short Intervals.

N	*25*	*d*	*50*	*d*	*100*	*d*	*200*	*d*
6	24	2	55	7	78	7	179	18
12	27	4	51	6	87	7	181	18
16	24	5	47	4	83	15	152	22
18	25	4	49	5	88	7	181	17
22	26	6	47	8	89	10	164	11
24	25	2	48	3	84	4	129	22
28	47	17	56	9	88	17	127	8
30	25	2	48	4	95	4	181	9
32	33	2	63	9	91	16	147	8
36	29	2	64	7	110	11	197	20
38	28	2	54	6	85	10	132	9
40	26	3	46	2	84	15	129	12
42	44	5	72	13	101	18	173	19
46	23	2	51	9	96	13	117	7
48	23	2	48	4	88	11	138	12
52	32	10	39	4	70	13	166	19
54	39	10	54	6	94	6	141	12
56	33	7	59	6	100	14	189	18
58	28	6	55	6	82	15	185	19
60	20	3	46		94		169	16
Average	29	5	53	6	89	12	159	14

The numbers at the head of the columns give the standard intervals.
Unit of measurement, 0.01 sec.
N, the observers by number.
d, mean variation.

TABLE XVI. Continued. (*B.*) *Women.*

N	*25*	*d*	*50*	*d*	*100*	*d*	*200*	*d*
1	27	6	57	11	85	11	160	25
3	28	4	50	9	95	11	182	11
5	30	3	40	8	99	0	136	10
7	26	8	55	6	93	7	154	14
9	31	4	52	5	85	6	174	31
13	24	4	53	5	94	3	169	21
15	27	2	44	5	98	4	175	6
17	23	3	56	5	103	9	197	15
19	25	2	47	5	97	1	159	19
21	34	6	50	10	76	10	151	15
23	34	3	60	4	98	5	141	13
25	32	6	61	6	96	12	138	6
27	39	12	68	11	91	14	192	7
29	30		49	2	73		183	18
31	32	7	55	9	94	7	182	29
33	27	2	46	5	82	12	113	18
35	38	3	48	3	93	9	172	12
37	32	3	52	3	92	5	121	10
41	38	4	66	9	105	5	152	8
43	22	2	48	4	91	4	129	4
45	27	3	57	3	78	15	108	9
49	31	8	41	4	91	10	134	14
51	27	2	54	8	91	7	184	16
55	28	3	57	8	92	11	129	11
57	26	3	47	8	97	13	180	20
59	38	2	52	13	95	14	179	16
Average	29	4	52	6	92	9	157	15

method and were read in hundredths of a second. Eight trials were made for each interval in the double fatigue order. The observers did not know anything about the length of the intervals or their order in the series, except what could be learned in the progress of the actual test. Special instructions were given in writing as follows:

"The experimenter marks off a certain interval of time by two clicks. After about two seconds, reproduce it by two similar clicks with the key. *Cautions:* Try to think of the passing time only. Do not count, estimate number of seconds, or use any mechanical aid. If any such process occurs invol-

untarily the trial must be repeated. In pressing the key act with precision."

The short intervals are overestimated and the long intervals are underestimated by amounts shown in Table XII. This tendency is uniform and the uniformity is in no sense due to the observers' knowledge about each others records. Men and women show equal ability.

A comparison of Tables XIII and XVI shows that the observers, who overestimate the shortest interval most, tend to have poor motor ability and reaction-time. But the overestimation of this interval is not due to inability to act quickly enough, because two taps can be made in as rapid succession as a series of taps can be made, and no one required more than 0.20 sec.

TABLE XVII. (*A.*) *Men.*

Time-estimate: Long Intervals.

N	*5*	*d*	*10*	*d*	*20*	*d*	*30*	*d*
6	5	1	10	1	19	2	33	4
12	5	1	10	3	16	2	31	4
16	5	1	9	3	20	2	36	3
18	5	1	10	1	18	3	35	4
22	5	1	10	1	20	3	33	4
24	7	2	10	2	22	5	40	2
28	4	2	7	3	13	2	29	5
30	6	2	11	3	15	2	26	3
32	7	1	11	2	25	6	33	6
36	5	1	13	4	20	3	42	10
38	4	1	8	1	11	1	17	1
40	6	2	12	4	24	6	45	10
42	9	3	11	4	25	5	33	4
46	5	2	11	3	18	5	34	8
48	5	2	9	5	23	10	40	6
52	5	1	9	1	17	2	36	3
54	5	1	12	2	19	1	35	2
56	6	2	12	4	22	4	35	10
58	5	1	10	2	22	3	36	7
60	5	2	9	2	16	4	44	7
Aver.	5.5	2	10.2	3	19.3	4	34.7	5

Unit of measurement, 1 sec.

Other notation same as in Table XVI.

TABLE XVII. Continued. (*B.*) *Women.*

N	*5*	*d*	*10*	*d*	*20*	*d*	*40*	*d*
1	4	1	7	1	15	3	27	5
3	3	1	9	1	23	0	34	2
5	4	2	6	1	22	8	31	2
7	3	0	10	4	13	3	34	15
9	5	1	9	2	22	1	35	7
13	5	1	10	2	17	6	28	5
15	6	1	13	5	20	4	33	3
17	4	2	6	1	13	3	27	4
19	6	2	10	1	22	4	44	8
21	4	1	7	2	12	4	23	2
23	5	1	8	1	12	2	29	2
25	6	1	11	3	17	3	44	4
27	5	1	10	2	20	1	51	6
29	9	2	14	4	35	3	46	7
31	10	3	18	4	31	9	34	5
35	6	2	10	5	15	3	28	2
37	4	1	9	2	15	3	26	2
41	4	0	9	0	16	4	39	7
43	4	0	7	1	14	2	35	10
45	4	1	9	1	21	3	41	4
49	4	1	6	1	11	2	26	5
55	5	1	9	2	18	1	39	4
57	3	1	9	2	19	4	36	4
59	4	1	8	1	17	1	33	7
Aver.	4.9	1	8.9	2	18.3	4	34.3	5

(F in Tab. XIII) for the complete movement in tapping, which was exactly the same as the movement in the signalling in this test. Fearing that the overestimation might still be due to the inability to act, I called back some of those who had overestimated, repeated the trial, and asked them whether they were aware of the fact that they had overestimated. No one was distinctly aware of the overestimation and no one thought that the interval was too short to be reproduced.

It is to be regretted that I cannot give a more adequate account of the observations upon this very important point. However, the gauging of the smallest interval by the measured voluntary motor ability, and the information elicited through questions in the extra trials, convince me that this overestima-

tion of the short interval is not due to inability to act quickly enough, but it is a normal illusion in the perception of the standard interval. The evidences warrant the conclusion that the time that is associated with his own quick actions seems shorter to the slow person than to the quick person. Does the slow boy realize how long an interval elapses before he begins to reply to the teacher's question? And, when he has once started, do the intervals between his words seem as long to him as they seem to the quick boy at his side? The lagging of a slow person in all rushing activities does not seem as great to him as it does to the quick person. Persons who are habitually too slow in their quickest actions have established erroneous associations between standards of time and the time of their own actions.

Second Series: Long Intervals.

(*This test was made by Miss Anna Kierulff.*)

This test followed immediately upon the foregoing and was made as nearly as possible under the same conditions. The experimenter kept time with a Runné chronometer and signalled with the sounder by pressing an electric key. The observer had a similar key and signalled with the same sounder at the beginning and at the end of the interval. The chronometer was equipped with an electric starting and stopping attachment in such a way that the interruption of the circuit that produced the first signal started the chronometer and the interruption that produced the second signal stopped it. The observer was seated at a distance of forty feet from the experimenter, and kept the eyes closed. Four records were taken upon each point in the double fatigue order. The written instructions that had been given before were repeated, and a few preliminary trials were given with intervals chosen at random within the limits of from five to fifteen seconds. The results are contained in Table XVII.

In this series the shortest interval is estimated almost correctly, and the other intervals are underestimated somewhat in proportion to their length. But the shortest interval in this

series is two and a half times as long as the longest interval in the foregoing series, which is underestimated by nearly one-fourth of the actual length. The conclusion is evident, that the tendencies to overestimate and to underestimate are not absolute. They are related to the setting, as it were, in the associated intervals. The two parts of this experiment became, to the observer, two different groups of time relations with little or no effect upon each other. If they had been reduced to one unbroken series, tried with the same apparatus, and with the observer in the same position, it is probable that the illusory effects would have been stronger for the extremes, $1/4$ sec. and 40 sec., and that the tendency to underestimate would not have appeared for the four short intervals. The variation that depends upon the grouping of the intervals is due to the bodily and the mental attitudes of expectancy that are created by the grouping.

Third Series: Variation with Mental Development.

(*These tests were made by Miss Mary Hornibrook.*)

The Second Series of tests were repeated upon school children. (See description of the general method and conditions, p. 3.) Sight signals were substituted for the sound signals in order to simplify the conditions. The experimenter and the child each had a card fastened to a staff like a flag. Each also had a screen behind which the card could be readily placed out of view. The experimenter gave the instructions in the form of a simple and direct command requiring the child to observe how long she showed her card and then show his card for exactly the same length of time. Two trials were made for each interval in the double fatigue order. After the regular series of trials, the child's conception of a common time-interval was measured by requiring him to show his card "exactly one-half minute." As there is no constant variation with sex, the records for the boys and the girls are given together in Table XVIII.

The illusion is of the same nature as for the students, but it is stronger for the children. It decreases with increase in

the age of the children. This is primarily an illusion of childhood. It is easy to see how the feeling of suspense may lead them to terminate the interval prematurely. The error in the estimation of an absolute interval is much greater than the error in the reproduction of a given standard interval. To the children, one-half minute means less than a quarter of a minute.

TABLE XVIII.

Time-estimate by Children.

Age	*n*	*5*	*d*	*10*	*d*	*20*	*d*	*X*	*d*
6	19	5	2	6	2	10	4	9	6
7	19	4	1	6	2	10	4	10	5
8	23	4	1	7	2	14	5	13	7
9	16	4	2	8	3	13	4	12	4
10	15	5	2	8	2	15	4	17	6
11	23	5	2	8	2	18	4	16	6
12	20	5	1	8	2	14	3	13	4
13	20	5	1	9	2	16	3	13	3
14	19	5	1	8	1	15	3	12	4
15	13	4	1	9	1	17	4	18	6
Average		5	1	8	2	14	4	13	5

Unit of measurement, 1 sec.
n, number of children, boys and girls taken together.
d, mean variation.
X, free estimate of one-half minute.

To determine whether the bright children are subject to these time-illusions to a less extent than the dull children, the classification of the children according to general mental ability was collated with the classification according to the degree of illusion, by the method illustrated on p. 59. There appears to be no functional relation between the two processes.

On the Analysis of Perceptions of Taste

BY

PROFESSOR G. T. W. PATRICK.

The following is an experimental study[1] designed to contribute to the further analysis of so-called perceptions of taste into their constituent sensations. It is based upon a series of tests made with an anosmic observer in tasting a considerable number of common articles of food and drink. The account of the experiments themselves is preceded by a brief discussion of the psychology of taste as at present understood.

Taste perceptions are, for the most part, complexes having as their constituent elements sensations of taste, smell, touch, temperature, sight, and muscle sensations, but no exact analysis of such perceptions has ever been undertaken. Popularly taste perceptions are supposed to be made up largely at least of taste sensations, and we speak of the "taste" of cheese, milk, wine, tea, coffee, chocolate, strawberries, peach, apple, beef, mutton, turkey, oysters, etc., as well as of sugar, salt, and vinegar. In the same way all volatile substances which are perceived through the nose are popularly called odors, and we speak of the smell of ammonia, menthol, acetic acid, as well as of violet, wintergreen, and camphor. If we turn from the popular view to the sciences of physiology and psychology, we find only a partial escape from this confusion. Despite the recent valuable researches of Kiesow upon the sense of taste and the work of Zwaardemaker on the physiology of smell, both the physiology and psychology of these senses are in a very undeveloped condition, particularly as

[1] A preliminary report upon some of the experiments included in this article was made at the meeting of the American Psychological Association, at New York, December, 1898.

regards our knowledge of their qualitative differences. We learn that sensations of smell are indefinite in number and roughly and unsatisfactorily classifiable into nine or ten classes.[1] Whether these many odors are unanalyzable simple sensations or complexes of a few elementary odors is not known. Nagel, from recent experiments in mixing odors, thinks it probable that our common perceptions of smell are fusions of a certain number of elementary odors, but these he does not attempt to name.[2]

As regards taste, physiology and psychology teach us hardly more. We learn that there are only four simple tastes, sweet, bitter, salt, and sour; that these elementary taste sensations do not stand in any determinable relation to each other except that they present some of the phenomena of compensation and contrast; and finally that tastes and odors are constantly confused in experience. If we read further in the text books, we find that there is much confusion as to the composition of the taste perceptions of common experience. Instead of four simple tastes, we are told presently that there are a vast and indefinite number, and that sweet, bitter, salt and sour are merely four *classes* into which all tastes may be conveniently divided. For instance Baldwin says: "Tastes are infinite in their variety and cannot be classified. Certain classes of tastes are well discriminated in experience, such as sweet, bitter, sour; but they are very few compared with the vast number which remain undescribed."[3] Foster says: "We recognize a multitude of distinct tastes, which may be broadly classified into acid, saline, bitter and sweet tastes."[4] Wundt says: "We can distinguish four distinct primary qualities. Between these there are all possible transitional tastes, which are to be regarded as mixed sensations."[5] To quote finally from a

[1] Compare ZWAARDEMAKER, Die Physiologie des Geruchs, XIII.

[2] *Ueber Mischgerüche und die Komponentengliederung des Geruchssinnes.* Zeitschr. f. Physiol. u. Psychol. d. Sinn., 1897, XV, 82.

[3] Hand-book of Psychology: Senses and Intellect, p. 87.

[4] Text-book of Physiology, Fifth edition, Part IV, p. 221.

[5] Outlines of Psychology, Eng. trans., p. 53.

recent valuable monograph by Dr. Kahlenberg, he says: "The sensations of taste are commonly classified as those of sweet, sour, salty, and bitter. * * There can be no doubt, however, that there are very many kinds and shades of taste that are quite distinct and not to be referred to sensations of touch, and that the above classification can claim at best to be only a very rough one. The investigator of this subject is soon struck by the fact that we have so few names to describe the various tastes."[1]

Now there are several interpretations that might possibly be given to these expressions, all of them perhaps representing views that are freqently held as to the composition of taste perceptions. The first is the one already referred to, that sweet, bitter, salt, and sour are merely classes of tastes based on four kinds of resemblances among an indefinite number of qualitatively different taste sensations. This view, I think, is not seriously held by any physiologist or psychologist. The tendency has been towards a constant decrease of the number of qualitative differences, from an indefinite number, first to ten, then six, then four, and finally by some to two. The second view is that the four elementary tastes have merely a physiological basis in four different kinds of nerve endings, like the three or four hypothetical elementary processes in the theories of color sensation, and that an indefinite number of tastes may result from the proportions in which these elementary processes are set up. We might thus have any number of transitional tastes like the various color tones of the spectrum. This view also must be rejected, as there is no known ground for it either in anatomy or physiology, or in psychological observation, and it derives no support from experiments in combining simple taste stimuli. A third view is that the indefinite number of tastes are not physiological or psychological compounds, but rather mixtures or fusions of the four elementary taste sensations, analogous perhaps to the fusion of tones in musical clangs. A fourth view is that there are not

[1] *The Action of Solutions on the Sense of Taste.* Bulletin of the University of Wisconsin, 1898, No. 25, p. 11.

an indefinite number of tastes at all, but only four. To this hypothesis I shall return below.

As regards the third view, it is perhaps the one which most of the writers quoted above would endorse. However, it presents not only serious theoretical difficulties, but seems to lack clear experimental proof. If we omit quantitative relations, this theory would not give us a vast and indefinite number of tastes but only *fifteen*, for from four elementary tastes only eleven combinations can be made, admitting groups of two, three, and four; and even if the components of the groups might be quantitatively varied, it would seem that we should in experience have a few ever recurring unique and well recognized tastes, as for instance a special taste resulting from the sour-salt fusion, or from the bitter-sour-sweet fusion. But the tastes of common experience are not at all of this kind. Experiments which I have made in the mixing of simple solutions of sweet, bitter, salt, and sour substances have failed to reveal clearly tastes which can properly be called fusions of simple tastes. The observer may say that he tastes a mixture of sweet and sour, where psychologically there are present two simple sensations referred to a mixture of two solutions. The musical clang can only be analyzed in perception by the trained ear and then not always or fully. In any case it has a distinct unity and individuality. Omitting for the moment the possible exceptions described by Kiesow and to be discussed below, no such fusion can be obtained in mixing sweet, bitter, salt, and sour. The solutions are not only analyzable but they are described as "sweet and sour," "salt and bitter," etc.

As a preliminary experiment, I prepared the following standard solutions: Cane sugar, 40 per cent; sulphate of quinine, 0.125 per cent; table salt, 5 per cent; tartaric acid, 5 per cent.[1] These were then combined in equal amounts in the eleven com-

[1] The above percentages for the several substances were chosen because they seem by rough tests to give solutions which, psychologically considered, are of about equal strength. Among these four substances there is certainly not an entire absence of chemical action, as for instance between the salt and tartaric acid, but with these strong solutions the chemical action is not sufficient to affect materially the results.

binations possible with groups of two, three, and four. In addition to these eleven combinations, six other combinations of salt and sugar were made with quantitative variations, with the purpose of determining whether different proportions of simple tastes might combine to produce a new taste. Four observers, two men and two women, tasted the mixtures. The method of procedure was the same as that described in detail below in connection with my other experiments, except that the observers were not blindfolded. The solutions were in glass stoppered bottles marked merely with a code number. The instructions given to the observers were to taste the solution as carefully as possible, swallow it, and observe whether it was sweet, bitter, salt, or sour, or a combination of these, or whether in particular any other taste than these appeared, then, to write the result after the code number of the solution upon paper provided for each observer. The amount of each mixture tasted was about one ccm. taken from a silver spoon. The results of the experiments are exhibited in Table A. In the

TABLE A.

SOLUTION.	MRS. S.	MR. S.	MISS W.	MR. E.
Sweet and sour	Sweet and sour	Bitter.	Sour.	Sour and (sweet)
Sweet and salt.	Sweet.	Sweetish, with an after salty taste.	Sweet and salt.	Salt and sweet.
Salt and sour.	Sour and salt.	A salt and bitter taste combined. The salt first noticed.	A salt, the like of which I never tasted.	Very sour. Does not seem to be simple.
Sweet, salt and sour.	Sour and salt and sweet.	Bitter, followed by an astringent effect.	A peculiar sour, not sweet or salt or bitter.	Sour and salt. Not same as before. Perhaps something more.
Salt and bitter.	Bitter.	A bitter of quinine, very decided.	Bitter.	Bitter and (salt)
Sweet, sour, and bitter.	Bitter and sour.	A bitter, followed by a possible sweetish taste	Bitter and salt.	Sour and bitter.

TABLE A. Continued.

SOLUTION.	MRS. S.	MR. S.	MISS W.	MR. E.
Sweet, salt, sour, and bitter	Bitter, sour, and sweet.	Bitter.	Bitter and sour.	Sour and bitter.
Sweet, salt, and bitter.	Sweet and bitter.	A sweet, decidedly disagreeable bitter.	Bitter and sweet and salt	A faint bitter and sweet.
Sweet and bitter.	Sweet and bitter, sour.	A sweet bitter taste.	Bitter and sweet.	Sweet and bitter.
Sour and bitter.	Bitter and sour.	Bitter.	Bitter, but not quinine. Puckering, like bark.	Bitter, sour and pungent.
Salt, sour and bitter.	Sour and salt.	A sour, bitter taste.	Salt and sour, yet different.	Sour and (salt)
Sweet, 1 part. Salt, 2 parts.	Salt and sweet.	A salty, sweet taste.	Salt and sour.	Sugar and salt.
Salt, 1 part. Sweet, 2 parts.	Sweet.	Sweet.	Sweet and salt, (sour).	Salt and sweet.
Salt, 1 part. Sweet, 4 parts.	Sweet.	Sweet.	Sweet and sour.	Salt and sweet.
Salt, 1 part. Sweet, 3 parts.	Sweet. There is something else. Cannot say what.	Sweet.	Sweet.	Sweet and salt.
Sweet, 1 part. Salt, 4 parts.	Salt and slightly sweet.	Salty.	Salt.	Salt and (sweet)
Sweet, 1 part. Salt, 3 parts.	Salt and sweet and bitter.	A sweetish salty taste.	Salt and sour. A seemingly new combination.	Sweet and (salt)

column under the name of each observer is given his judgment upon each mixture in his own words. We notice, first, that in the sixty-eight judgments included in the tests no new tastes appear. The distinctness or intensity of the sensation varies, but it is always "sweet," "salt," or "sour," or "sour *and* salt," "sweet *and* bitter," etc., although one observer speaks of "a peculiar sour." There are only three exceptions to this. In one case we find the word "astringent," in one case,

"puckering," and in one case, "pungent." These, however, are not tastes, but touch sensations. In the second place, we notice a considerable facility in analyzing the mixtures. Those containing two ingredients are usually correctly analyzed, and those containing three are sometimes correctly analyzed, while without exception every ingredient of every mixture is detected by some of the four observers. The one exception is the mixture containing all four ingredients, where the salt is not detected by any observer. In the combination of sweet and salt in different proportions, we notice that when one or the other is proportionally increased, the observer may detect this alone, but that such a mixture does not result in a qualitatively new taste. This experiment I supplemented by another, taking the standard solutions of different strengths from those in the first experiment and combining them in all the possible ways. It is not necessary to report this experiment in detail, but the results agree with those of the first test.

These experiments, preliminary and incomplete, of course, contribute nothing, therefore, to the support of the third view mentioned above, viz., that the numerous tastes of common experience are mixtures or fusions of four elementary tastes. Thus far there does not appear to be anything like a fusion of tastes comparable to the fusion of tones or colors, which could give us tastes different from the four simple tastes, as white is different from red or green, or as the musical clang is different from the tones which compose it.

Kiesow, in one of the series of interesting and valuable researches upon the taste sense which he has published in Wundt's *Studien* and elsewhere, has considered this problem of the fusion of simple taste sensations, and as a result of his experiments has come to a conclusion which seems to be somewhat different from the one just suggested.[1] As a result of a series of experiments in the mixing of tastes, he concludes that the sense of taste is not like that of smell, where the mingling of different stimuli gives the phenomenon of rivalry rather than

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[1] *Beiträge zur physiologischen Psychologie des Geschmackssinnes. 4. Compensations- und Mischungserscheinungen.* Phil. Stud. (Wundt), XII, 254.

of fusion, and that in this respect the sense of taste is more comparable with sight and hearing. He finds that under certain conditions simple taste qualities, such as sweet and salt, unite to form a mixed sensation, which is a qualitatively new or ground sensation, in which sometimes at least the elementary tastes may be distinguished as we distinguish the elements of a musical clang or of a mixed color like brown.[2] Kiesow finds the best experimental illustration of this in the mixture of sweet and salt. In certain proportions these neutralize each other as such and produce a new sensation which is called insipid, or alkaline, or insipid-alkaline, ("fade," "laugig," "laugigfade"). He tried among others not reported in detail, twenty-five combinations of cane sugar and salt, the solutions combined varying in strength from 1 per cent to 40 per cent. These mixtures were tried upon himself as observer. Of the twenty-five, two gave no taste except insipid or alkaline, three others gave an insipid or alkaline taste in connection with sweet or salt, while all the rest gave either a sweet or salt or a strong preponderance of one or the other.

It is not at all the primary object of the present paper to discuss this problem and it was not possible for us to try any experiments bearing directly upon it which should have anything more than a suggestive value, though it seems to me very desirable to repeat these experiments using a larger number of observers and trying all sorts of combinations of the elementary tastes. Kiesow's results suggest two questions: First, may not the flat, insipid, and alkaline tastes which he observed be otherwise explained than by a fusion or mixture of sweet and salt? Second, even if they are to be so explained, can we hope to find in such fusion any results at all adequate to account for the "infinite variety" of tastes of which some psychologists speak? As regards the first question, I have found that observers tasting distilled water, frequently give the judgment "flat," "insipid,"

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[2] A statement of this view adopted from Kiesow may be found in WUNDT'S Outlines of Psychology, Eng. trans., p. 53.

or "alkaline," and if a weak solution of salt in distilled water is used, the alkaline taste is not unusual. It seems to me highly probable that the alkaline taste that Kiesow got from a mixture of sweet and salt was due to the salt alone. A solution of salt, as much weaker than the one used as the amount of neutralization effected by the sugar, might give the same sensation. I have observed a peculiarity about the taste of salt to which I think attention has not sufficiently been called. In experiments upon minimal tastes made some years ago, Nichols and Bailey reported that salt was tasted by male observers in the proportion of 1 to 2240.[1] While Nichols and Bailey's results in respect to sweet, bitter, and sour have been somewhat closely confirmed by subsequent experiments, those respecting salt have not. Salt may be recognized by a few observers in the proportion of 1 to 600, but more commonly the proportion is 1 to 400 or 1 to 300. Looking for the explanation of this discrepancy, I found that, while with sweet, bitter, and sour, the threshold of recognition coincides nearly with the threshold of sensation, this is not the case with salt.[2] If a solution of salt is made of the strength of 1 to 2000, it can be distinguished constantly, at least by some observers, from distilled water, but it cannot be recognized as salt. The taste is sometimes called alkaline. One of my observers (See Table B.) beginning with a solution of salt in the proportion of 1 to 1100, called the taste alkaline continually until the proportion of 1 to 300 was reached. This circumstance may explain Nichols and Bailey's results and it may also explain the alkaline taste of Kiesow's mixtures of salt and sweet. As for the explanation of the circumstance itself, the hypothesis may be made that the small amount of salt in the solution is sufficient to affect the end

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[1] *The Delicacy of the Sense of Taste.* Nature, XXXVII, p. 557.

[2] Kiesow refers to the difference between the threshold of sensation and that of recognition in the case of sweet, salt, and sour, but makes special reference to it in the case of salt. He is discussing the general fact that all taste sensations are accompanied by touch sensations. See Wundt's *Studien*, X. 525, 531. I have not found that the above difference is appreciable in the case of sweet, bitter, and sour.

organs of touch but not those of taste, the alkaline "taste" being really a touch sensation. Indeed, there are several reasons for thinking with Valentin and others that salt and sour are not true tastes at all. Kiesow's experiments with cocaine, however, seem to substantiate his position that there are four tastes, but that salt and sour in particular are attended by touch sensations.[1]

Returning to Kiesow's conclusions about mixed tastes, I attempted to verify them by repeating some of his experiments. I selected from his list those mixtures of sweet and salt which gave, according to him, most decidedly the new tastes, that is, the insipid and alkaline tastes. These were cane sugar, 1 per cent, and salt, 1 per cent, mixed in the proportion of 50 to 25; cane sugar, 2 per cent, and salt, 2 per cent, mixed in the proportion of 50 to 20; and cane sugar, 4 per cent, and salt 4 per cent, mixed in the proportion of 50 to 10. I tested four observers, two men and two women, with these solutions. They were blindfolded and ½ ccm. of the solution was placed upon the tongue by means of a glass dropper. They were instructed to taste the material carefully and swallow it. They were given a second trial if they desired. They wrote their judgments upon prepared slips, being simply instructed to name the taste if any, whether sweet, bitter, salt, sour, alkaline, or metallic, or any other taste. The results of the test are exhibited in the first part of Table B. The three solutions mentioned above are indicated in the table by the letters a, b, and c. Of the twelve judgments concerning these solutions, three revealed sweet and salt; five, salt; two, sweet; and two, a slight unrecognized taste. No observer pronounced the taste in any case alkaline, flat, or insipid. To complete the experiment, I prepared solutions of salt ranging in strength from 1 to 1100 and 1 to 200, and with these I tested the same observers. The results are exhibited in the second part of Table B. With one

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[1] *Ueber die Wirkung des Cocain und der Gymnemasäure auf die Schleimhaut der Zunge und des Mundraums.* Phil. Stud. (Wundt), IX. 523. Compare idem. X. 524.

TABLE B.

SOLUTION.	MRS. S.	MISS W.	MR. S.	MR. E.
a Cane sugar, 1% 50 parts. Salt, 1% 25 parts	Slight taste, but do not recognize it. Might be distilled water.	Very weak, possibly the faintest suggestion of salt.	A slight taste. I believe slightly sweet.	A taste, but I can't tell what.
Distilled water.	Water.	Same as above, only a little stronger.	Distilled water.	No clear taste.
b. Cane sugar, 2% 50 parts. Salt, 2% 20 parts	Slightly salty.	A little more salty than the preceding.	A salty taste, but very slight.	Salt, perhaps something more.
Distilled water.	Water.	Very like the first. May be a little sweet.	No taste. Water	No taste.
c. Cane sugar, 4% 50 parts. Salt, 4% 10 parts	A salt, sweet taste.	Seemingly a mixture of salt and sweet, but weak.	Sweet, very distinct.	Salt and sweet.
Distilled water.	Water.	Very like water No other taste	Distilled water.	Bitter.
Salt, 1-1100.	No taste.	No taste. Like water.	Slightly alkaline. A trifle like distilled water.	A taste, but I cannot tell what.
Distilled water.	No taste.	No taste. Like water.	I believe it is distilled water, though it seems to be alkaline.	No taste.
Salt, 1-1000.	No taste.	It is like water, but there is a taste.	I believe it is distilled water, although it seems to be alkaline.	Bitter.
Distilled water.	Water.	Same as preceding.	Distilled water.	Bitter and something more.
Salt, 1-900.	Slight taste, but do not know what it is.	Same taste as preceding, but stronger.	Distilled water.	No taste.
Salt, 1-800.	No taste.	Same as preceding.	A slight taste. Do not know what it is.	No taste.

TABLE B. Continued.

SOLUTION.	MRS. S.	MISS W.	MR. S.	MR. E.
Distilled water.	Water.	Same as preceding	No taste. Water	No taste.
Salt, 1-700.	Slightly alkaline.	A little stronger. Possibly some salt. Too faint.	Slightly alkaline, but may be distilled water.	No taste.
Salt, 1-600.	Some taste, but do not know what.	Same as preceding. Can't recognize the taste.	No taste. Distilled water.	May be salt (?)
Distilled water.	Water.	Same as preceding.	Distilled water	No taste. Might be bitter.
Salt, 1-500.	Alkaline.	Same as preceding, but stronger.	Seems slightly alkaline; may be distilled water	No taste.
Salt, 1-400.	Some taste, but do not know what.	Stronger than preceding. Possibly salt.	Slightly alkaline, I think.	Salt.
Salt, 1-300.	Alkaline.	Still stronger than preceding. Same taste.	Salty, *sure*.	Mixed taste. There is salt in it.
Distilled water.	Water.	Same as preceding.	Slightly alkaline. Probably distilled water.	May have some taste.
Salt, 1-200.	Salty.	Same as preceding, but stronger.	Salty, *sure*.	Salt.

observer, a suspicion of salt begins with the 1 to 700 solution. With a second it begins at 1 to 600. The third recognized it at 1 to 300, and the fourth at 1 to 200. But the alkaline taste, although it failed in my tests to appear at all with the sweet-salt solutions, appears now eight times with the pure salt solutions and twice with distilled water. It seems to me doubtful, therefore, although this single experiment of mine can have only a suggestive value, whether the alkaline taste is a mixed taste made up of salt and sweet as indicated by

Kiesow and as suggested by Wundt.[1] As regards the second question mentioned above, even if the mixture of salt and sweet should be found to produce a new neutral sensation, such as insipid or alkaline, it would seem that the experimental results of such mixtures or fusions have thus far been too meagre to encourage us to look in this direction for the source of the multiform "tastes" of common experience.

To gather up the results of this preliminary survey of the psychology of taste, I may say that the hypothesis which seems at present most in accord with known facts is that there are only four taste sensations (possibly only two); that these remain distinct in consciousness, not subject to fusion or mixture with each other; and that the manifold taste perceptions of daily experience are made up of these four taste sensations with their grades of intensity, and sensations of smell, touch, temperature, sight, and muscle sensations. The fine discriminations of foods and drinks, called "tastes," are due to the delicate sense of touch possessed by the tongue and (as has long been known) to smell. Of these two perhaps the touch sensations are more important, while sight sensations, as will appear below, play a more essential part than has commonly been supposed. The taste sensations themselves have a comparatively unimportant role so far as perception is concerned. They have little to do with discrimination. Their affective value, however, is great. Sweet things are "good" and bitter things are "bad." To take an illustration at random, honey and all the different kinds of syrups and molasses have only one taste, viz., sweet, and indeed to a child of six years they are all alike "good."[2] The peculiar "flavor" of the syrups and molasses is due in this case to smell. The pleasant maple "taste" of pure maple syrup is an odor. It is only necessary to close the nose and take some maple syrup upon the tongue, without swallowing, to show this. It is merely sweet and smooth and has a certain amount of stickiness and viscos-

[1] Outlines of Psychology, Eng. trans., p. 53.

[2] An exception may be found in New Orleans molasses which has a slight bitter taste together with the sweet.

ity. Swallow it or release the nose and the peculiar maple flavor comes out in a striking manner. To some extent, however, the various syrups and molasses may be distinguished without the sense of smell by touch and the muscle sense, owing to their different degrees of smoothness and viscosity. (See below, Table C, Sec. IX.) It is in this way probably that honey may be distinguished from the syrups. Its consistency and its absence of odor, rather than the intensity of its sweetness, are its marks. Without sight, however, as will be seen from Table C, honey is by no means easily distinguished from other sweets, especially if it is made as limpid as possible by heating.

The perpetual confusion of tastes and odors is much increased by the so-called gustatory smelling. Particles from the masticated food or from fluids taken into the mouth, ascend on either side of the soft palate through the pharynx and the posterior nares to the olfactory region, an action greatly facilitated, of course, in swallowing. By tightly closing the nostrils, the air currents are precluded, and in this way very satisfactory experiments may be instituted in eliminating smell sensations from taste perceptions. This, however, can best be attained in the case of anosmics.

The analysis of taste perceptions into their respective elements is a matter of considerable difficulty. Sight may be eliminated by blindfolding the observer, and smell by experimenting with an anosmic subject. In the experiments reported in this paper, I have in this way satisfactorily eliminated these two sets of elements. The elimination of touch and temperature sensations is more difficult. Cocaine removes sensibility to taste as well as touch.[1] Gymnemic acid which destroys for hours the sensibility to sweet and partly to bitter, might perhaps be used upon a blindfolded anosmic observer with interesting results. In the following experiments, the only attempt made to eliminate touch and temperature sensations was

[1] See Kiesow, *Ueber die Wirkung des Cocain, etc.*, Phil. Stud. (Wundt), IX. 4.

by presenting the substances in a uniform fluid condition of the same temperature. This, of course, was not always possible and when possible not always effective. With many substances a change of temperature is accompanied by a change of texture or viscosity, and the observer makes a conscious or unconscious allowance for such change. If for instance one attempts to prevent the identification by texture of butter or honey by heating them, the observer, perceiving the unusual temperature, makes allowance for the decreased viscosity. (See Table C, Sec. IX.)

Anosmia, or absence of the sense of smell, may be partial or complete. Cases of partial anosmia, like cases of color-blindness, are valuable for the aid they may give in the settlement of problems concerning sensations of smell. Cases of complete anosmia are valuable in separating sensations of smell proper from those caused by mere irritation of the nasal membranes, and particularly for separating sensations of taste from those of smell with which they are so often confused. Cases of complete anosmia are rare and have been little used for psychological purposes. One case has been reported by Jastrow[1] who made some interesting researches in connection therewith, and other instances have been reported in medical journals. A case of complete anosmia having come to my attention some months ago, a series of experiments was begun for the purpose of making some contribution to the problems before us. The observer, Mrs. S., is a very good one for such researches. She is a married woman, twenty-six years of age, of education and refinement. Having made formerly a special study of chemistry, she is familiar with the names and tastes of ordinary substances such as would be used in these researches. Being a housekeeper of experience, she is acquainted with the names and tastes of the materials of the common foods and drinks. According to her own testimony, she is almost entirely, if not entirely, devoid of the sense of smell and has always been so. She recalls that as a young girl she

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[1] American Journal of Psychology, Vol. IV. p. 407.

suffered disappointment because she could not smell the fragrance of flowers as the other girls said they did. Her anosmia may therefore be congenital, but her family history reveals no other defect of this kind and it is much more probable that the defect dates from a severe attack of scarlet fever which she had at the age of five. Rhinoscopic examination by a specialist revealed nothing abnormal in the appearance of the olfactory region. My own conclusion is that the case is one of complete anosmia of the intracranial kind. The completeness of the anosmia was, however, determined by the following experiments. The observer was tested with a large number of odorous substances representing all the nine classes mentioned by Zwaardemaker.[1] She was blindfolded and the odorous substances were held under the nose while she tried to detect the odor by sniffing. Care was taken to avoid fatigue by having many sittings and by repeating many of the substances upon different occasions. The following substances gave no sensation or reaction whatever: Absolute alcohol, methyl alcohol, butyric acid, acetone, orange, sweet orange peel, oil of lemon, oil of tansy, oil of spike, peppermint, spearmint, origanum, cardamon, anilin, oil of cloves, wintergreen, pennyroyal, lavender water, camphor, tincture of arnica, thyme, oil of cinnamon, oil of mustard, Java and Mocha coffee and black and green tea (these both powdered and in infusions), rosemary, cinnamon, mustard, mace, pepper, allspice, cloves, ginger, vinegar, thymol, oil of bitter almond, extracts of white rose, heliotrope, violet and verbena, vanillin, gum benzoin, turpentine, acetamid, musk, raw onion, asafoetida, javelle water, hydrochloric acid, tincture of iodine, valerianic acid, india rubber, iodoform, gum ammoniac, gum myrrh, carbolic acid, napthaline, vaseline, resorcin, benzol, benzine, naptha, Venice turpentine, diphenylamine, tobacco, tobacco smoke, burnt cheese, lactic acid, rancid lard, paregoric, laudanum, dried blood, decomposing animal matter. The following substances gave a reaction: Ammonia, ammonia sulphide,

[1] Physiologie des Geruchs, p. 216.

aromatic spirits of ammonia, acrolein, acetic acid, carbon disulphide, sulphurous oxide, ether, chloroform, bromine water, menthol, and pyridin. Oil of cloves upon second trial gave a faint sensation "far back in the throat." While at least some of the last named substances have odors, their characteristic reaction is not that of smell. They affect either the end organs of taste, being like chloroform very volatile and reaching the end organs of taste about the soft palate through the nose, or they are pungent and attack the membranous linings of the nasal cavities, stimulating in this way branches of the fifth nerve. For instance, menthol produced a sharp reaction with the observer, not being recognized, however. But menthol when held near the eyes produces a painful reaction. The action of ammonia and many of the ammonia compounds is similar, as is well known. Chloroform produced what the observer called a pleasant sweet sensation, felt in the back part of the mouth, which was of course its taste, it having a sweet taste. The character and reaction of ether is similar. Pyridin produced a marked and very unpleasant sensation located in the mouth. If held to the open mouth of the observer, the sensation was the same but stronger. Pyridin is a strong organic base very volatile and very miscible with water. The fumes of pyridin pass through the nose and diffuse over the tongue mixing with the fluids of the tongue and producing a true taste, combined probably with touch sensations. A normal observer may test this by holding a small bottle of pyridin under the tip of the tongue with the mouth closed tightly around the tongue and the nose closed. The unpleasant effect is very apparent. The substances mentioned in my second list may, then, be regarded as samples of a rather large class of volatile substances which give rise to touch and taste sensations commonly mistaken for odors.

This preliminary experiment was followed by others. I tested next the observer's sensibility to pure taste stimuli, using four normal women as control observers and comparing the results with such published records as we have concerning normal taste sensibility. Solutions of cane sugar, common

salt, tartaric acid, sulphuric acid, sulphate of quinine and strychnine were used. The following table exhibits a summary of these results:

Solution.	*Mrs. S.*	*Four Women.*	*Forty-six Women.* (Nichols & Bailey.)
Cane sugar	1—150	1—144	1—204
Salt	1—200	1—675	(1—1980)
Sulphuric acid	1—2000	1—2368	1—3280
Tartaric acid	1—1000	1—1500	
Quinine sulphate	1—160000	1—640000	1—456000
Strychnine	1—1000000	1—1000000	

From these experiments it appeared that my anosmic observer's sensibility to taste is slightly below the average sensibility of other women, although hardly enough tests upon normal women have been made for purposes of comparison. Certainly Mrs. S's sensibility to taste is not above the average, as would be expected if taste sensations play any important part in taste perceptions.

Experiments were made upon the observer's sensibility to touch and temperature stimuli. The pressure sense upon the fingers, hands, and face was found to be normal. Space discrimination upon the hands and fingers was also normal, as was the temperature sense. It was thought that the absence of the observer's sense of smell, since no superiority in the sense of taste was found, might find its compensation in an unusual fineness of the touch sense upon the tongue. A series of experiments was therefore made upon the active and passive touch sense of the tip of the tongue. It was possible to make the same tests upon only two other female observers and the results therefore can claim little value. They showed, however, a decided superiority on the part of the anosmic observer in respect to fineness of discrimination in passive touch, but no superiority in active touch.

The experiments in taste to which the above mentioned tests were all preliminary were begun in October, 1898, and continued for about eight weeks. About 200 substances, for the most part common foods and drinks, were tried with the anosmic observer and simultaneously with two (or three) con-

trol observers. Three married women, between twenty-five and thirty-five years of age, acted in the latter capacity, only two of them serving at a time. They were all very bright women of good education, and at the same time experienced housekeepers familiar with the taste and smell of all ordinary articles of food and drink and having a knowledge above the average of flavoring extracts, fruits, spices, etc. The observers met for the experiments from one to three times a week. The sittings lasted from one to two hours. During this time from fifteen to twenty substances were tasted. Although there were periods of rest during the experiment, it was at first thought that the last tests in each sitting would be unreliable owing to fatigue, and they were accordingly repeated at the next sitting. This was found to be unnecessary. The substances were so different in character and for the most part the tastes and odors were so mild, that there were no disturbing influences from fatigue. The method of procedure at each sitting was as follows: The three observers, the anosmic and the two normal observers, sat around a large table. They were carefully blindfolded and each provided with a silver teaspoon, a glass of lukewarm distilled water, and a lead pencil. After each taste the mouth was rinsed with the distilled water. There was also for each observer a beaker for spitting out the water or the substance tasted when the latter was necessary. Usnally, however, the substance was swallowed, as in this way the observer had the greatest possible advantage in tasting and smelling. Three sets of cards were prepared beforehand, one for each observer, with her name and the name of the substance to be tasted. An assistant handed the appropriate card to each observer, and after tasting she wrote upon it her judgment. She was required to name the substance if she could; if not, to name or describe the taste; and if she could not do this, to write whether it had any taste or not. The amount offered was from one-half to one teaspoonful. It was handed to the observer, who smelled it as much as she wished and then tasted it as carefully as she could. She was not required to take the whole amount, for it was found that with many

substances a small amount gave the best results. This could always be wisely left to the judgment of the taster. The observers were not allowed to talk during the experiments, nor to discuss the tastes afterwards, and were not told as to the accuracy of their judgments. As far as possible, and except where noted in the tables, the substances were presented at a like temperature, about 25° C. Exceptions to this rule are noted in the tables. In all cases where it was possible, the substance was presented in liquid form, or in both liquid and solid forms. The object of this was to reduce to a minimum the action of the muscle and touch sensations connected with the tongue. For instance, meat broths may be prepared having very nearly the consistency of water, and certain vegetables, such as cabbage and onion, may be separated from the water in which they are boiled and the latter used in taste experiments. These devices, however, apply to only a limited number of the substances tried and in many cases they are not wholly effective in eliminating the touch and muscle sensations.

In the table which follows, the judgments of Mrs. S., the anosmic observer, are for convenience of comparison printed in italics. The three control observers appear as Mrs. R., Mrs. K., and Mrs. L. On one occasion (See Section VI), neither Mrs. K. nor Mrs. R. being available, another woman, Mrs. O., was substituted. Mrs. O. is a housekeeper noted for her skill in cooking. On one other occasion (See Section V), there was an additional male observer, Mr. S., a married man 30 years of age, instructor in the University. In blindfolded tasting some proficiency is gained by practice, or at any rate, one or two hours' practice is necessary before the observer becomes accustomed to the conditions so as to give the best results. This may account for the somewhat unsatisfactory results exhibited in Section I, and it may also explain in part the rather wild judgments of Mrs. O. and Mr. S. upon the two occasions when they were introduced into the experiment. The effect of merely eliminating sight sensations in tasting is striking enough with Mrs. R., Mrs. K., and Mrs. L.; it becomes extreme

with Mrs. O. and Mr. S. Making some allowance for lack of practice in the case of these two observers, I am disposed to think that my three control observers, Mrs. R., Mrs. K., and Mrs. L., possess a power of discrimination in tasting somewhat above the average. The substances which appear in Sections I and II were nearly all obtained from a druggist, the fruit syrups, however, having been prepared from pure fresh fruits. The substances which appear in all the other Sections were procured by the writer either in the market or at his own home and were known to be pure. It may be added that in a few instances Mrs. S. was asked to say what her judgment was based on, the purpose being to ascertain whether she depended upon the taste or the texture of the material. This may explain the form of certain of the answers.

TABLE C. SECTION I.
October 26 and 29, 1898.

SUBSTANCE.	MRS. S.	MRS. R.	MRS. K.
Normal alcohol.	*Like maple sap.*	Like brandy or alcohol.	Alcohol.
Raspberry syrup.	*Honey.*	Raspberry.	Currant juice.
Distilled water.	*Water.*	Water.	Water.
Tincture of licorice.	*Licorice.*	Something like rhubarb or honey	Licorice.
Distilled water.	*Water.*	Like water.	No taste.
Peach syrup.	*Sugar syrup.*	Like syrup.	Quince jelly.
Normal alcohol.	*Like water.*	Do not know.	Alcohol.
Tincture of celery	*Alcohol.*	Bitter like quinine	Acid and nauseating.
Distilled water.	*Water.*	Like water.	Like thickened water.
Tincture of vanilla (with sugar).	*A sweet hot taste; I can think of nothing like it.*	Like vanilla.	Vanilla.
Strawberry syrup	*Sweet taste like honey.*	Like peach syrup.	Sugar syrup.
Pineapple syrup.	*Sweet.*	Like pineapple syrup.	Pineapple.

TABLE C. SECTION I. Continued.

SUBSTANCE.	MRS. S.	MRS. R.	MRS. K.
Distilled water.	*Water.*	Like water.	Water thickened.
Oil of rose and alcohol.	*Certainly one of the alcohol series, but do not know which*	A perfume. I can't tell.	Very bitter, but I do not know what it is.
Spirits of almond	*Some spirit or alcohol.*	Like almond extract.	Almond. Told by smell before tasting.
Spirits of Camphor.	*Camphor.*	Like camphor.	Camphor.
Spirits of wintergreen.	*Might be cinnamon oil.*	Like peppermint.	Wintergreen.
Distilled water.	*Water.*	Water.	Thickened water.
Tincture of ginger.	*Like red pepper.*	Quite bitter and spicy.	Pepper.
Spirits of peppermint.	*Peppermint.*	Like peppermint.	Peppermint.
Absolute alcohol.	*A tincture of something sweet and rather volatile.*	Like oil of catnip.	Something hot. Can't tell.
Asafœtida.	*A tincture of something. Might be cloves.*		
Spirits of cinnamon.	*Cinnamon oil.*	Like cinnamon extract.	Cinnamon.
Grape syrup.	*Like grape juice.*	Like strawberry flavoring.	Grape juice.
Ethyl alcohol $\frac{n}{4}$	*Water.*	Lemon.	Alcohol only.
Boiled turnip, mashed and salted.	*Only the flavoring, salt.*	Turnip.	Turnip.
Peach syrup.	*A very sweet taste, but nothing particular to distinguish it.*	Pineapple syrup.	Peach juice.
Rolled oats, porridge, salted.	*Oatmeal, by texture.*	Oatmeal.	Rolled oats.
Boiled potato, mashed.	*Potato.*	Potato.	Potatoes.
Strawberry syrup	*Like the sweet juice of some fruit, but do not know what.*	Syrup	Strawberry juice.

TAALE C. SECTION I. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.
Navy beans, baked and mashed.	*Beans.*	Beans.	Beans.
Cherry syrup.	*A very pleasant delicate taste, but do not know what it is. Sweet, might be fruit.*	Wild cherry syrup.	Almond syrup.
Raspberry syrup.	*A pleasant sweet taste, might be juice of some fruit*	Raspberry syrup.	Fruit juice. Guess apricot.
Distilled water.	*Water.*	Water.	Water.
Oil of catnip, with alcohol.	*Spirits of camphor, I think, but after a time the taste which should be present is absent.*	Bitter, lemon extract.	Don't know. Pennyroyal possibly.
Absolute alcohol.	*One of higher alcohols.*	Alcohol.	Alcohol mixture, can't tell.
Tincture rhubarb	*Bitter, but I cannot even guess what it is.*	Rhubarb.	Rhubarb.
Distilled water.	*Water.*	Water.	Water.
Spearmint, with sugar.	*Very pleasant sweet taste. Delicate enough for a perfume.*	Wintergreen.	Peppermint.
Oil of rose, with alcohol.	*Must be a tincture, but no taste except alcohol.*	Rose extract.	Rose flavoring.
Vanilla extract.	*Some familiar taste, pleasant and spicy, but cannot name it.*		

TABLE C. SECTION II.
November 8, 1898.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. K.
Raspberry syrup.	*Like raspberry juice, by flavor.*	Grape.	Raspberry syrup. Like New Orleans syrup.
Sherry.	*Like a light sour wine, though I never tasted any.*	Spirits, wine. Guess whiskey.	Sherry.
Lactic acid, $\frac{n}{4}$	*Like citric acid, but taste not persistent enough.*	Acid, can't tell.	Lemon and licorice water.
Distilled water.	*Water.*	Water.	Water.
Tincture rhubarb	*Bitter. Do not know anything with which to compare it.*	Rhubarb.	I have smelled this but never tasted it. Is it asafœtida?
Port wine.	*Do not know. Has a warming taste.*	Wine, guess sherry.	Port wine.
Oil of fennel with alcohol.	*A tincture of something that is pleasant. Must have a strong odor.*	Alcohol with familiar flavor. Guess caraway.	Mother Winslow's Soothing Syrup without much sugar.
Distilled water.	*Water.*	Water.	Water.
Vinegar.	*Acetic acid.*	Vinegar.	Vinegar.
Oil of caraway and alcohol.	*Alcohol at least. No other taste.*	Can't tell. Biting, contains alcohol.	Tincture of caraway.
Asafœtida.	*I do not know. It is bitter, probably a tincture.*		Something with onion in it.
Strawberry syrup	*Sweet, thick liquid. I think cane sugar*	Sugar syrup. Can't tell flavor.	Syrup.
Peach syrup.	*Cane sugar.*	Fruit juice. Can't distinguish.	Peach.
Spirits of peppermint.	*Peppermint.*	Peppermint in alcohol.	Peppermint.
Milk, 50% water.	*Like water with something dissolved, perhaps corn-starch.*	Milk and water.	Milk.
Juice of stewed apples with sugar.	*Peach juice.*	Juice of apple sauce.	Juice of apple sauce.

TABLE C. SECTION III.

November 11 and 12, 1898.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.
Ethyl alcohol $\frac{n}{2}$	*Water.*	Water. Slight taste of alcohol.	Don't know. Water, with dish-watery taste.
Distilled water.	*Water.*	Distilled water.	Water.
Baked squash, mashed.	*Very little taste. Must be some vegetable. Starch taste quite perceptible. From texture should think it might be a squash.*	Baked squash.	Baked squash.
Raw apple, scraped.	*Apple, both by taste and texture.*	Raw apple, scraped.	Apple, scraped.
Milk.	*Corn-starch dissolved in water.*	Milk and water.	Milk.
Beef broth, salted	*Like chicken broth.*	Beef broth.	Chicken soup or broth.
Banana, fresh, crushed.	*Sweet; should say some kind of fruit, but no decided taste.*	Banana, mashed.	Banana.
Distilled water.	*Water.*	Distilled water.	Water.
Canned tomato, cooked, strained.	*Tomato.*	Tomato, strained.	Acid, cannot tell what it is.
Valerianic acid, $\frac{n}{100}$	*Warm water.*	Liquid of little taste. Smells of nuts.	Water, with a mouldy taste.
Lard.	*Lard.*	Fat of boiled beef, or possibly lard.	Lard.
Date, small piece cut from common date.	*Dates.*	Dates.	Dates.
Chicken broth, salted.	*Chicken broth.*	Beef broth, or soup stock.	Chicken or beef broth.
Orange juice, from fresh fruit.	*A very pleasant sour taste, but I do not know what it is.*	Orange.	Orange.
Fig, separated from seeds.	*Prunes.*	Fig.	Fig.

TABLE C. SECTION III. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.
Port wine.	*Like weak whiskey.*	Spirits. Sherry?	Port wine. Is there any egg with it?
Distilled water.	*Water.*	Water.	Water.
Cheese, Club House.	*Cheese.*	ClubHouse cheese.	Cheese.
Kerosene.	*It is an oil, but has no taste.*	Kerosene, by smell and taste.	Kerosene oil.
Grape juice and sugar.	*Very pleasant taste. Some kind of fruit juice, but not recognized. Texture might indicate quince, but so sweet, flavor is not distinct.*	Grape juice.	Grape jelly.
Distilled water.	*Water.*	Distilled water.	Water.
Chocolate, boiled in water, unsweetened.	*Bitter and very unpleasant, but I do not know what it is.*	Water with cocoa, slightly greasy.	Chocolate.
Currant jam.	*Quince juice.*	Currant flavor, sugar syrup.	Apple and quince juice.
Cranberry jelly.	*Certainly same as before, which I thought might be quince, but too sweet to get flavor.*	Sugar syrup. Peach or apple flavor.	Fruit syrup, like preserved apple.
Milk.	*Starch water.*	Milk with water.	Milk.

TABLE C. SECTION IV.

November 16, 1898.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. R.
Sweet cream.	*Corn starch.*	Cream.	Slightly sweet. I cannot tell what it is.
Vaseline.	*Has no taste, but from texture I should think it might be cooked clear starch.*	Vaseline.	Like a salve.

TABLE C. SECTION IV. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. R.
Distilled water.	*Water.*	Distilled water.	Water.
Claret.	*Unpleasant sour taste, not acid but soured. Do not know what it is like.*	Claret.	Sour, like wine or claret.
Baked apple.	*Flavor of apple, but texture is not of cooked apple.*	Apple sauce.	Apple sauce.
White of hard-boiled egg.	*Very little taste, but by texture and the little taste there is, I conclude it is white of egg.*	Guess some kind of breakfast food.	White part of an egg.
Cabbage, boiled, the liquid only.	*Very unpleasant taste, but do not know with what to compare it.*	Cabbage flavor.	Sweet, tastes like turnip.
Water, undistilled.	*Water.*	Distilled water.	Water.
Preserved quince and apple, juice only.	*Just a sweet liquid with no distinguishing flavor.*	Quince juice.	Sweet, and has the flavor of apple.
Olive oil.	*Thick oil, but has no taste.*	Olive oil.	Some kind of oil; the kind used for sewing machines
Distilled water.	*Water.*	Distilled water.	Water.
Sour milk.	*Do not know; there is a slight taste of grain and sweet.*	Sour milk.	Milk, a little sour.
Cabbage, boiled, mashed, salted.	*Some vegetable that has been salted, but there is no other taste.*	Cabbage.	Turnips.
Orange juice, from fresh fruit.	*First sensation is of sweet, then of sour. Do not know what it is. Pleasant.*	Orange juice.	Juice of orange.
Yolk of hard-boiled egg.	*I do not know. It has no taste.*	Yolk of hard-boiled egg, told by texture entirely. What I called breakfast food before.	Like yolk of an egg.

TABLE C. SECTION IV. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. R.
Preserved quince, small piece.	*A very sweet fruit, but no distinct flavor.*	A piece of cooked quince.	It has the taste of quince.
Water, undistilled.	*Water.*	Distilled water.	Water.
Tincture of aloes with licorice.	*It is quinine, with something like licorice.*	A very bad taste, like bad medicine.	Dandelion extract.

TABLE C. SECTION V.

November 19, 1898.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.	MR. S.
Distilled water.	*Tepid water.*	Warm water.	Milk and water.	
Cabbage, boiled, the liquor only.	*Rather a sweetish taste, but do not know what it is.*	Cabbage flavor.	Know it, but can't name it.	Sour milk.
Beef broth, unsalted.	*A very unpleasant taste. Do not know what it is.*	Beef soup.	Beef extract and water.	Chicken broth.
Brandy, California.	*Vanilla.*	Whiskey.	Brandy.	Alcohol. It has an agreeable ethereal taste.
Distilled water, warm.	*Water.*	Water.	Milk and water, I think, not sure.	Water.
Pork broth, unsalted, (fresh pork).	*A flat washy taste, nothing decided.*	Beef flavor.	Beef soup.	Contains cabbage.
Navy beans, baked, thick gravy only.	*Oily taste. Do not know what it is like.*	Beans, baked.	Juice or gravy of baked beans.	Beans.
Malt extract.	*Slightly sharp taste at first, then a decided bitter, which persists.*	More like yeast than anything else.	Ale.	Yeast.
Baked sweet potato, scraped.	*Sweet potato.*	Sweet potato.	Sweet potato.	Sweet potato.

TABLE C. SECTION V. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.	MR. S.
Oysters, raw, the liquor only	*Do not know whether this has any taste or not, former one so distinct.*	Oyster flavor.	Oyster juice.	Oyster soup.
Red raspberry juice, sweet.	*Good, sweet grape juice.*	Raspberry, slightly mildewed.	Raspberry juice.	Juice of red raspberry.
Mutton broth, unsalted.	*Tastes flat, as if it might be good if seasoned.*	Beef, I think something richer but can't tell.	Mutton broth.	Broth of meat, and tastes like cabbage.
Veal broth, unsalted.	*Tastes flat, like those others.*	Beef flavor.	Beef or mutton broth, much diluted.	Chicken broth.
Oyster stew, the liquor only	*Oyster soup.*	Oyster flavor.	Liquid from raw oysters.	Oyster soup.
Pork broth, salted, (fresh pork).	*Beef tea.*	Beef extract.	Beef extract, weak.	Chicken broth.
Distilled water.	*Water.*	Water.	Water.	Water.
Veal broth, salted.	*Stronger beef tea.*	Salted beef extract.	Oyster soup, or a mixture of milk and beef extract.	Chicken broth tinge of cabbage.
Oyster stew, salted, the liquor only.	*Just like oyster soup.*	Oyster flavor.	Like the liquid of oysters, mixed with something unpleasant.	Carrot.
Beef broth, salted.	*Beef tea.*	Salt and beef extract.	Had this several times; not sure; meat broth.	Chicken broth.
Mutton broth, salted.	*Still stronger beef tea.*	Salty mixture, beef extract.	Meat broth, same as last.	Like chicken broth.
Roast duck, chopped fine.	*Beef, cooked.*	Small bits of chicken meat.	Duck.	Chopped chicken.
Boiled mutton, chopped fine.	*Veal.*	Mutton in small pieces.	Meat, but insipid as if soaked in water.	Chopped chicken, I believe.
Fresh pork, boiled, chopped fine.	*Pork.*	Beef.	Boiled beef, I believe, not sure.	Chopped beef, possibly by texture.

TABLE C. SECTION V. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.	MR. S.
Boiled beef, chopped fine.	*By texture I know it to be a meat of some kind. Should think it very young beef, or veal.*	Beef.	Boiled beef.	Beef; these tastes are alike to me.
Boiled veal, chopped fine.	*Beef.*	Beef.	Same as last two, boiled beef.	Beef, by texture.

TABLE C. SECTION VI.

November 25, 1898.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. O.
Distilled water, salted, warm.	*No taste, but it is not water.*	Like water in which matches have been soaked	Not much taste, slightly salty.
Raw potato, chopped fine.	*I do not know; seems like cabbage but is too fine grained.*	Raw potatoes; I think some dust in it.	Something like acorns.
Boiled pumpkin, strained.	*It has very little taste, but seems a little like squash.*	Pumpkin.	Something sweet, also flat.
Fresh pear, chopped fine.	*Pears, both by taste and the small granules.*	Pear, raw.	Sweet berry, a little fermented.
Liebig's extract of beef.	*Chicken broth.*	Meat broth, can't tell what.	Bouillon, a little salty.
Distilled water, warm.	*Warm water.*	Lukewarm water.	Least bit sour.
Roast pork, chopped fine.	*Turkey.*	Turkey, I think, but there was a fragment of bone, so am not quite so sure.	Boiled beef, salted, well-boiled with taste nearly gone.
Raw turnip, chopped fine.	*Cabbage, raw.*	Cabbage, raw.	Raw cabbage-heart, a little sweet.

TABLE C. SECTION VI. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. O.
Raw apple, chopped fine.	*Raw apple, both by taste and texture.*	Raw apple.	Grape juice, a little fermented. Don't recognize the other ingredients.
Roast turkey, dark meat. (There was onion in the dressing).	*It is meat, but has no distinguishing taste.*	Turkey, slight taste of onions; think the other may have been pork, (referring to roast pork, see above).	Boiled beef, mixed with a little onion.
Distilled water, salted.	*Like slightly salted water, might be a very weak soup, salt is the only distinct taste.*	Milk and water, with a little butter and salt.	A little salt water.
Malt extract.	*A disagreeable, bitter taste, but do not know with what to compare it.*	Fermented cider, I think, but it may be ale or porter.	Bitter, cherry wine or phosphate.
Preserved strawberries, juice only.	*It has a pleasant fruit taste, but I do not know what it is.*	Preserved strawberry juice.	Strawberry preserves.
Distilled water, warm.	*Water.*	Water.	Water.
Cranberry sauce, juice only.	*Cranberries, entirely by taste.*	Cranberries.	Sweetened orange juice.
Turkey, light meat, chopped fine.	*Turkey. This taste seems quite distinct.*	I can't tell. Slight taste of onion or something like it. Meat.	Boiled veal, salted.
Cocoa in milk, (as usually prepared, unsweetened).	*It has a slight bitter taste, which resembles coffee without sugar.*	Chocolate or cocoa in water, but it doesn't taste right.	Chocolate or cocoa, sweetened.
Celery, chopped fine.	*I think we had it before [turnip?], and I rather think I judge more by the texture than by taste. Yet I think there is some.*	Celery.	This is something like the second article we had. [Raw potato]. Cannot tell what it is unless it is tasteless celery.

TABLE C. SECTION VI. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. O.
Horse-radish, chopped fine. (The chopping removes to some extent the sharp, biting effect).	*It has a pleasant sweet taste before chewing, which brings out a sharp stinging taste. Do not know what it is. Might be mild horse-radish*	Horse-radish.	I do not know; never tasted anything like it.
Raw onion, chopped.	*A bitter sharp taste Do not know what it is.*	Onion.	Onion and something bitter.

TABLE C. SECTION VII.

November 26 and December 2, 1898.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Venison, fried and chopped fine.	*It is meat, tastes like beef.*	Beef.	Beefsteak, chopped.
Veal, fried and chopped fine.	*It is meat, but do not know what kind.*	Meat. I can't tell what kind; richer than the other.	Some kind of meat, veal.
Roast turkey, chopped fine.	*Turkey.*	Turkey or chicken	Chopped, cold turkey.
Turnip, boiled and mashed.	*Pumpkin. Has no taste. Judge by texture.*	Turnip, I think, but the texture doesn't seem quite right.	Mashed turnip.
Distilled water, warm.	*It has no taste.*	Water.	Like water.
Peanuts, roasted, chopped fine.	*Peanut. The taste is quite distinct. Knew it was a nut by texture.*	Roasted peanuts.	Roasted peanuts.
Almonds, chopped fine.	*Hickory nuts, but taste is not so distinct as peanut.*	Almonds.	I think it is chestnuts.
English walnuts, chopped fine.	*English walnuts. The slight bitter taste is quite distinct.*	Hickory nuts.	Like hickory nuts.

TABLE C. SECTION VII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Yolk of raw egg.	*Melted butter.*	Egg.	Rather a sweetish taste, very much like yolk of an egg, uncooked.
Cabbage, raw, chopped fine.	*It certainly tastes like cabbage.*	Cabbage.	Cabbage.
Coffee, black.	*Coffee, entirely by taste which is bitter.*	Coffee.	Coffee.
Cheese, New York.	*Cheese.*	Cheese.	Cheese.
Vanilla extract, somewhat dilute.	*Tea, weak.*	Water with a very little vanilla flavoring.	Vanilla flavoring.
White of raw egg.	*Some oily fluid, might be oil from a cooked fowl.*	Egg,—yolk and white beaten together.	White part of egg, raw.
Boiled onion, liquid only, unsalted.	*An unpleasant sweetish taste. I do not know what it is.*	Don't know what it is. Slight taste of onion.	Sweet taste. It may be onion.
Coffee, with cream and sugar.	*Coffee, with both cream and sugar.*	Coffee, with milk and sugar.	Sweetened coffee.
Almond flavoring extract.	*It tastes like mountain tea, just a little.*	Vanilla. The time I said vanilla before I was wrong.	Almond flavoring.
Onion, boiled, with milk and salt.	*I rather think it is some vegetable cooked in cream, but it is not familiar to me; unpleasant, sweet taste.*	Boiled onion.	A sweet taste. I think cooked onion.
Tincture of ginger.	*Very sharp burning taste, like red pepper.*	Ginger, I think Jamaica.	Ginger and alcohol.
Venison, fried, small square piece.	*Beefsteak.*	Beefsteak.	Beef.
Veal, fried, small square piece.	*Veal. Texture is finer than the beef and taste seems more delicate.*	Boiled beef.	Porterhouse steak.

TABLE C. SECTION VII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Turkey, roasted, small square piece.	*Turkey. A part of the breast; later I guessed because one side was perfectly smooth.*	Turkey.	Turkey or chicken.
Fresh butter, unsalted, partly melted.	*Some kind of oil, but it has no taste.*	Something oily, like thick cream.	Butter without salt.
Cornstarch, cooked with milk.	*Like cornstarch, cooked without any sugar.*	Like cornstarch, not well cooked.	Thick, like cornstarch.
Tapioca, cooked in water.	*Clear starch.*	Tapioca.	No taste. Thick.
Buttermilk, strained.	*Like cornstarch dissolved in water.*	Milk, slightly sour	Sour milk.
Boiled rice, mashed.	*Rice. A distinct starchy taste, but I am sure the texture assists some.*	Rice.	Ground rice.
Graham bread.	*I think it is bread, perhaps whole wheat.*	Bread, white, a little something sweet on it.	Brown bread, or whole wheat flour.
White bread.	*Bread, made from white flour.*	Whole wheat bread.	White bread.
Boston brown bread.	*Brown bread, Boston baked.*	Corn meal cake.	Brown bread, a little taste of graham.
Whole wheat bread.	*White bread.*	Bread, heavier than white, but not like whole wheat.	Some kind of bread.
Rye bread.	*Rye bread.*	Bread, finer texture; white, a little like salt-rising bread.	White flour bread.
White bread, spread with salted lard.	*White bread with butter.*	Bread with lard.	White bread, buttered.
Lemon juice.	*Citric acid.*	Lemon juice or citric acid.	Lemon juice.
Fresh butter, unsalted, hard.	*Some kind of fat, but it has no taste.*	Like thick cream; seems a little too oily.	Fresh butter.

TABLE C. SECTION VII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Tea, ordinary infusion.	*Water.*	Tea.	Tea.
Sodium carbonate, 1¼% solution.	*Somewhat familiar but not known. A little taste of soda. There is a slight alkaline effect on the flesh.*	Solution of soap. Lye.	Lime water.
Cherries, canned, juice only, rather sour.	*Some kind of fruit juice. Might be cherry.*	Cherry juice.	Cherry juice.
Coffee, ordinary decoction.	*Slightly bitter, but I do not know what it is.*	Coffee.	Coffee.
Currant jelly.	*Jelly; do not know what kind.*	Currant jelly.	Jelly, grape.
Maple syrup.	*Maple syrup.*	Maple syrup.	Maple syrup.
New Orleans molasses.	*New Orleans molasses.*	Molasses. I think not New Orleans but I don't know what to call it.	New Orleans molasses.
Sorghum.	*Sorghum.*	New Orleans molasses.	Molasses, I do not know what kind.
Syrup, "Honey Drop."	*I do not know. It tastes like soft butter scotch.*	Molasses, with a taste of peanuts like melted peanut candy.	Sugar syrup.
Honey.	*Honey.*	Honey.	Honey.

TABLE C. SECTION VIII.

December 3, 1898.

SUBSTANCE.	MRS. S.	MRS L.	MRS. K.
Mace. (This and the following were all pure ground spices).	*Nutmeg.*	Nutmeg.	Nutmeg.
Allspice.	*Some spice, may possibly be allspice.*	Cloves.	Cloves.
Cinnamon.	*Cinnamon.*	Cinnamon.	Cinnamon.

TABLE C. SECTION VIII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. K.
Cloves.	*Cloves.*	Cloves, the other was allspice.	Cloves. Allow me to change other to allspice.
Ginger.	*Certainly is pepper.*	Ginger.	Ginger.
Mustard.	*Seems something like mustard, but not strong enough*	Mustard.	Mustard.
Black pepper.	*Red pepper.*	Black pepper.	Pepper.

The following additional substances were tried at a later date and with different control observers for the purpose of determining the effect of heating the substances and so, by reducing them to a like consistency, eliminating to some extent the sense of touch. Only indifferent success attended this effort for the reason mentioned above, p. 99. The syrups and molasses were heated to a temperature of 80° C. but before they could be tasted from a spoon had fallen to about 60°.

TABLE C. SECTION IX.
April 11, 1899.

SUBSTANCE.	MRS. S.	MISS. W.	MR. R.
Sorghum.	*Sorghum molasses.*	Hot molasses, sugar cane; recognized by smell before tasting.	A peculiar, familiar sweet taste. Not a perfect maple taste.
New Orleans molasses.	*New Orleans molasses.*	Molasses with something else added. Can't tell what.	Sorghum.
Sorghum.	*New Orleans.*	Just simple molasses, or sorghum.	A syrup resembling the sugar beet. Less sharp.
Syrup, "Honey Drop."	*Syrup. Do not know what kind. Peculiar velvety texture.*	Sorghum and syrup.	Seems like honey.
Maple syrup.	*Cane-sugar syrup.*	Maple syrup.	Maple syrup.

TABLE C. SECTION IX. Continued.

SUBSTANCE.	MRS. S.	MISS W.	MR. R.
Light brown sugar, melted.	*It is syrup with a slight maple taste, but not strong.*	Syrup, made from brown sugar.	Resembles New Orleans syrup.
Dark brown sugar, melted.	*It is maple, but not strong.*	Syrup, made from brown sugar.	Syrup. Like New Orleans. Possibly trace of maple.
Maple syrup.	*Maple syrup.*	Maple syrup.	It contains maple syrup.
Honey, strained.	*Honey. Recognized by taste and smooth texture.*	Syrup made from granulated sugar.	Honey, sure. Possibly I was wrong before.
Maple sugar, melted.	*Maple syrup.*	Melted maple sugar.	Maple syrup.
Brown sugar, dried and pulverized.	*Brown sugar.*	Sugar, known as C. sugar.	The soft brown sugar.
White sugar, pulverized.	*Cane sugar, both by taste and texture.*	Granulated and powdered sugar mixed.	Soft brown sugar.
Maple sugar, dried and pulverized.	*Maple sugar, by the taste.*	Powdered maple sugar.	Maple sugar.

In discussing the results of the experiments as shown in the tables, we might say that in theory those substances which are not recognized by any of the observers depend for their recognition upon sensations of sight; that those substances recognized by the normal observers, but not by the anosmic, depend upon sensations of smell for their recognition, while those recognized by all the observers depend upon either taste, touch, or muscle sensations. We might indeed go one step farther and say, since Mrs. S. did not possess any superiority in the sense of taste, that those substances recognized by her and not by the other observers, depend for their recognition upon touch or muscle sensations. It is obvious, however, that experiments of this kind cannot have that degree of exactness which would justify such complete generalizations. Sugges-

tion plays an important part in taste and a few of the correct judgments were evidently the results of happy guessing. Making due allowances for these facts, the following lists have a considerable value as indicative of the part played by the several kinds of sensations in taste perceptions. The most interesting list, though perhaps not the most instructive, comprises those substances recognized by the two normal observers, but not by the anosmic. The characteristic "taste" of these substances is presumably their odor. They are as follows, omitting those made doubtful by giving different results upon different trials: Tincture vanilla, vanilla extract, spirits of almond, pineapple syrup, orange, lemon, banana, grape, quince, strawberry, fig, tea, cocoa, chocolate, milk, sour milk, vinegar, claret, oil of rose, rhubarb, onion, boiled turnip, navy beans, (liquid form,) liquor of raw oysters, yolk of egg, white of egg, and kerosene. This list, however, should be supplemented by the second list including those substances recognized by one of the normal observers but not by the anosmic. No doubt, concerning these the same conclusion should be drawn. They are as follows: Peach syrup, currant jelly, wintergreen, port wine, sherry wine, brandy, unsalted butter, cream, olive oil, vaseline, cabbage, pumpkin, raw potato, beef broth, mutton, and mutton broth. These lists, it should be observed, do not include all the substances, even of those tried in my experiments whose characteristic "taste" is really an odor, for some things, such for instance as pear, apple, cheese, and the different kinds of syrups and molasses have also a sufficiently characteristic texture or consistency to be recognized by the anosmic observer. Raspberry and cherry and the fruits in general should be included in these lists, although upon a single occasion these two were correctly named by the anosmic observer. Raspberry juice is sweet and moderately sour and slightly viscous and it is not strange that it should be correctly named once out of three trials without its characteristic odor.

The importance of sight sensations in taste is seen in the list of substances which, while usually recognized in daily experience, failed of recognition either by one or both of the

normal observers when blindfolded. Most conspicuous among these are the various kinds of meats and breads. They have no taste except that of the salt with which they are prepared or possibly a very faint sweet, while their characteristic odors are too faint to serve as marks of recognition. It should be noticed that the unsalted meat broths could not be recognized even as meat broths by the anosmic observer, much less distinguished, but as soon as they were presented with salt they were recognized as meats but not differentiated. There was a tendency on the part of all the observers to call all the meat broths, beef or chicken, where the effect of suggestion is apparent. With the chopped meats, or even with the larger pieces, the case is not much better. Chicken, turkey, and pork have in common a characteristic tenderness which assists in their identification by the muscle sense, but they are not easily distinguished from each other. It is not likely that the turkey would have been named at all by any of the observers in the experiments except that it was presented immediately after Thanksgiving, and furthermore smelled unfortunately of the onion in the dressing. The anosmic observer recognized it on two occasions and in one case explained that the recognition was due to the fact that the piece was very smooth on one side and was inferred to be a portion of the breast. The anosmic observer succeeded better than any of the others in identifying the different kinds of bread, a fact due to their differences of texture. One of the observers correctly named roast duck, but she had eaten this the day before. Among other things in the "taste" of which sight sensations have an important part, are butter, cream, olive oil, and various fruits and vegetables. It is noteworthy that Mrs. O., a housekeeper of long experience, when blindfolded failed to recognize raw turnip, raw potato, boiled pumpkin, cranberry sauce and fresh pear. If turkey, for instance, has no taste and very little or no characteristic odor, it may be asked why it is that it is so much prized. This is a problem for some student of the psychology of the emotions to work out. But of course turkey does have a "taste." Its "taste" is made up of a nicely

adjusted set of relations between a number of sensations, of which the most important are muscle, touch, and sight sensations, with a faint odor and perhaps a faint sweet taste and the indispensable taste of the accompanying salt. If it is asked why turkey is so commonly considered to be better than chicken, we must perhaps be satisfied with the prosaic answer,—because there is more of it. If it is still further asked, why, then, is quail better than either turkey or chicken, we may perhaps answer,—because there is less of it, that is, it is rare and hard to get. Deep rooted associations connected with hunting and the chase may have much to do with our appreciation of certain kinds of meat, and to appreciate them fully we must *see* them, or at least be told what they are.

If now we consider the results of the experiments in their relation to the problem of taste sensations themselves, we are met by the difficulty that the experiments afford no means of separating sensations of taste from those of touch and the muscle sense. We may however inquire whether the results tend to weaken or confirm the hypothesis which we have made above, namely, that there are only four taste sensations, that these play a very unimportant part in the discrimination of foods and drinks, and that they are not fused or united to form the taste perceptions of common experience. It has already appeared that sight sensations and particularly smell sensations make up an important part of our taste perceptions, that they are indeed the essential elements in the "taste" of our most common foods and drinks—such as fruit, meats, bread, butter, tea, coffee, cocoa, wines, etc., which are among the things that *seem* to have the most decided and characteristic taste. It is at least conceivable that the remaining substances which can be recognized without the aid of sight or smell, may be recognized by the delicate sense of touch possessed by the tongue, and by the muscles used in chewing, together of course with the help which may be given by taste sensations of salt, sweet, sour, and bitter. About forty substances altogether were correctly named by Mrs. S. As we read this list we notice that a large percentage of the substances, if not all of

them, have a characteristic texture, or consistency, or else a characteristic biting or astringent effect or pungency. With a few exceptions they are the substances which could not be presented in liquid form. The list is as follows: Camphor, peppermint, cinnamon, allspice, pepper, mustard, cloves, mace, oatmeal, potato, beans, squash, raw apple, baked apple, lard, date, cheese, sweet potato, oyster stew, celery, peanuts, cabbage, cornstarch, rice, cherry juice, grape juice, licorice, honey, tomato, chicken broth, white of boiled egg, beef broth, pork, turkey, pear, cranberry, horse radish, English walnuts, white bread, brown bread, rye bread, coffee with cream and sugar, maple syrup, sorghum, New Orleans molasses. Some of the substances mentioned in this list were tried more than once, and with other trials failed of recognition. Maple syrup was correctly named three times but failed of recognition once, while melted brown sugar was called maple twice. A few of the judgments were no doubt good guesses, especially perhaps those of the sweet and sour substances, like the few fruits that were correctly named. The majority, however, were probably recognized by the muscle and touch sensations. In many cases, indeed, this was admitted by the observer to be the means of recognition. Cheese could be easily recognized without taste, smell, or sight by its biting effect together with its characteristic consistency. Pear is sweet, sour, and granular; cranberry, sweet, sour, and astringent. The chopped nuts would be revealed by their texture and the Eng'ish walnuts differentiated by their oiliness. The texture of horseradish together with its pungency would be its sufficient marks. A mildly bitter limpid fluid with a slight sweetness and an oily feeling would probably be coffee with cream and sugar. Oatmeal, potato, squash, lard, rice, cornstarch, etc., all have their characteristic textures. The syrups and molasses could be recognized by their sweetness and differentiated by their different consistencies. Some of the answers which Mrs. S. gave when she failed to name the substances are instructive in their bearing upon this subject. Orange juice (See Section IV) was said to taste first sweet and then

sour. Preserved strawberries (See Section VI) were said to have "a pleasant fruit taste," the marks of the "fruit taste" no doubt being a slight viscosity accompanied by sweet and sour, the predominance of the sweet making it "pleasant."

On the whole the experiments confirm, so far as they go, the hypothesis made in this article, and, while not diminishing the importance which has been given to sensations of smell in the "tastes" of common experience, they indicate that touch and muscle sensations play an unexpectedly important part.

A special supplementary series of tests was made upon the taste of coffee and tea. These are not reported in detail because they merely confirm what is already well known, though not always clearly comprehended, about these tastes. Strong and weak decoctions of Mocha and of Java coffee and strong and weak infusions of Gunpowder and of Oolong tea were tried with normal observers blindfolded and with closed nose. Distilled water of the same temperature was used as a control substance. The same preparations together with very dilute solutions of sulphate of quinine were tried with the anosmic observer. Tea and coffee in finely powdered form were also used in the tests. Briefly stated the conclusions are that coffee and tea have a bitter taste, not to be distinguished from the bitter of quinine or any other bitter substance when the intensity is the same. Weak coffee and tea are, without the sense of smell, often confused with water of the same temperature, and if strong are confused with bitter substances. Apart from their odor, tea and coffee with cream and sugar can scarcely, if at all, be distinguished from each other, but if clear and of medium strength they may be distinguished by the slight astringency possessed by the tea.[1] The latter

[1]One of the experiments which I tried with tea and coffee with Mrs. S. may be mentioned. I prepared ten teacups and into each I put a tablespoonful of rich cream and a teaspoonful of sugar. The cups were then filled with the following preparations and kept during the tests at a temperature of 55 C. which is about the temperature of coffee as it is drunk at the breakfast table. No. 1 was filled with water; No. 2, with a solution of sulphate of quinine in the proportion of 1 part quinine to 20,000 parts water; No. 3, with sulphate of quinine, 1 to 10,000; No. 4, with coffee

is of course due to the excess of tannin possessed by the tea, while the bitter taste of tea and coffee is due to the alkaloids, theine and caffeine, and the characteristic aroma to the volatile oils which they contain. To the bitter theine and caffeine belong the peculiar dietetic and stimulating effects of tea and coffee.

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as ordinarily prepared, using 1 tablespoonful of ground coffee to 1 cup of water; No. 5, with tea, ¼ teaspoonful black tea to 1 cup of water; No. 6, with quinine, 1 to 5,000; No. 7, with coffee, ½ tablespoonful to 1 cup of water; No. 8, with tea, ½ teaspoonful to 1 cup water; No. 9, with tea, 1 spoonful to 1 cup of water, but without sugar; No. 10, same as No. 9, but with the cream and sugar. Mrs. S. was then blindfolded and the cups were brought to her one at a time and she tasted each mixture directly from the cup, taking whatever amount she wished, rinsing the mouth with lukewarm water after each test and writing her judgments upon prepared slips. The judgments were as follows: No. 1, (water): "Tastes like weak tea with cream and sugar" No. 2, (quinine): "Coffee." No. 3, (quinine, stronger): "Tastes bitter, like poor coffee." No. 4, (coffee): "Coffee with some cream and sugar." No. 5, (tea): "It has a sweet creamy taste; do not know what it is; do not like it." No. 6, (quinine, stronger): "Very bitter; do not think it is coffee; might be a grain drink." No. 7, (coffee, weaker): "Hot water, with cream and sugar." No. 8, tea (stronger): "I do not know what it is; tastes sweet." No. 2, (quinine, repeated): "Coffee." "I think it is weak but of good grade." (The latter in answer to an inquiry about the quality of the "coffee.") No. 5, (tea, repeated): "Milk." No. 9, (tea, strong, with cream only): "Coffee, with cream; do not quite like the flavor." No. 10, (tea, strong, with cream and sugar): "I think it is coffee with sugar. I do not know whether it is good grade or not." It may be added that the observer is a habitual user of coffee of which she is fond, and which she takes with cream and sugar. She does not use tea. Quinine is a drug which she says is peculiarly distasteful to her.

Some Peculiarities of the Secondary Personality.[1]

BY

PROFESSOR G. T. W. PATRICK.

Of the many unsolved problems in psychology, that of automatism is perhaps the most baffling. Automatic utterances, whether in the form of writing or the speech of the so-called trance-medium, present certain peculiarities which distinguish them so clearly from the utterances of normal subjects as to require some special explanation. Other abnormal mental conditions, such as mania, melancholia, hypnosis, or hallucinations, present peculiarities each of its own kind, but these are by no means so puzzling as those of automatism. If not at present fully explained, we believe that they may be eventually understood as exaggerations or perversions of normal forms of mental life. In automatism, however, we are apparently confronted with phenomena of a different kind. They belong to that class which the scientist of the day would call 'remarkable,' demanding instant attention and careful verification, and requiring if they persist some special explanation. Indeed the extremely striking character of some of the phenomena of automatism may be illustrated by the nature of the hypotheses that have been made to explain them. I have in mind, in particular, one series of automatic utterances which have been under investigation for nearly fourteen years by psychologists trained in scientific methods, and at the end of this time one of these psychologists, who has been most intimately connected with the investigation,

[1] Reprinted from The Psychological Review, Vol. V., No. 6, November, 1898.

reckoned to be a man of sanity and careful logical habits, has proposed, as the only hypothesis capable of explaining the facts, that the person from whom the utterances come is 'controlled' by one or more disembodied 'spirits' of the deceased.[1]

Such a hypothesis violates almost all the conditions to which a legitimate hypothesis should conform. It does not connect the phenomena in question with any other known facts or laws. Proposing as the basis of explanation certain wholly unknown forms of being, it admits of no deductive inference of consequences. It cannot, furthermore, be clearly and definitely conceived, and does not, finally, explain all the facts. I mention this merely to illustrate the straits which psychologists are in to explain the phenomena of automatism. The peculiarity of the situation is not greatly lessened when we learn that other psychologists maintain in all seriousness that, without recourse to the 'spirit' hypothesis, the phenomena may all be explained by 'telepathy'—a doctrine itself of questionable antecedents.

Under these circumstances, what should be the attitude of psychologists towards automatism? No one can doubt the answer which every scientist would make to this question. We want more facts and more hypotheses—especially facts. While this is the true attitude, unfortunately it is not the one which has usually been held. Too many have treated the whole subject with neglect if not with actual contempt. This wholly unscientific attitude has been the result of no real want of the spirit of investigation on the part of psychologists; it has been due rather to the frowns of the science world in general, and this again is explained if not excused by the unhappy history of the phenomena of automatism. These bear, at least in England and America, the corrupting marks of evil associations. They suggest all sorts of charlatanry and superstition. It has been felt that to maintain the dignity of experimental psychology, this subject and certain

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[1] RICHARD HODGSON, LL.D. *A Further Record of the Observations of Certain Phenomena of Trance.* Proceedings of the Society for Psychical Research, February, 1898.

related ones must be ignored. They have been almost uniformly kept out of American psychological laboratories, where infinite labor has been spent upon other probably less fruitful problems. But experimental psychology has now long passed its probationary period and may quite freely choose its subjects for research, and at present there is perhaps no other subject promising to throw more light upon certain dark chapters in mental science than that of automatism. No one can read the reviews that have appeared of Dr. Hodgson's report upon the trance-utterances above mentioned, indicating the self-confessed confusion of those most intimate with the case, coupled with a half readiness to accept almost meaningless explanations, without feeling the urgent need of a wider acquaintance with related facts. To be sure, the psychical research societies and a number of distinguished and devoted French investigators have been for some years assiduously cultivating this field. Whether we judge the results of their labors, however, by the conclusion lately arrived at by Dr. Hodgson, or by the excellent summary contained in Mr. Podmore's recent Studies in Psychical Research, the conviction is strengthened anew that we want more facts and more explanations. Without denying its great debt to psychical research, experimental psychology may profitably take up the problem of automatism, apply still more rigorous methods, and, what is of greater importance, include in its investigations a larger number of more simple cases. In the psychical research literature, one is wearied by the perpetual recurrence of a few remarkable 'classical' cases. It would be desirable to have fresh cases, a good many of them, and they should be simple ones. The cases reported hitherto have been too complex and remarkable, or rather the examination has not included a sufficient number of the less complex and less remarkable. The primary object of the present paper is to urge the extension of experimental work in this direction. The thorough study of simple cases of automatic writing and of all forms of automatism in normal healthy subjects is wholly practicable in the laboratory and certainly desirable.

Encouraging beginnings have been made in American laboratories by Solomons and Stein[1] in two researches upon normal motor automatisms, and by Jastrow[2] and Tucker[3] in researches upon involuntary movements.

About three years ago I undertook, as a contribution to this subject, to make a study of a simple case of automatic writing. Owing to the absence of the 'subject' from the city for two years, the study was only recently completed. I present it now rather as an indirect means of furthering my object above mentioned than as a study possessing any intrinsic value in itself. For this reason I add certain details of procedure, which, while familiar to every 'psychic researcher,' may perhaps be useful to the larger body of investigators which I conceive to be demanded by the importance of the problem. I wish also to use the occasion to call attention to certain peculiarities of the secondary personality appearing in this and in other cases, and incidentally to notice their relation to certain hypotheses that have been made to explain them. I shall, therefore, rather freely preface the account itself with some general remarks and some mention of other experiments that I have made. I use the term 'secondary personality' advisedly, finding it preferable to secondary consciousness, or subliminal or subconscious personality, or any other phrase, as it is justified by the facts, and is in harmony with any, even the 'spirit,' hypothesis. In automatic writing, for instance, we find ourselves in communication with a source of intelligence that hears and answers questions, reasons, exhibits pleasure and anger, assumes a name which it retains from day to day and from year to year, and displays an accurate memory extending over long intervals of time. To such a source of intelligence we cannot refuse the name of personality. When in connection with the same physical organism we find a synchronous or alternating intelligence, exhibiting different mental peculiarities, having a different

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1 Psychological Review, Vol. III., p. 492. Ibid., May, 1898.

2 American Journal of Psychology, Vol IV., p. 398.

3 American Journal of Psychology, Vol. VIII., p. 394.

name and displaying a different set of memories, we find it not only convenient but suitable to speak of a primary and a secondary personality. This secondary personality may be an apperceptive unity corresponding to a special grouping of association tracts in the subject's brain, it may be some lower mental stratum belonging to a sort of universalized psychic faculty, or it may be the 'spirit' of my deceased grandfather; it may or it may not be subliminal; it is even conceivable that it should not be conscious, but it bears all the common marks of personality.

Thus far the problem presents no very serious difficulty. The mere fact that there should be in connection with the same organism two personalities is not more wonderful than that there should be one. There is nothing in our present knowledge of the ego either from the psychological or physiological standpoint preventing us from admitting that the elements which usually join in a single group may, under certain conditions, so associate themselves as to form two or three or any number of different groups, nor indeed that the same elements, as for instance memory images, may at once form a part of both or of several systems. Furthermore, there is another circumstance which would seem to make the scientific study of the secondary personality at least possible. It has certain pretty clearly defined marks, traits, or peculiarities, capable of logical description. The presence of these traits in all the cases of automatism which have been reported forces upon us the conviction that they all belong to the same general class and that the investigation of the simpler cases may throw much light upon the more complex ones. If we compare a simple case of automatic writing, such as may be found in one of almost any company of schoolgirls, with the wonderful case reported by Dr. Hodgson, the difference is as great as between a kitten and a tiger, but perhaps not greater, for a careful observer will discover 'marks' which indisputably place them in the same genus. What we need now is a more complete description of these marks. Besides the case presented below, I have recently had opportunity of studying two other

cases of automatism, both instructive, neither of them very remarkable, and in all of them I have been impressed by the presence of the usual marks, for instance, suggestibility, fluency, absence of reasoning power, exalted or heightened memory, exalted power of constructive imagination, a tendency to vulgarity or mild profanity, the profession of 'spirit' identity and of supernatural knowledge, and finally a certain faculty of lucky or supernormal perception difficult to name without committing oneself to a theory, which therefore we may call a kind of brilliant intuition. It seems to me not impossible ultimately to make a complete list of these marks, and then perhaps to explain why they are characteristic of the secondary personality. Some time ago I paid a visit to a 'medium' residing in a small western city. She is a married woman with a family, and was made known to me by one of my students whose family was intimately acquainted with the woman, having known her from her girlhood. My investigation left no doubt in my mind that she is an honest woman and passes into a genuine trance, and upon awaking is ignorant of her trance-utterances. These take the form of the personality of a Quaker doctor or of a little girl named Emma, both professing themselves to be 'spirits' of deceased persons, and to have supernormal and supernatural knowledge. I conversed for an hour with 'Emma,' and was throughout struck by the remarkable likeness in the general form of the utterances to those more remarkable ones recorded by Dr. Hodgson and others, so that I cannot doubt that we have to do with phenomena of the same genus and species, and that the explanation of the simpler case, were it at hand, would throw much light upon the more complex one. The similarity extended even to an accurate and astonishing statement, made (as so often happens) at the very beginning of the sitting, about my place of residence and my occupation. This was certainly an interesting trait and in need of explanation, although it would not have suggested to me the hypothesis that 'Emma' was the 'spirit' of a real person, for, however difficult it might be for a woman who had apparently never seen or heard of me before

to tell me my home and occupation, it would evidently be more difficult for a young girl to do so who had lived and died prior to the circumstances and relations mentioned. If we have to ascribe to our communicator powers of perception transcending time and space, it makes our hypothesis needlessly complex to ascribe them first to a 'spirit' and then locate the 'spirit' in the person before us. If we ascribe them directly to this person we avoid the trifling inconvenience of supposing that things are known before they happen, or, if we must violate time and space, we have to violate less of both.

Again, not long ago, I became acquainted with a young girl who was an automatic writer and whom I had several opportunities of studying. She wrote rapidly and legibly, only requiring that some other girl should hold the pencil with her. I convinced myself that the writing was purely automatic. It usually purported to come, and was sincerely believed by the girl to come, from the 'spirit' of her deceased mother. I shall mention one or two characteristic utterances from this case, but what I wish to emphasize is merely that the general form of the utterances was so similar to the others which I have studied and to those referred to above, that I cannot doubt that we have here again to do with closely related if not identical phenomena, and that the full explanation of the one would remove the mystery from the other. In all the writing which I saw from this subject (I shall mention some other examples from it below), there was one utterance and one only of the brilliantly intuitive type, and this again came early in the first sitting. In response to my questions, the correct answer was received that I had three sisters and two brothers, that the brothers were both younger and one of the sisters younger and two older. In response to my inquiry about their names, one of my sisters' names, a common one, was given, and then 'Gussie' was written which was spontaneously changed to 'Bessie,' the latter being correct. Admitting that the chances of correctly guessing such a combination as the above at the first guess are too small to make this a probable explanation, and admitting that the young girl, who

was an entire stranger to me at the time, could not have known in any normal way what the most intimate friend that I had in the city could hardly have known, what is the most that can be made out of such an utterance? If found to be a real intuitive utterance, not conforming to the usual laws of perception, memory, or constructive imagination, and if found to be similar to a sufficient number of other automatic utterances, it becomes an interesting mark of the secondary personality, but so far as I can see is not consistent with one more than another of the various hypotheses that have been offered. Probably no thoughtful investigator would apply the 'spirit' hypothesis, for instance, here, but so vitiated have we become in our logical methods when we enter the field of psychical research, that it seems to be generally accepted that if we could adopt this hypothesis it would explain utterances of this class. But, however difficult it might be to understand how the young girl could have known about my family, it would be still more difficult to believe that her deceased mother, who had never even heard of me, could have known, and there was no time to ascertain by inquiries. It is easy for the popular mind to understand all sorts of telepathic, clairvoyant, and time-obliterating powers when attributed to 'spirits' instead of every-day people, and the history of philosophy, despite the warnings of William of Occam, is full of that kind of reasoning. It has become very rare, however, in modern science. The 'spirit' hypothesis accounts for these peculiar phenomena of automatism in the same way that Descartes' 'animal spirits' accounted for the interaction of mind and body, or that the mythological tortoise explained the supporting of the world. From the logical point of view, however, it seems to me that little better can be said of Mr. Myers' theory of a 'spectrum of consciousness indefinitely extended at both ends,' with its 'telepathic and clairvoyant impressions,' 'falling under some system of laws of which supraliminal experience could give us no information' and 'transcending in some sense the limitations of time as well as of space,' having powers 'subject, not to the laws of the known molecular world, but to laws of that

unknown world in which the specific powers of the subliminal self are assumed to operate.' This is a metaphysical, not a psychological hypothesis.

The subject of the experiments which I wish to mention in more detail is a young man, 22 years of age at the time the experiments began. I shall speak of him in the following account as Henry W. He is now a graduate of the University of Iowa, a young man of unquestioned integrity, a quiet and intelligent student, standing high in his class and respected by all who know him. His parents are honest farming people, both native Americans. He has never exhibited any signs of abnormality of any kind, excepting the automatism to be described. He has good physical health and mental balance. Neither he nor his parents are spiritists. He has an aunt, however, who is a spiritist, and about four years before these experiments were begun he had some conversation with her upon the subject and probably opened some books relating to it. This, however, he says, made no impression upon him, and if he casually heard or read at that time any spiritistic phrases, such as 'pass out' for 'die,' he has no conscious recollection of them. He has no interest in the subject and has regarded it, so far as it has entered his thoughts at all, as a curious superstition. About the time of the beginning of the experiments, he became interested in hypnotism, and attended two or three times the performances of a travelling hypnotist, offered himself as a 'subject,' and proved to be an excellent one. He had never previously been hypnotized.

Shortly after this, having read of post-hypnotic suggestion, he inquired of me about it, and at his request I made a trial of it with him. Hypnosis was readily induced by a few suggestions, and I told him that exactly five minutes after he awoke he would go to the next room, secure a book from a desk and bring it to me. A few other simple tests were made, which, though commonplace in themselves, should be mentioned here for reference later. Hallucinations, both positive and negative, were readily induced. I suggested that a small barbed-wire fence was stretched across the floor, over which it would

be necessary for him to step carefully. This hallucinatory fence he saw and stepped over with great care. Upon awaking he remembered nothing of what he had heard or done. Exactly five minutes after awaking he carried out in detail the suggestion about the book. A few days after this, the subject of automatic writing having come to his attention, Henry W. incidentally mentioned to me that when he held a pencil idly in his hand, his hand moved continuously, making scrawls but never writing anything. I therefore made an appointment with him for the study of automatic writing. Three sittings were held and then a period of two years intervened. Then followed three more sittings. All were held on Saturday mornings. The procedure at each morning's sitting was as follows: I provided a quiet room and one assistant. At the second sitting only, others were present. A plentiful supply of very large sheets of smooth brown paper was provided. The subject was so seated with his right side toward the table, that his body was slightly turned away. His right hand held an ordinary pencil in an easy position on the paper.[1] His head was turned slightly to the left, and he held in his left hand an interesting story-book or sometimes the morning paper, which he read and to which he was instructed to give his whole attention. No screen was used, as the sub-

[1] I have never found the ordinary planchette of any use in automatic writing. When it is discovered that two persons succeed better in writing than one, both may grasp a common lead pencil, one hand above the other. The instrument used by Professor Jastrow, consisting of a glass plate upon glass-marble rollers, whether used for automatic writing or any involuntary movements, has the disadvantage of moving by its own momentum when once started. When it is necessary to 'educate' from the beginning an automatic writer, a delicate planchette mentioned by Miss Stein may be used. It consists merely of a board swung from the ceiling by a small wire. The one used in our laboratory consists of a light board six inches square, upon which the fingers rest as upon the common planchette. Through the board is a hole fitted with a glass tube in which a pencil is placed so that it will move up and down. A weight attached to the top of the pencil keeps it pressing lightly and evenly upon the paper below. Such a planchette, swung from the ceiling over the table, will glide around upon a large sheet of paper with the slightest effort, the pencil point always leaving its tracings.

ject could not see the writing without turning his head. The sittings lasted two or three hours with intervals of rest. The writing was usually quite clear, but occasionally illegible. If illegible, the communicator was asked to write the answer again. At one time I suggested to the communicator that he was a good penman, his chirography being round, clear and rapid. Instantly it became so and gave us no more trouble at the time. Henry W. never knew what he had written without reading it, except in a few instances when, his attention being allowed to wander from his book or newspaper, by following the movements of his hand he could tell something of the communication. He was much interested in the writing and was occasionally allowed to look at it. When it was nearly illegible he was never able to decipher it better than the others. The questions were either prepared beforehand and numbered or else taken down and numbered by the assistant, who also numbered the answers as written. My space will not permit me to give more than a portion of the questions and answers, nor would it be profitable to do so. They may be classed in three groups: Those of the first group were intended to bring out all the information possible about the communicator himself, his past history, his present mode of existence, his mental habits and his emotional peculiarities. The second group was intended to test his professed supernormal knowledge. The third group was directed to possible remarkable powers, such as telepathic knowledge, mathematical ability, hypermnesia and prophecy. The questions of the first group were connected more directly with the object of my inquiry. No remarkable telepathic or intuitive powers were discovered. If such powers had been found, they would have been of interest, but hardly more important for gaining a thorough knowledge of the secondary personality than more simple if less striking traits.

The first sitting opened as follows:

Q. Who are you?
A. Laton.

This was illegible, and Henry W. was allowed to look at

the writing. He read it as 'Satan' and laughed. A further series of questions revealed the name as 'Laton.'

Q. What is your first name?
A. Bart.
Q. What is your business?
A. Teacher.
Q. Are you man or woman?
A. Woman

No explanation of this answer was found. Laton assumed throughout the character of a man.

Q. Are you alive or dead?
A. Dead.
Q. Where did you live?
A. Illinois.
Q. In what town?
A. Chicago.
Q. When did you die?
A. 1883.

Then followed many questions, first relating to the bill of fare of Henry W.'s dinners for one, two, and three weeks back. Laton could give the *menu* somewhat correctly for two weeks back, but beyond that he said "I don't know." His memory of them seemed somewhat but not greatly superior to Henry W.'s. Various problems in mental arithmetic were given, the simplest being 16 x 9. The answers were always promptly written and were uniformly wrong. Tested upon the dates of well-known historical events, his answers were all incorrect. Asked about my mother's name he wrote 'Mary Peters,' but afterward changed it to 'Lucy Williams,' both wholly wrong. My sisters' names were given as 'Winnifred,' 'Jennie,' and 'Carrie'—all wrong.

Q. Have you supernatural knowledge, or do you just guess?
A. Sometimes guess, but often spirit knows. Sometimes he will lie.

The next sitting was held two days later.

Q. Who is writing?
A. Bart Laton.
Q. Who was mayor of Chicago when you died?
A. Harrison. [Carter Harrison was mayor of Chicago from 1879 to 1887.]
Q. How long did you live in Chicago?
A. Twenty years.

Q. You must be well acquainted with the city.

A. Yes.

Q. Begin with Michigan avenue and name the streets west.

A. Michigan, Wabash, State, Clark (hesitates) forget

Henry W. is then asked to name the streets, and can name only Michigan, Clark and State.

Q. Now your name is not Bart Laton at all. Your name is Frank Sabine, and you lived in St. Louis, and you died November 16, 1843. Now, who are you?

A. Frank Sabine.

Q. Where did you live?

A. St. Louis.

Q. When did you die?

A. September 14, 1847.

Q. What was your business in St. Louis?

A. Banker.

Q. How many thousand dollars were you worth?

A. 750,000.

Q. Can you tell us something which Henry W. doesn't know?

A. Perhaps. I'm not a fraud.

Q. Who was mayor of St. Louis when you died?

A. John Williams.

At the next sitting, a week later, Henry W.'s father and mother, who were visiting him, were present, and a young lady named Miss J.

Q. Who is it that is writing?

A. Bart Laton.

Q. Where did you live?

A. Chicago.

Q. When were you born?

A. 1845.

Q. How old are you?

A. 50. [This sitting was held in 1895.]

In this and other answers where easy computations are correctly made, there is a slight hesitation accompanied by muscular indication of effort in the arm.

Q. Where are you now?

A. Here.

Q. But I don't see you.

A. Spirit.

Q. Well, where are you as a spirit?

A. In me, the writer.

Q. Multiply 23 by 22.

A. 3546.

Q. That was wrong; how do you explain your answer?

A. Guessed.

Q. Now, the other day you represented that you were someone else. Who was it?

A. Stephen Langdon.

Q. Where from?

A. St. Louis.

Q. When did you die?

A. 1846.

My question was in the form of a suggestion that he, the writer, is Stephen Langdon, which is naively accepted.

Q. What was your occupation?

A. Banker.

Q. But who was Frank Sabine?

A. I had the name wrong. His name was Frank Sabine.

Q. Now I want to know how you happened to take the name Laton.

A. My father's name.

Q. But where did the name Laton come from? Where did Henry W. ever hear it?

A. Not Henry W. but my father.

Q. (By Miss J.) Have you any message for any of us?

A. I don't know you well enough, but Prof. P—— should not be so incredulous about spiritualism.

According to Laton's later account of himself he was a tutor in a family in Chicago before the Civil War, where Henry W.'s father was a chore boy in the same family. Altogether inconsistent with this is his present statement that he doesn't know any of the company well enough to give them a message.

Q. But tell me how you came to assume the name Laton?

A. *I am a spirit.* (Written with great energy as heavily as the pencil would write.)

Q. What is your relation to Henry W.?

A. I am a spirit, and control Henry W.

Q. Of all the spirits why did you come to control Henry W.?

A. I was near when he began to develop.

Q. Now look here, this is nonsense. You are not a spirit, and you know you are not, and I must know how you came to pick up the name Laton.

A. Darn you, I am Laton.

Henry W. is allowed to read this, and, his father and mother being present, is greatly vexed and asks, "Did I write that?"

After this sitting Henry W. was absent for two years. Dur-

ing this time he never tried automatic writing, was never hypnotized, and apparently gave no thought to the previous experiments. The sittings were renewed in the spring of 1897.

Q. Who are you?
A. Bart Layton. [Note change of spelling from this on.]
Q. What have you to say to us?
A. Glad to see you.
Q. When did you write for us before? Give year, month, and day.
A. I don't know.
Q. In what year was it?
A. 1895.
Q. In what month?
A. Don't know. April, I remember. [It was June, 1895.]
Q. Tell us more about yourself.
A. I lived in Chicago.
Q. Do you live there still?
A. I am here now.
Q. How long did you live in Chicago?
A. Twenty years.
Q. Why did you leave there?
A. None of your business.
Q. In what year did you leave?
A. 1872.
Q. What was your occupation?
A. Doctor and carpenter.
Q. In what year were you born?
A. 1840.
Q. In what year did you die?
A. Did who die?
Q. In what year did you pass out?
A. 1875.
Q. Who was Stephen Langdon?
A. Chicago friend.
Q. Did you write Chicago friend?
A. Yes, can't you read?
Q How many minutes was it before you brought the book.
A. Five. (After hesitation.)

This question relates to the post-hypnotic experiment tried upon Henry W. two years before and related above. It was sprung upon the communicator to test his relationship with Henry W.'s hypnotic personality. The answers to the questions following about the fence are still more striking, for Henry W. never knew anything at all about the fence episode, having been tested after the experiment two years before.

Q. Where did you get the book?
A. Table.
Q. What did you do with it.
A. Gave it to you.
Q. Who else was with us?
A. Mr. Grimes. [Correct.]
Q. What was it you had to step over.
A. Fence.
Q. What kind?
A. Barb wire.
Q. Who was it stepped over the fence?
A. I did, you fool.
Q. What was your name?
A. Bart Layton.

The following questions and answers were from the last two sittings held two and three weeks later. At the beginning, an attempt, not very successful, was made to cultivate a good humor in the communicator. At the end, a second successful attempt was made to anger him.

Q. Who is writing?
A. Bart Layton.
Q. Good morning, Mr. Laton. Glad to see you. Would like to get better acquainted with you.
A. I don't care.
Q. Now, Mr. Laton, will you give us some message if you will be so kind?
A. From whom?
Q. Well, from yourself.
A. I am all right.
Q. From whom could you bring us a message?
A. Whom do you know?
Q. Well, I have many friends. Are you in communication with my friends?
A. George White.

In all Laton's writings this was the one single instance of the brilliantly intuitive type, though not a very striking one. I had an uncle named George White for whom I was named and who was killed in the Civil War. Henry W. knew nothing of this, but he had had opportunities of seeing my own name written in full, containing these two names with a third name, however, Thomas, between them. In answer to further questions, Laton said that George White was my father or grandfather and 'passed out naturally' fifteen years ago. Upon

a request for a message from George White, he wrote, He is glad to see you so well.

Q. Tell us, Mr. Laton, something we don't know, won't you?
A. Think you're smart, don't you?
Q. When did you write for us before?
A. Five weeks ago.
Q. Where have you been in the meantime?
A. Everywhere.
Q. Tell us something of your own life. How do you pass your time every day?
A. I never entirely leave Henry W., but partly so.
Q. When you leave him where do you go?
A. Anywhere or nowhere.
Q. What were you doing yesterday at this time?
A. With Henry W.
Q. What did you have for supper Thursday of this week?
A. None of your business.

Then followed questions in mental arithmetic in which my assistant and I both thought attentively of a certain incorrect answer. Wrong answers were given in each case, but not the ones we thought of. Laton was also asked to give the time of day, which in each case he gave incorrectly, even when we were looking intently at our watches.

Q. What was Mr. Laton's occupation in Chicago?
A. Carpenter.
Q. Two years ago you said he was a teacher.
A. Well, he—I used to be a teacher.
Q. Do you dance?
A. We don't dance who have passed out.
Q. Why don't you who have passed out dance?
A. You can't understand; we are only as you would say partly material.
Q. When you get through writing today, where is the part that is not material going?
A. It goes nowhere or anywhere as you choose to know space.
Q. Do you ride a bicycle?
A. Only through Henry W.
Q. Two years ago you spelled your name 'Laton.' How do you account for that?
A. Too many Latons; like the other better.
Q. I think you are an unmitigated fraud. What have you to say to that?
A. Shut up, you poor old idiot. Think I most always answer your danned old questions right? I can lie to you whenever I danned please.

This answer was accompanied by great muscular excitement of the hand and arm. There being one or two illegible words, I had the communicator repeat parts of the answer several times. The word 'danned,' evidently intended for 'damned,' was so spelled each time. Henry W., meanwhile, was calmly reading and never knew what had been written.

The automatic writing was now discontinued, as evidently there was little more to be gained from Laton. But the familiarity of the communicator with the hypnotic actions of Henry W. suggested one further experiment. If Henry W. were hypnotized, would the hypnotic personality assume the name Laton, and give the same account of himself orally? Henry W. consenting, hypnosis was induced by a few suggestions and was tested by a simple experiment in hallucination. I suggested that there was a five-dollar gold piece on the edge of the table. The subject saw it and asked whose it was. My assistant jokingly said that it must be Laton's, whereupon the subject went through the motions of grabbing it and putting it in his pocket with great glee, remarking, "If it's Laton's, it is mine, for he is a part of me." Evidently, then, the hypnotic personality did not necessarily consider itself as Laton, but my assistant's remark was perhaps a suggestion that Laton was not present. I therefore changed the subject's seat, bade him close his eyes for a moment and suggested that he was Laton. This was instantly successful, and a free conversation was then carried on with Laton as long as I wished. The subject's eyes were wide open and his manner easy and unconstrained, though not quite that of Henry W. There was no sign of Laton's recent anger, but the account that he gave of himself was the same as given in writing, with some added details. He said that he 'died' in 1875 at the age of sixty, that he lived on North Clark Street, that he was before the war a tutor in the family of Mr. Pullman, where Henry W.'s father was then a chore-boy, that he was a tutor of Mr. Pullman's little girl, but failing in the capacity of a teacher, and Chicago building up rapidly, he went to carpentering. He said further that he had been with Henry

W. since '75 ['95?], that he had chosen him because he was the right kind. "He developed," he said, "and I got a chance to show myself." A few other questions were asked testing the power of thought-transference, but without result. The subject was then awakened and found to have no knowledge of what had happened. A striking feature of the experiment was the instantaneous and naive assumption of the personality of Laton after the suggestion was made. As soon as the word was spoken, there was no confusion of 'he' and 'I' as relating respectively to Henry W. and to Laton.

Before commenting upon any peculiarities of the secondary personality indicated by the above conversations, I may mention some other details of the investigation. As may be seen, my attempts to trace from internal evidence the origin of the name, Bart Laton, were not successful. The external evidence yielded no better results. I could not learn that Henry W. or any member of his family had ever known any one bearing the name Bart Laton, or even Laton. The hypothesis that there was a real Bart Laton whose 'spirit' was communicating through Henry W. will hardly appeal to any one who has read the questions and answers, even if we grant, with Dr. Hodgson, that communicating 'spirits' must *a priori* be suffering from a certain amount of 'confusion,' or even 'aphasia' and 'agraphia.' The frequent contradictions as to the time of his birth and death, his uncertainty as to whether he was a teacher, carpenter or doctor, his willingness to resign his personality in favor of Frank Sabine or Stephen Langdon, together with the unmistakable evidence that the whole 'history' was progressively constructed in answer to my questions, make such a view as improbable as it is unnecessary. I did not, however, omit to make diligent inquiries in Chicago. The experiments were completed before Mr. Pullman's death, and through the kindness of Hon. Frank Lowden, his son-in-law, I learned that none of Mr. Pullman's family had known any one bearing the name, Bart Laton, that Mr. Pullman's daughter had never had a tutor by that name or any other male tutor. The chronology given by the communica-

tor would in any case make such a relation impossible. The communicator's statement that Henry W.'s father was at one time a chore-boy in Mr. Pullman's family was correct, but this was known by Henry W. and may indeed have served as a basis for the communicator's romance. I conclude, therefore, that the origin of the name is to be traced directly to the constructive imagination of the secondary personality.

In attempting any description of the marks of the secondary personality, either from a study of this or of other cases of automatism, we are struck perhaps first of all by the remarkable activity of the constructive imagination. Quite independent of all theories, the presence of this particular form of mental activity is characteristic. It is shown in this case throughout the whole conversation, for instance, in the fictitious answers to the mathematical problems, in the construction of the Chicago 'history,' and in the invention of the names, Mary Peters, Lucy Williams, Stephen Langdon, John Williams, etc. Frank Sabine differs from the others only in this, that I invented it myself and suggested it to the communicator. By way of experiment, any number of such names, some commonplace like John Williams, others more unique like Bart Laton, may be collected by any one who will ask a number of his friends to assume or invent a name on the spur of the moment. If, for the sake of the argument, we omit the comparatively few instances of the brilliantly intuitive type, the great mass of automatic utterances in this and in all other reported cases reveals the activity of the constructive imagination and shows further the most rigid adherence to the law of limitation to the store of memory images possessed by the subject. This limitation is painfully apparent in the utterances of my subject. The communicator has a vivid imagination, but the materials are all drawn from the experience of Henry W. The hypermnesia exhibited by many subjects and shown in a very trifling degree by mine—as, for instance, when Laton mentions one more of the Chicago streets than Henry W. can—in no way, of course, violates this law.

The suggestibility of the secondary personality is also apparent from this case. The communicator is willing, in response to my suggestion, to change his whole personality, and become Frank Sabine of St. Louis, and then proceeds to construct a 'history' consistent with the suggestion. In response to my suggestion again, he accepts the name Stephen Langdon, at another time becomes a good penman, admits that he 'guessed' the answers, etc. His suggestibility is limited only by a sort of insistent idea that he is a 'spirit,' which determines the answers in the form of a 'spirit' personality limited to the scant knowledge of what such a personality should be, possessed by Henry W. The very opposition which he shows in the later sittings is apparently the result of my indirect suggestion of hostility shown by the skeptical and disrespectful attitude which I assumed. In this connection, it is worthy of notice that in any conversation with a secondary personality, the questions themselves form a series of suggestions, and that properly prepared questions are of first importance. In the present instance, my questions may have determined the whole 'history' of Laton, and a different set of questions would have resulted perhaps in a totally different account. My first question, Who are you? really suggests a doubling of the personality. My question, Are you alive or dead? suggests perhaps the 'spirit' idea. The questions were well adapted to the study of the birth and development of a 'spirit' personality, but it would be interesting to know what a wholly different set of questions would have produced. We should observe that the question, Who are you? or, What is your name? is an indirect suggestion of a doubling of the personality. My first question might then have been, not, Who are you? but, Write your name in vertical script. If then the communicator had given the name, Bart Laton, I might merely have expressed surprise that his name was not Henry W., thus avoiding any even remote suggestion of a 'spirit' presence.

Another peculiarity of the secondary personality which has been noticed in other cases is its rather low or 'common' moral

and intellectual tone. This was conspicuous with Laton as well as with the other communicators mentioned in this paper. In the case of Laton, my skeptical attitude was assumed for the purpose of allowing this trait to develop and to see what kind of language the communicator would use when angered. Stupid profanity was the result. The answers throughout were commonplace. When asked for a message from the 'spirit' of my uncle, he can only say: "He is glad to see you so well." This peculiar trait is strikingly illustrated in one of my other subjects, the young girl mentioned above. To test her alleged clairvoyant powers, I had prepared a name written upon a sheet of paper and sealed in an opaque envelope. The communicator, the 'spirit' of the girl's deceased mother, professed to be able to read it and said that it was 'Mamie Nolds.' This was wholly incorrect, and I so stated. The communicator, however, insisted and insisted again that the name was 'Mamie Nolds.' I therefore opened the envelope, held up the writing, and triumphantly asked, "Now what have you to say?" To which this interesting and characteristic answer was written, "I think you are furrucht in the kopf," misspelled school-girl slang of rather a low order, such as I think the subject herself would not have used even with her associates. The utterances are sometimes of a flippant tone. One of the 'controls' of the girl just mentioned, professing to be the 'spirit' of 'Ben Adams,' who passed away in 1872, always wrote flippant answers. For instance, his veracity being questioned, he wrote, "I am not a fraud or a frog either." Asked the day and month of his death, he said, "I don't know. I got hit on the head."

Among the peculiarities of the secondary personality we may, perhaps, regard as fourth in order the brilliantly intuitive character of a very limited number of these utterances. In the case described by Dr. Hodgson these are very striking. With my subjects I have mentioned two instances of such utterances. Even with Bart Laton there is, as it were, a trace of the presence of such a trait in his mention of George White. Considering the sluggish character of Laton's mind and his

very slight ability to use the latent memories of Henry W., it does not seem very probable to me that Laton was shrewdly using a latent memory of a part of my name, hoping that it might happen to coincide with the name of some deceased relative. Such an explanation is possible, or it may have been a chance guess, but, considering the large number of such cases which the history of automatism affords, it seems to me better to note this power of happy intuition as one of the marks of the secondary personality. The explanation of it is not within the purpose of the present paper. It seems like the flickering survival of some ancient faculty. One thing only is sure in this case, the origin of the utterance was with the immediate participants in the experiment. For let us suppose that it was not a guess nor the revival of a latent memory of Henry W., but that it was communicated from some outside source. We should have to choose then between its being communicated unconsciously by me and its being communicated by the 'spirit' of the deceased George White. Put in this form, the 'spirit' hypothesis immediately becomes absurd, for, even if we have to assume, as is not indeed really necessary, that the name was communicated 'telepathically' by me, we must assume this and a great deal more if it was communicated by George White. Furthermore, if I may risk taxing the patience of the reader by further reasons where none are necessary, it would be more probable that the suggestion came from me from the fact that I have always had a romantic interest in the memory of this uncle, while George White, himself, hardly knew me at all. To my mind, however, rejecting the 'spirit' hypothesis does not mean accepting that of 'telepathy.' When the characteristics of the secondary personality become subject to accurate scientific description, some other hypothesis may be found quite apart from either.

Meanwhile it seems to me of the highest importance that the dignity of psychological science should be maintained by the use of modern logical methods. For instance, it seems to be regarded as a 'test' of the 'spirit' hypothesis by those who have urged it and to be naively accepted as such by reviewers

and critics, when the communicator is able to relate that which is occurring at a distant place. The instances of this seem always to have some element of uncertainty about them. But granting that such uncertainty were removed, what then? Eliminating fraud, and telepathy from those present, it is argued that it must then be 'spirits.' Imagine such methods pursued now in the physical sciences! Any new manifestations or reaction not following known laws might be attributed to 'spirits'! For instance, light-rays do not penetrate opaque substances. The new X-rays ignore this law; they must be due to 'spirits.' But, it may be said, we have, in the case reported by Dr. Hodgson, other tests of quite a different kind, where only the 'spirit' hypothesis is applicable. The one which appears to be particularly convincing is that of a communicator who gives as his name that of a New York man known to have died some time before, and who offers various convincing proofs of his identity. But is the logical aspect of this kind of evidence any better? Again, supposing that fraud, and telepathy from those present, are eliminated (and from the published reports, fraud at least seems to have been conscientiously eliminated), the bare facts of the case are that a certain woman in the city of Boston, in a certain abnormal condition, writes or relates occurrences which happened not only at a distant place, but at a past time, and shows herself familiar with certain friends and doings of the New York man. This is more 'remarkable' even than light-rays piercing opaque substances. Surely it must be due to 'spirits'! It may be that science will ultimately gain such knowledge of disembodied minds that it can use them as the basis of an explanation of phenomena in abnormal psychology, but at present the advancing of such hypotheses by psychologists can only serve to further the cause of superstition, to which people are already only too willing to fly when something mysterious presents itself.

As regards the various traits of the secondary personality, some of which have been referred to in this paper, it has been suggested by Mr. Podmore and others that they are instances

of survival or reversion. One cannot indeed fail to be impressed by the similarity of these traits to what we know or conjecture about the primitive mind. The general low moral and intellectual tone of the communications, the vulgarity and mild profanity, the frequent impersonation of the medicine-man, Quaker doctor, Indian doctor, etc., the keen memory and dull reason, the vivid constructive imagination, the deception and prevarication, the unwavering belief in spiritism, and the superstitious devotion to amulets, trinkets, and petty articles of ornament or apparel, all point to an early stage in the evolution of mind. Even the peculiar intuitive power sometimes exhibited by the secondary personality may be compared to the superior intuition of woman, whose mental peculiarities are in general representative of the more stable, basal and abiding phenomena of mind. Both may point to some nearly extinct faculty no longer serviceable. Still other peculiarities suggest the same theory, such as the extreme suggestibility and motor force of ideas, marks of automatism and of the hypnotic state, and at the same time characteristic of the child and savage mind. In close relation to this is the peculiar intimate connection between ideas and organic, nutritive and circulatory processes, best shown in hypnosis, and common to this group of phenomena. In view of such facts as these, certain of the more simple physiological theories of double personality gain considerable plausibility, such, for instance, as the revival of disused and outgrown brain tracts, particularly perhaps those of the less specialized hemisphere. The frequent appearance in automatic writing of *Spiegelschrift*, which occurs also among children, lends some support to this view.

It has not, however, been my purpose in this paper to propose any new theory or establish any old one to account for the phenomena of automatism, but rather to urge the extension of experimental inquiries in this direction, to point out certain prevailing peculiarities of the secondary personality, and to insist that the more complex and mysterious cases are to be understood by a constant reference to the simpler ones.

NEW PSYCHOLOGICAL APPARATUS.

BY

C. E. SEASHORE.

I. A SPARK CHRONOSCOPE WITH ACCESSORIES.

Of the hitherto known forms of apparatus for measuring short intervals of time, the graphic spark apparatus is the most accurate and the pendulum apparatus the most convenient. In the chronoscope that is shown in the accompanying figure, the spark method of recording is combined with the pendulum action.

The cut is reduced to a scale one-sixth of the size of the apparatus. The pendulum is shown in the starting position. The lower bob terminates in a knife edge which rests upon the projecting edge of a mechanical release key. The action of this key is soundless and gives the pendulum no impetus in either direction. On the other side of the apparatus is a spring key which catches the pendulum at the end of the swing. When the pendulum is released from this, it swings back with little assistance to the starting point and makes all necessary adjustments automatically. On the back of the lower bob is an index point which runs at the upper edge of the scale and serves as a spark point.

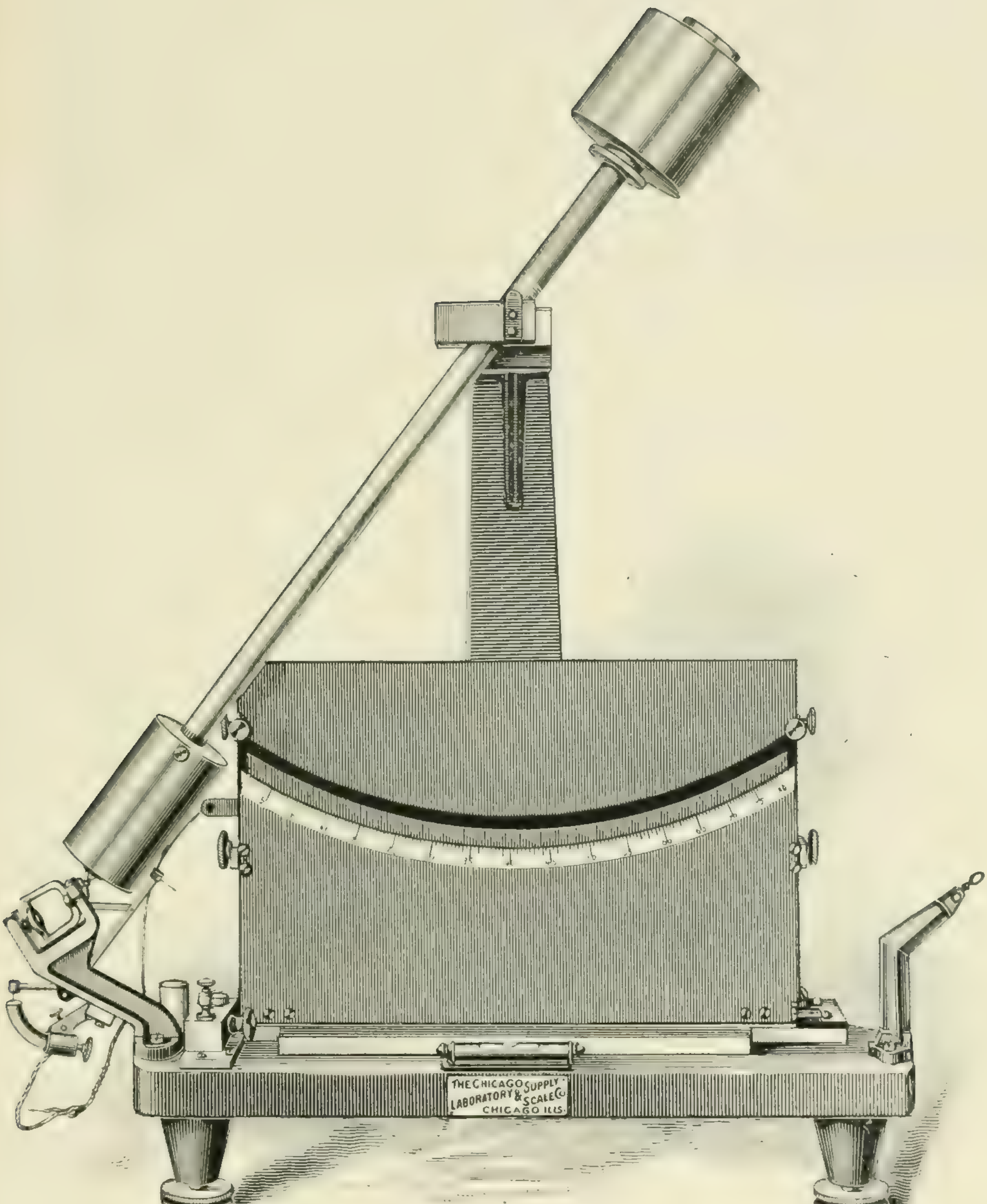
The record is made upon a smoked paper which is seen through the slit above the scale. This paper is stretched upon two rollers; it also rests upon an insulated metal plate which serves as an electrode and keeps the paper straight and smooth back of the scale. Back of this plate is a third roller by means of which the tension of the paper may be adjusted. The paper support is built on a carriage so that it may be removed and replaced without disturbing the rest of the apparatus. In preparing the paper this carriage is removed

and the paper is smoked as on an ordinary kymograph drum. As a complete record consists in a single spark which may be recorded at once, several hundred records may be made with one preparation of the paper, which may then be re-smoked so that a single paper may serve for several thousand records. The paper is moved as needed by a thumb screw at one end of the upper roller.

In reaction experiments the stimulus is given automatically by the apparatus when the pendulum indicator passes the zero point on the scale. A double rocking lever at this point makes one circuit and breaks another, either of which may be used in giving the stimulus. These contacts are adjustable platinum and mercury contacts and their adjustment may be verified by direct sight. The closing of the circuit is soundless, and the stopping of the lever in a soft rubber clutch makes no sound that can be heard a few feet away.

The reaction, or termination of the interval to be measured, is indicated by a spark on the sensitive paper at the edge of the scale. The spark is produced by interrupting the primary circuit of an ordinary induction coil. One secondary terminal is connected with the insulated plate on which the paper rests and the other is connected with the body of the apparatus. The point of the pendulum indicator is the nearest metal to the plate; therefore the spark flies from this point, through the sensitive paper, to the plate.

The scale is graduated empirically by the most reliable graphic method into hundredths of a second, and each unit is divided into halves. The average space of one unit is 5 mm. on the arc of the scale. With this adjustment the scale covers 0.80 sec. and the records are read in half-hundredths with ease and accuracy. This division is the most convenient and appropriate to use in reaction experiments. The variation in the movement of the pendulum is negligible because the pendulum is carefully constructed and balanced and moves without friction. The variation in the make contact is also negligible because the platinum terminal moves much faster than the pendulum indicator. The spark tends to take the short-


THE CHICAGO LABORATORY SUPPLY & SCALE Co
CHICAGO ILLS.

est course between the point and the plate, but it may be deflected. The maximum distance between the spark point and the paper is 1 mm. The maximum deflection of the spark may be estimated to be about 45°. That amount of deflection is not liable to occur for the maximum distance, but if it did the maximum variation would be ± 1 mm. on the scale which is equal, on the average, to ± 0.002 sec. The average distance between the spark point and the paper is about ½ mm. and the average angle of deflection of the spark is less than half of 45°; therefore the average variation in the spark is less than ± 0.001 sec.

The chronoscope may be adapted for the measurement of longer intervals, as in the study of association, by two minor changes which can be made in a minute. A small weight is fastened on the top of the upper bob. This makes the pendulum swing so slowly that it takes three seconds to cover the arc of the scale. A corresponding scale, graduated empirically in hundredths of a second, is clamped over the regular scale. The accuracy is nearly proportional to the speed of the pendulum.

Similarly, if there should be a demand for finer readings than those obtained by the standard adjustment, an extra weight may be placed on the lower bob that will cause the pendulum to cover the arc of the scale, for example, in one-third of a second. If the corresponding scale is graduated in thousandths of a second each unit will occupy, on the average, 1 mm. of space. The degree of accuracy will be nearly proportional to the speed, because the latent time of the spark is negligible and the action is frictionless.

Much of the value of a chronoscope lies in its adaptation to the attachment of a variety of accessories. In this one the operator has the choice of using the automatic make or the automatic break. For auditory stimulus, I use a telephone receiver, with or without an induction coil, and in the make or the break circuit. For visual stimulus I use the apparatus described on p. 66 of this volume, in the make circuit.

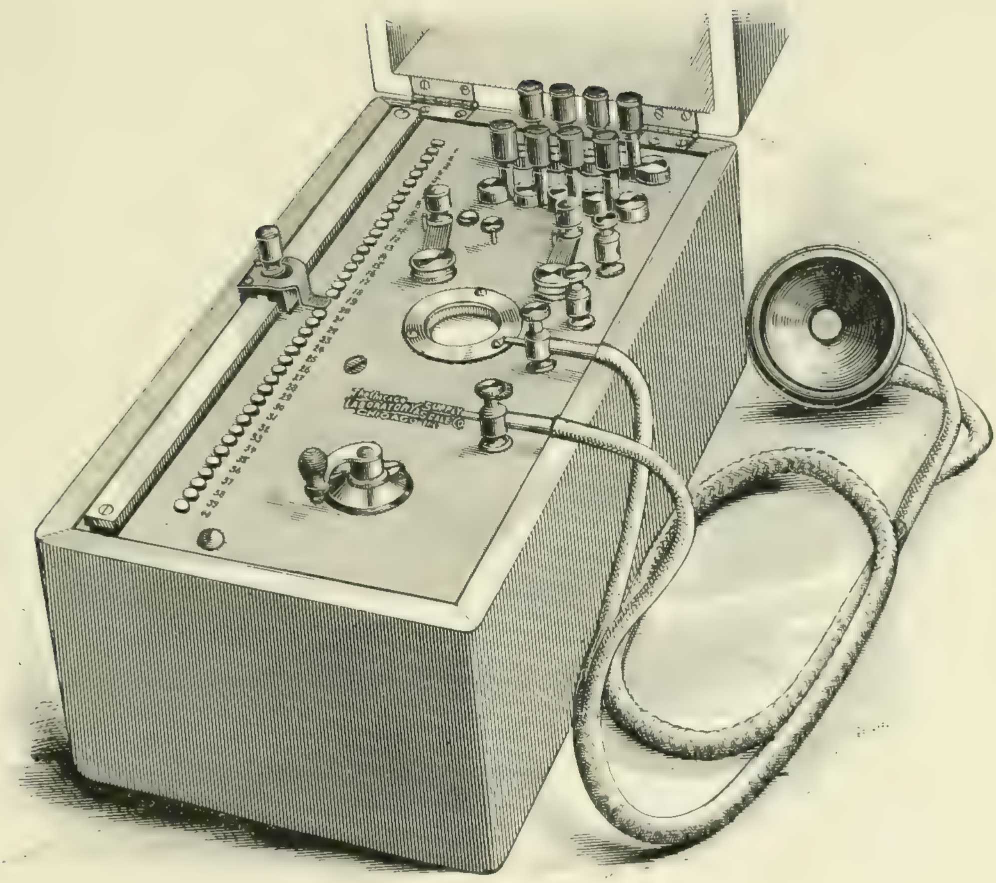
For the study of associations, etc., with visual stimuli, I have

constructed a new tachistoscope which is adapted for use with the chronoscope. A face board contains an aperture 2 cm. high and 4 cm. long. Back of this is a double electric shutter which opens to expose the word or object to be seen. When the circuit in the electromagnets which keep the shutter closed is broken, one leaf of the shutter is drawn up and the other down by the action of two pairs of springs. In the opening, straight springs co-operate with coil springs making a quick exposure, but after the leaves have passed the aperture the former counteract the latter and thus minimize the vibration and sound of the shutter. The shutter exposes one section of a chance-wheel in which a series of cards may be inserted. The time for the exposure of any particular area can be readily measured with the chronoscope. This apparatus is also adapted for use in the usual tachistoscopic experiments in time exposure. It may be connected electrically with a pendulum which breaks and makes the circuit at measured intervals. This tachistoscope, like the chronoscope, has been made by the Chicago Laboratory Supply and Scale Company.

The following may be mentioned as special merits of this chronoscope: accuracy, adaptation for a variety of connections, soundless action, direct reading, ease and permanence of adjustment, and quickness and convenience of manipulation.

II. An Audiometer.

There exists a great demand for an instrument by which to produce and measure relative variations in the intensity of sound. Many instruments and methods have been devised for this purpose, but aurists and psychologists to-day measure the sound stimulus in terms of "my watch" or "my voice." The audiometer shown in the accompanying cut has been constructed primarily for use in the measurement of keenness


The Chicago
Laboratory & Supply Scale Co
Chicago

of hearing.[1] It is adapted to the needs of the psychological laboratory, the school room, and the aurist's office.

The essential and unique feature of this apparatus consists in the method of varying and measuring the relative intensity of the sound. This is accomplished by applying the principle that, for certain given relations between the primary and the secondary coils of an induction coil, the induced current varies directly with the number of turns of wire in the secondary coil. The complete apparatus consists of an induction coil, a battery, a galvanometer, a resistance coil, switches and a telephone receiver, all except the receiver being built into one compact and portable piece.

A dry battery is so connected that it may be thrown into the primary circuit of the induction coil by turning the left-hand switch. The galvanometer, seen through the crystal in the center, may be thrown into circuit by turning the right-hand switch. The fall of potential over the primary coil is reduced to the standard, e. m. f., by varying the resistance by means of the plugs at the farther end of the chest and gauging it by the galvanometer. The resistance permits of as small variations as can be detected by the galvanometer; and the galvanometer detects smaller variations in the current than can be detected by the ear at the receiver. The lever at the near end of the chest is a key which is used for the rapid closing and opening of the primary circuit in producing the stimulus. No current is drawn except for the moment that the circuit is closed by this key. The primary coil is longer than the secondary. The latter is wound in forty sections, arranged in a series according to the number of turns of wire that each contains, as may be seen in the accompanying table. Each of these sections is so connected with the surface terminals along the scale that the spring contact on the sliding carriage throws into circuit the number of sections indicated by the numbers on the scale. Therefore, to vary the energy communicated to the receiver in this circuit, it is necessary

[1] I am indebted to Mr. Charles Bowman, instructor in physics in this University, for much valuable aid in developing this audiometer.

only to move the carriage along the scale to the proper terminal. As it is most convenient to vary the stimulus in a geometric ratio according to the psycho-physic law, this principle has been taken as a guide in determining the scale of intensities of the sound. The numbers on the audiometer scale are given in the first column in the accompanying table; these indicate the corresponding number of sections involved in the secondary circuit. The second column gives the corresponding number of physical units in terms of the total number of turns of wire in circuit. The ratio of the increments in the sound is such that the forty steps in the series are, as nearly as can be determined, psychologically equal. The serial numbers on the scale are used in all readings. These measurements all refer to the strength of the current which energizes the receiver. The functional relation between the strength of current and the amplitude of vibration in the receiver is somewhat complex, but for the present purpose it may be regarded as fairly constant.

Table of Values for the Audiometer Scale.

I	*II*	*I*	*II*	*I*	*II*	*I*	*II*
1	1	11	13	21	58	31	270
2	2	12	15	22	68	32	315
3	3	13	17	23	79	33	368
4	4	14	20	24	92	34	429
5	5	15	23	25	107	35	500
6	6	16	27	26	125	36	583
7	7	17	32	27	146	37	680
8	8	18	37	28	170	38	793
9	9	19	43	29	198	39	925
10	11	20	50	30	231	40	1079

I, scale on the audiometer.
II, corresponding values.

The range of the intensity of the sound is such that it is not probable that any person can hear the weakest sound and all who can hear ordinary conversation at all can hear the strongest sound. The average threshold for normal ears lies near the middle of the scale. No delicate parts of the appara-

tus are exposed, and the battery, which is practically the only part that deteriorates, can be replaced without disturbing the adjustments. So long as the apparatus remains intact, the given relative measurements are constant for the same audiometer. Furthermore, the Chicago Laboratory Supply and Scale Company, which has taken great pains to perfect the mechanism of this apparatus, has one standard piece by which all audiometers made by them are standardized. Thus all who use this audiometer have a common standard and unit of measurement.

The relative scale of the intensities of the sound, the means of retaining the standard, the compact form, and simplicity and convenience of manipulation, are some of the merits in this apparatus.

For certain tests by aurists and experiments in the psychological laboratory, it is desirable to have a tone instead of a click for stimulus. Provision has been made for the production of tones in the audiometer. The inside connections are so arranged that by attaching a double contact electric tuning fork to the binding posts seen to the right, the fork may be made to interrupt the primary circuit of the audiometer and thus produce the tone of the fork in the receiver. This tone may be varied and measured in the same way as the regular stimulus.

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CONTENTS

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The UNIVERSITY OF IOWA STUDIES IN PSYCHOLOGY is a bulletin devoted to the publication of researches carried on in the psychological laboratory. For the past two years the work of research in the laboratory has centered about the study of illusions in geometrical forms. Some of the results of these researches are reported in the present volume (Vol. III) in the article by Miss Williams entitled Normal Illusions in Representative Geometrical Forms, the basis of a work to be submitted by her as a thesis for the doctor's degree. Further experiments upon the same subject are reported by Dr. Seashore and Miss Williams in an article entitled An Illusion of Length. This is the only article in the present volume that has been previously printed.

The planning and building of a new laboratory, also described in this volume, has made it necessary to give much time and attention to the matter of equipment. This work has been under the immediate direction of Dr. C. E. Seashore, who in two articles describes some of the more important pieces of apparatus devised by him during this period.

Iowa City, May, 1902

A METHOD OF MEASURING MENTAL WORK: THE PSYCHERGOGRAPH [1]

BY

C. E. SEASHORE

In his presidential address at the meeting of the Psychological Association a year ago, Professor Jastrow outlined some problems in psychology that await solution at the present time. One of those problems is in part the subject of this paper, and I quote the following general statement of the needs, the possibilities, and the significance of this work:

"An adequate set of tests of normal functional efficiency, that shall receive a considerable authoritative sanction, is a great desideratum for the present-day needs, and an end by no means beyond the goal of properly directed endeavor. Its starting point is a correct analysis of the most distinctive modes of exercise of the several elementary components of our mental functions; the second step is the devising of tests that shall most simply, naturally, and definitely measure the functional efficiency of a selected factor or process; this accomplished, the way is prepared for the extensive utilization of such standards or norms of efficiency, (a) by their correlation with one another, (b) by a comparison with similar results obtained upon children at different stages of their development, thereby gaining an insight into the order and nature of their generic unfoldment, and (c) by a comparison with irregular, undeveloped, defective and decadent forms of such processes as they occur in connection with individual variation, with the

[1] Read before the American Psychological Association Jan. 1, 1902.

consequence of mental stimulation, or in disease. This program, which could readily be expanded, is even in outline a most extensive one—rich in detail, fertile in mutual suggestiveness of its parts, possibly momentous in its practical consequences. The conclusion is obvious that for a host of comparative purposes the determination of norms or standards of functional mental efficiency is indispensable. That such determination involves conventions and artificialities is true and proper and inevitable. But neither is a foot, nor a meter, nor a candle-power, nor a volt, nor an ohm a natural and predestined *ding-an-sich*. Yet the arbitrary and conventional character of these units does not interfere with their utility. I am not advocating a ready-made mental yard-stick which shall show in what measure all men are not equal, and how each may discover the thumb marks of his individual success or failure. All this has been attempted before, and with necessarily futile results. The problem is recognized to be one of a general statistical nature, freighted doubtless with practical consequences, but the application of which must always be uncertain and dependent for its success upon judgment and insight.''[1]

These functional tests may be divided into two classes. To the first class belong the tests on single acts, such as an act of discrimination, memory, or voluntary control. The commonest of these are tests on the senses. To the second class belong tests on the repetition of one or more such processes, with continuous effort and without interruption, for seconds, minutes, or hours. Such continuous exertion of effort is commonly called mental work, and the amount of work done is estimated in terms of the results accomplished. The tests which I shall describe belong to this second class.

What is mental work, and in what sense can we speak of

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[1] JASTROW, Psychol. Rev., 1901, VIII, 12.

measuring it? These two questions are naturally suggested by the title. Work is usually defined in terms of physics. By analogy we naturally think of mental work in terms of mental energy but an attempt to define this would lead us into endless difficulties. I therefore simply beg leave to use the expression mental work in the popular acceptation of the term, without involving any metaphysical implication concerning the expenditure of energy or any explanation of the causal relation between mental and physical processes. In the same manner, I must use the term measure in the freest sense. The idea of quantity is involved when we say a man solved ten problems, learned thirty verses of poetry, or settled five cases of dispute in an hour. But we should hardly speak of measuring these results, because the common units are too indefinite. The problems were not equally difficult and the man profited by practice; the verses differed and were not equally well learned; and the cases involved many variables. The determining of the amount of such work has ordinarily no scientific value because we cannot define the processes or control the conditions. We may speak of measuring the results of an act in proportion to the extent to which it is well defined and constant, and the conditions are known and controlled. The following description will show in what sense this can be accomplished. In professing to measure mental work, I simply attempt to answer with tolerable accuracy four questions in regard to the mental working capacity under given conditions: What? How much? How well? With what variations?

In dealing with this problem we are yet on the stage at which we must labor to perfect methods and means of measurement. The real problem will lie over, and many new ones will arise, while the experimenter devises tests and engages in a critique of method. Good progress has been made in the development of the first class of functional tests, but I am not aware of any generally accepted

test for the second class. Nor dare I hope that the principles herein set forth will prove a panacea. During the last three years I have devoted some time to the study of this principle of measuring mental work. One of the results is the elaboration of a principle of measurement and the devising and testing of apparatus for the purpose. The apparatus has received the descriptive name *psychergograph*, because it is a means of measuring mental work in the sense indicated above. In order to make the description clear and concise it will be necessary for me to make direct and unqualified statements and beg in return of the psychologist to supply self-evident qualifications.

In the designing of the psychergograph, I set myself the following aim: To devise means by which it shall be possible (1) to call forth a relatively simple and definite complex of mental activity, (2) to repeat the same for any desired length of time without interruption, and (3) to measure (a) the amount of work done, (b) the time taken, (c) the quality of the work, and (d) fluctuations in speed and quality.

A simple case of discriminative action seems best to serve as object of measurement. As a typical setting, I adopted the following: *Given one of four known signals, to recognize it and make the corresponding one of four simple responses.*

The apparatus consists of two distinct parts, the stimulator and the recorder. The stimulator makes a series of quick exposures of different signals. The observer is required to respond to each signal by a selective reaction. Every reaction brings out a new signal. The recorder makes a permanent graphic record. The two parts of the apparatus are seen in Fig. 1, the stimulator to the left and the recorder to the right.

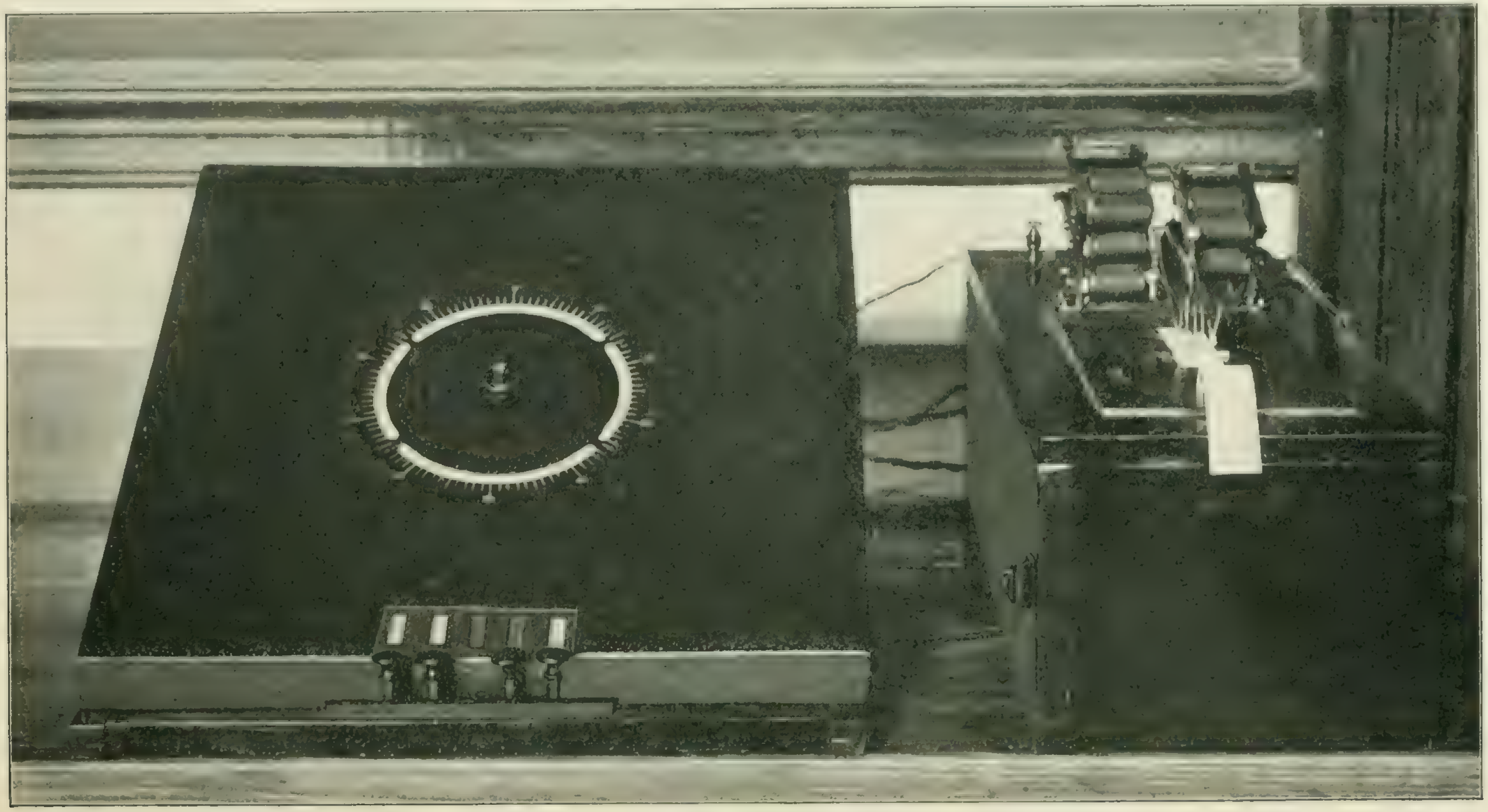

FIGURE 1. THE PSYCHERGOGRAPH

The stimulator is seen as a plain case, 40 cm. square, with a slanting cover. Near the front edge of the cover is a signal window, 8 mm. wide by 20 mm. long, through which the signals are seen. One hundred signals are pasted or printed on a paper disk, 38 cm. in diameter, so that when the disk revolves they are seen singly in succession right back of the signal window. The paper disk is clamped on a metal wheel which has fifty teeth on its edge. This wheel is energized by a strong clock spring which revolves it and the disk one hundredth of a revolution, thus exposing a new signal, every time the detent which holds it is released. This detent is in the form of a lever escapement and is operated by electro-magnets. The manner in which it operates will be understood after the reaction mechanism has been traced. Four reaction keys are seen back of the signal window. There is a signal index on the surface in front of each of these. When a new signal appears, the observer responds by pressing the key indicated by the signal, and the slightest movement of the key instantly causes the next signal to appear. The connection between the key and the signal disk is electrical. There is one pair of electro-magnets on each side of the escapement detent. These are connected, through a battery, one with each side of a small rotating commutator. The commutator is in turn placed in circuit with a battery and the series of keys. Whenever a key is touched, this circuit is broken and the commutator turns one notch, thereby switching the current from one side of the detent to the other. This alternating of the current causes the detent to oscillate and allow the signal disk to move forward by one signal space for every movement of a key.

A circle of the revolving disk is seen through the cover. On this there is a cross line (at 43 in Fig. 1), which passes before the circular scale of a hundred units and indicates to the experimenter which signal is in view. This indicator serves at once as a counter of the number of acts

performed and as a guide for the beginning and the ending of a series. The order of the signals is determined in the making of the series, either by chance or by some suitable system. The experimenter, therefore, knows the actual sequence of the signals in every series, but the observer has no means of knowing at any time what signal will appear.

The spring is wound by a detachable key that fits on the projecting axis. All the electrical connections are made through binding posts and switches on the back of the stimulator.

The recorder furnishes a continuous tracing of the action of each of the keys in the stimulator, and parallel to these, a time-line. The essential parts of the recorder are electro-magnetic markers, a record tape, a motor, and an interrupter.

The case of the recorder is 40 cm. long, 20 cm. wide, and 17 cm. high. Five markers are built up into a system on the surface, connected with batteries, and furnished with lead tracing points which mark five parallel lines upon the tape. An interrupter, to give a time-line, is placed in the circuit of one of these markers. The form and frequency of the interrupter depends upon how fine detail is wanted in the record. For seconds or half-seconds, some form of pendulum such as metronome or clock contact may be used; for tenths of a second the contact breaker made by the Cambridge Instrument Company may serve. Each of the other markers is connected electrically with a key in the stimulator so that whenever a key is pressed the circuit through that key is completed and the connected marker makes a jag in its line, thereby showing which key was pressed and the time relations of this act. Each marker is capable of three adjustments by means of thumb screws. The connections between the keys and their respective markers, and between the interrupter and its marker are made through the binding posts

on the top of the recorder and the back of the stimulator. The four markers have a common terminal and a single battery. There is a place for dry-cells inside of the case and either these or outside batteries may be used for the two circuits.

The record is made on common paper telegraph tape an inch wide. From a suspended spool in the case, it comes to the surface through a slit and is drawn under the writing points at a constant rate by a strong clock-work motor. The motor draws the tape by means of friction rollers and feeds it out free and in sight as it is shown in the Figure. In the present model the speed of the motor cannot be varied through a sufficiently large range. There should be three possible speeds, adapted respectively to the second, the tenth of a second, and the hundredth of a second reading. The motor is started and stopped by means of a button on the side of the recorder not seen in the Figure.

The apparatus is built in two pieces, in the first place, because it may be desirable at times to have them in separate rooms in order to reduce the disturbance. In the second place, this recorder—a multiple recorder—is useful for many other purposes in the laboratory. It is much more convenient than the kymograph in most cases of chronographic records in which the amplitude of the vibration is not an element in the measurement. In this respect it presents many points of advantage over the ordinary chronographs. Then, again, the stimulator may be used to advantage without the recorder in certain experiments. The total time of a series of acts may then be taken with a stop-watch and the trained experimenter can see directly whether any mistakes are made and what they are. The stimulator is then properly called a psychergometer or psychergoscope, as the amount of work done is read off directly by sight. This method can be employed only in certain crude forms of experiments.

Only a general scheme of the mechanism and the action

of the apparatus has been given. Its operation is simple. When a key is touched, a signal appears; the observer touches the corresponding key, and the marker connected with this key makes a jag in its tracing; the pressing of this key calls forth the next signal, and this process may be continued as long as one may desire. If the wrong key is pressed, the jag will occur in the wrong line. If there is any delay or interruption, this is shown by reference to the time-line. If there is any fumbling that will be shown by the duplication of jags.

From the experimenter's point of view, the operation of the apparatus is completely automatic. He has only to start the recorder by pressing a button and give the signal to begin. The personal equation of the experimenter is therefore completely eliminated. The records are permanent and may be read at leisure.

Figure 2. Section of a Record

A section of a record is exhibited in Fig. 2. The upper line is the time-line divided into tenths of a second. The remaining four tracings are made with the markers so connected that whenever a key is pressed both the marker connected with that key and the marker next to the time-line respond. This double recording lends clearness and accuracy to the record. The time relations are all recorded on a single line, the one nearest to the time-line. The jags in the other lines simply show which key was pressed. The record represented in Fig. 2 shows that the keys were pressed in the following order: 3, 2, 1, 3, 4. Of course, when the first key is pressed there will be only one mark, as for the third act in the Figure.

A record of this kind is easily read. Take a long tape containing five hundred acts. It is first necessary to compare it with the standard series in order to trace the number and the nature of the errors. A list of the order of the signals is arranged as in the accompanying table, showing a standard record of a series of signals—or its equivalent, the required order of the responses. Here one hundred

A STANDARD RECORD.

1	2	1	3	4	1	4	3	1	2
1	3	2	1	3	4	2	4	1	2
1	4	2	4	2	3	1	2	4	3
4	3	2	1	2	1	2	1	3	4
3	4	1	3	4	1	4	3	2	3
4	2	3	4	2	1	2	4	1	4
2	3	4	1	4	1	2	4	1	3
2	1	2	1	3	2	4	3	4	3
2	1	4	3	4	3	1	2	3	1
4	3	2	3	4	1	2	3	2	3

acts are grouped by fives and by tens. The grouping into fives is made in order to facilitate the comparison. In the table one may see the order of five acts at a single glance and then, by a glance at the record, determine the errors. Thus, the fifth line of the first section of the standard table reads, 3, 4, 1, 3, 4; but the corresponding section of the record, shown in Fig. 2, above, reads, 3, 2, 1, 3, 4. This shows that an error was made in the second act; the response was made to the second signal instead of to the fourth. Should this error occur with exceptional frequency, the cause of it may be readily traced.

In order to determine the fluctuation of working power in a long series of acts, I have found it convenient to divide the acts into groups of ten by a check mark on the time-line after each group in the first reading. This is facilitated by having the table in the present form.

Three kinds of time measurements may be obtained by examination of the tracing of the first key, which lies adjacent to the time-line. First, the time of the entire series is found by counting the number of seconds from the beginning to the end. Second, it is economical, and important otherwise, to compare the time of regular groups of acts. A specimen table of successive groups of ten acts each is appended below. This is a very good record. The

TIME OF SUCCESSIVE GROUPS OF TEN ACTS EACH.

Observer, F. B. 497 acts. Total time, 296 sec.

6.0	6.0	6.5	6.0	6.5	6.0	6.0	6.0	6.5*e*	6.0
6.0	6.0	6.0	6.0	6.0	6.0	5.5	6.0	6.0	6.5
6.5	6.0	6.0	6.0	5.5	6.5	6.0	6.0	6.0	5.5
6.0	6.0	6.0*e*	6.0	5.0	6.0	6.0	6.0	6.0	5.5
6.0*e*	6.0*e*	6.0	5.5	5.5	6.0	6.5	6.0*e*	5.5	

observer shows remarkable self-control and power of endurance in continuous mental effort. The experiment was preceded by only two minutes of practice. The total time is only a little shorter than the average, but the constancy throughout the series is the distinctive characteristic. Only four errors were made. These are indicated by the small letter *e* for the groups in which they occurred. Most observers exhibit rhythmic fluctuations in speed and accuracy and characteristic variations with the progressive fatigue. Some work themselves into a "heat" and do better and better until some form of collapse occurs; others show progressive "disintegration" in loss of speed and accuracy, or in both. Third, the time of each individual act is obtainable. This may be taken in tenths or hundredths of a second and serves as a basis for the study of the variations within the group. In the specimen record

given, (Fig. 2), for example, the error in the second act caused a lengthening in the time of the third act.

What has thus been briefly described is the machine record. But every psychologist knows that this must be supplemented if any exact significance shall be ascribed to it. First, record should be made of the mental and physical condition of the observer before the test. The details of topics for these preliminary notes would depend upon the particular purpose in hand. Apparently trivial notes upon the previous and the present condition of the observer, if judiciously taken, may become complete explanations in the interpretation of peculiarities in the records.

Second, after each trial the observer must give an introspective account, as full as possible. He should especially try to account for the errors and describe the nature and the apparent causes of confusion states, periodic relaxation, progressive exhaustion, the struggle to inhibit automatisms, the intrusion of foreign trains of thought, and associations favorable or unfavorable. Experiments may be instituted for the express purpose of comparing the introspective account with the objective record.

Third, the experimenter has nothing to do but to watch the observer. He should make an objective study of conditions and effects, and should take private notes on the attitude of the observer in regard to interest, effort, strain, expressive attitudes, etc. He can make pencil checks on the record at the time to indicate parts to which the notes refer.

To pile up graphic records without some such supplementary notes as have been indicated would be sheer waste. The psychergograph gives us an accurate, unbiased record, but the real significance of this record depends upon our ability to account for the conditions which are elements in the process measured.

We may now turn to a brief outline of the possible vari-

ations in the experiments. The apparatus admits the introduction of a variety of signals. What the signal shall be depends upon the purpose of the measurement. It is often desirable to make the case as simple as possible. Then we reduce the difficulty of the discrimination to a minimum by making the signals as different as possible. We may take a color, a letter, a circle, and a dot. Or, we may take four readily distinguishable letters, geometric figures, pictures, words, or colors. In the test described thus far as typical, four clearly distinguishable colors were used. The degree of difficulty in the discrimination is a controllable variable and may itself be the object of measurement. Various degrees of small differences in the shade of colors, confusing geometric figures, or any other objects in which the degree of resemblance can be varied and described may be employed. Then again the higher processes in the act may be varied and complicated in many ways by requiring certain restricted associations, memories, judgments, and decisions based upon the recognized signal. In fact, all the variations and complications of the usual visual reaction experiment, both simple and complex, may be introduced, and provision is made for the uninterrupted and long continued repetition of the selected act.

The number of kinds of signals is not fixed. The apparatus makes it possible to use a hundred different signals, if so many should be wanted, as might be the case for example in a classification test in which the observer would be required to divide the whole series of objects into a small number of classes on the basis of certain class characteristics. But for most purposes it is best to use four signals, have an equal number of each kind, and have them of equal discrimination value.

Experiment has demonstrated that there is great advantage in using just four keys. The first model of this apparatus was made with ten keys. Unless one wishes to make the crudest kind of experiment, it is necessary to commit

to memory the order of the signal indexes before beginning the experiment. Ten proved entirely too confusing, because there was so great difference in the time that it took to find different keys, even after they had been well learned. Experiments were then made with six keys but even this was too many. Finally the system of four was adopted. In a series of discriminative actions like this, it is really necessary to have as many as four different ways of reacting in order to prevent anticipation, but very little is gained for this purpose by having more than four.[1]

The length of the experiment may be varied. The observer must work with a maximum degree of effort and there is no rest and no relief, because the work is uniform and continuous, requiring uninterrupted effort from beginning to end. Under these conditions a hundred acts is a great task and for many purposes that length of a series is satisfactory. But no one will learn the order of a hundred indifferent signals of this kind the first time he goes over them, especially if his effort be concentrated upon speed and accuracy. Furthermore, the observer has no means of knowing in what part of the series he begins or how he is progressing, because a small screen is placed in front of the scale. Therefore he may continue and go over the same series several times without learning to foresee a single sequence. Much depends upon what plan is fol-

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[1] In the discussion of this paper before the American Psychological Association, Professor Jastrow stated that it was not necessary to construct a special psychergograph because a typewriter may be used, and he recommended the Oliver machine on the ground that it prints the letters in plain view. So far as I am able to see, there are close restrictions to such use. Suppose, as was suggested, that the labels on the keys are interchanged by pushing lettered paper caps over the keys. If only a small number of keys should be used, the observer would soon learn this little series within which no variation obtains. If, on the other hand, a large number of keys should be used, it would take longer to learn the series but gross confusion would ensue in hunting for the keys. The difficulty is this: the typewriter key always prints the same letter throughout the series, whereas the psychergograph key may call forth any one of the total number of signals. This is an essential and radical difference. There are, however, many tests on routine processes that may be made with the typewriting machine.

lowed in the arranging of the series. If the order is determined by chance, two or more signals of the same kind might come in succession and these might serve as a clew to memory. Some order like that shown in the specimen series given on page 10 is better. If several tests are to be made in each sitting and to be repeated on several days, a different series of the same kind may be used for each test. But very many repetitions of the same series may be made without any disturbance from memory, if proper precautions are taken. In fact for some purposes, it is necessary to use the same series in a large number of successive trials.

The index signals are placed at the same level as the signal window and the keys, and they are all so close together that it is not necessary to move the point of regard from the signal window in order to see the index and the keys. The position of the four indexes are, however, learned before beginning the test and, for long experiments with the same order of signal indexes, it is best to have them covered and trust entirely to memory.

One important means of varying the experiment, as in the study of habit, consists in changing the order of the index signals.[1]

The bodily movement in the process, the pressing of the keys, is reduced to a minimum and a constant.

This outline of the construction of the apparatus and the method of measuring mental work is reported apart from its connection with any specific problem of research partly because, like the chronoscope or the kymograph, the psychergograph is adapted to a variety of uses. One investigation now in progress with this apparatus and method is on the subject of fatigue.

There are certain objections to this apparatus; for instance, its action is not soundless, it requires batteries, and it is rather expensive. But if the principle of the experi-

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[1] Some of these variations are suggested by Jastrow in his description of a sorting apparatus, Psychol. Rev. 1898, V, 297.

ment is correct, these objections may be overcome in time. This is the third model. The three have been built upon entirely different mechanical principles. In the first model, a column of celluloid disks was fed up to a signal window by a spring and at each reaction an electric hammer knocked away one disk, thereby exposing another. The same kind of record was obtained as in the present model, but through a simple mechanical action. This first model is so simple that it can be built in two days, by one man. I have used it successfully in more than a hundred experiments. For the one experiment, with colors, it is satisfactory, but it is limited to that use. The second model was a belt machine, the signals being carried on a belt which moved before the signal window. This proved unsatisfactory, probably on account of the imperfect workmanship in it. Thus there are many ways in which the main end may be accomplished. Then again, it requires only a minor step to make adaptations for the use of other sense stimuli, such as auditory or pressure stimuli.

The record may be called a psychergogram. It is truly a measure of mental work. First, it gives the amount of a particular work done. Second, it gives the time of each act, the time of small groups of acts as by fives or tens, and the total time of the series of acts. Third, it gives a quantitative expression to the quality of the work in terms of the number of errors, the sequence of errors, and the classification of errors. These are the mechanical elements in the record on the tape. Every feature of the record is a fact and these facts may be reduced to statistics, provided the conditions are observed and taken into account in the interpretation. He who wants the facts must labor to observe the conditions. There is a class of would-be observers who, when they see the beautiful automatic operation of the psychergograph, rejoice that they have at last found an automatic accumulator of statistics on mental processes. The psychergograph is not dedicated to them.

A VOICE TONOSCOPE [1]

BY

C. E. SEASHORE

The study of motor processes has remained comparatively neglected in experimental psychology. It is not that the motor processes, as compared with the sensory, are less significant, of a baser sort, so much simpler, or fewer in number. It is because the analysis of the one naturally precedes the analysis of the other. The study has been pursued in the natural order in this inceptive stage of the science, but interest now begins to center upon the study of the motor process, not only because the motor process is the outcome of, and the sequel to the sensory process, but also because it is the practical phase of life.

In the psychology of music, the time has now come to begin to turn from the study of the hearing of tones to the study of the singing of tones. But, as we have had need of delicate tone-giving instruments in producing and gauging stimuli for the sensory processes, we now need, in addition, the corresponding means of measuring the results of motor processes. The lack of such instruments is conspicuous. The unaided ear will not serve the purpose. The graphic and photographic apparatus in use are too cumbersome. It is difficult to obtain a simple and at the same time accurate and ready means of measuring the pitch of tones produced by the human voice.

[1] Read before the Section of Anthropology of the American Association for Advancement of Science, Jan. 1, 1902. The name "tonometer" was then used instead of tonoscope, but it is apparent that the latter term is preferable because it distinguishes this instrument more completely from other tone-measuring instruments.

The apparatus that I am to describe is somewhat complicated in construction, but when once built it is easy of manipulation, can be used for rapid work, and can be made sufficiently accurate for all purposes of routine measurements for which it is adapted. It is intended to be used in measuring the pitch of the human voice in singing and speaking; and, since it causes the vibration-frequency, which denotes the pitch of the tone, to be *seen* directly when the tone is sung, this apparatus may pass by the descriptive name of voice tonoscope.

The voice tonoscope is constructed on the principle of the stroboscope; that is, the vibrations of the voice are made visible upon a moving surface by the action of intermittent light. I take pleasure in acknowledging my indebtedness to Professor Scripture, who first employed this method of measurement in an exercise on the reproduction of tones.[1] The apparatus is "built up"; the individual parts, such as the manometric capsule, the vacuum tube, the stroboscopic disk, and the double contact fork, are used in physics, but Scripture was the first to use them in this way for psychological purposes. The special feature of the present apparatus is the stroboscopic screen, shown in Fig. 3, but a brief description of the complete apparatus and of the methods of measuring by means of it will be given.

The following are the essential parts of the apparatus: (1) means of producing a standard tone, (2) a special moving stroboscopic screen, (3) means of projecting the vibrations of the standard tone upon the screen, and (4) means of projecting in a similar manner the vibrations of the tone sung. These parts will be described in the order in which they have been mentioned.

The standard tone, from which the singer takes his key

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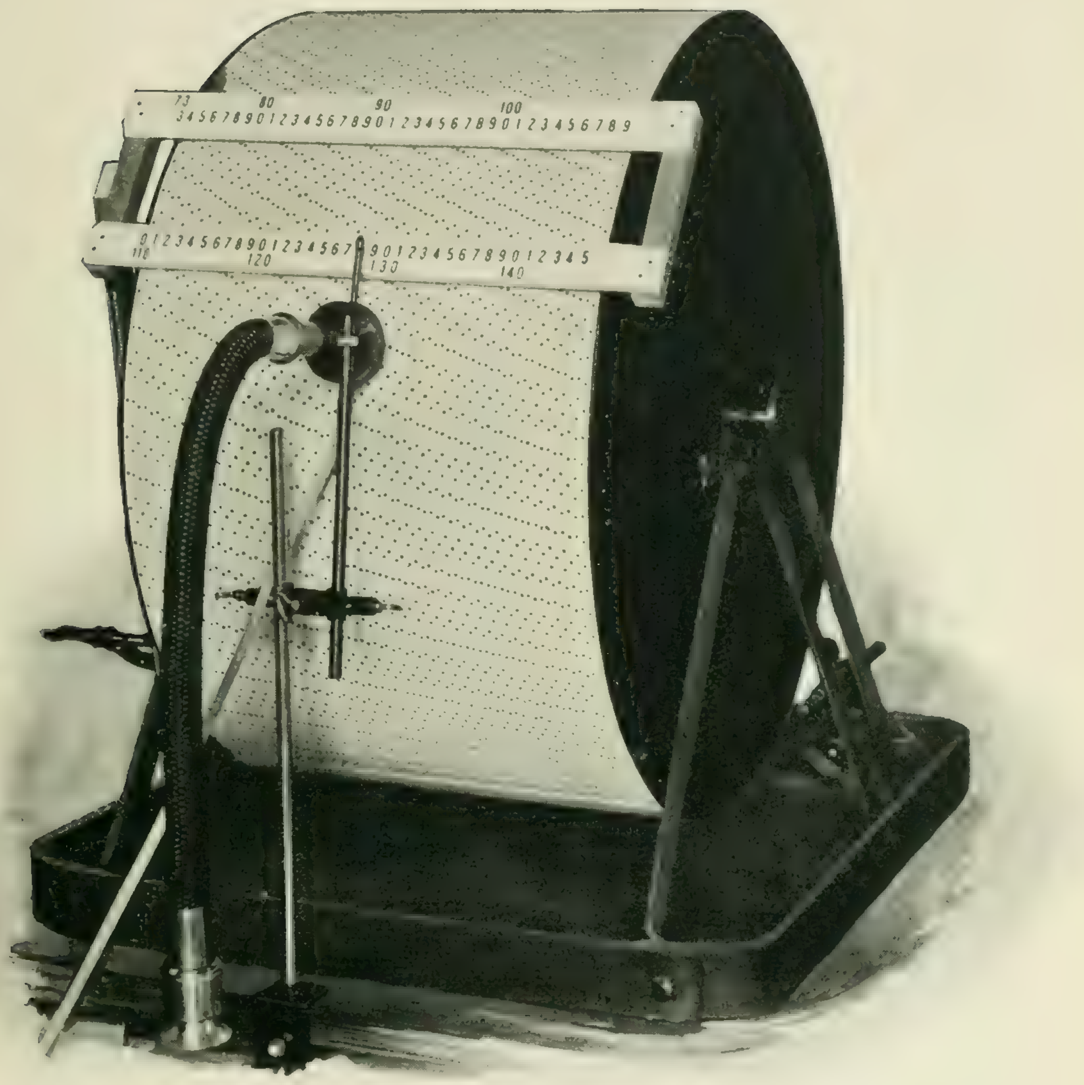
[1] SCRIPTURE, *Elementary Course in Psychological Measurements*, Studies from the Yale Psychol. Lab., 1896, IV, 135.

and by which the standard of measurement is determined, is produced by an electric fork in a distant room and is heard through a telephone receiver which is in shunt circuit with the fork. A switch makes it possible for the experimenter to sound the standard tone at required intervals of time; or the observer may himself regulate this roughly by moving the receiver to and from the ear.

The stroboscopic screen is made in the following manner: A metal drum 50 cm. wide and 50 cm. in radius is mounted on ball bearings and revolved by an electric motor. A heavy white paper is stretched around the drum and on this paper seventy-one parallel lines of dots are drawn. The dots are 3 mm. in diameter and equidistant and the lines extend through the complete circumference of the drum. The lines of dots are divided into two series. The first series contains thirty-six lines drawn equidistant and so spaced as to cover the whole screen. Their arrangement and the number of dots in each is shown by the numbers in the upper section of the scale seen in the Figure. The first line has 73 dots and each succeeding line in the series has one more dot than the line immediately preceding. The second series has thirty-five lines and these alternate with those of the first series. This arrangement and the number of dots in each line may be seen by the numbers on the lower section of the scale. As regards the number of dots in each line, the second series forms a direct continuation of the first and the dot-frequencies of the two series together, therefore, extend from seventy-three to one hundred and forty-five by a uniform increment of one dot for each line. The aim in the arrangement of the dots was to secure dot-frequencies which should correspond to the vibration-frequencies of all tones within the range of the human voice. In musical terms, we have here what corresponds to the vibrations of all tones within the octave 73 v. d. to 146 v. d. The stroboscopic effect is such that the dot-frequency for one



THE TONOSCOPE

octave will serve quite as well for tones in higher or lower octaves, as will be explained later.

The standard tone is reproduced to vision upon the screen. The accessories for that purpose are not shown in the illustration. On the back side of the drum, there is a scale like the one seen on the front side and occupying a corresponding position. A vacuum tube, 30 cm. long, is fastened at the upper edge of the scale and supplied with a reflector which throws the light on the scale and prevents it from shining over the drum. The terminals of this tube are connected with the secondary terminals of an induction coil. The primary circuit of the coil is completed through a battery and the double contact fork that produces the standard tone. The room is darkened in order to secure the intermittent light effect. Each vibration of the fork breaks the circuit and the resulting spark produces a flash of light in the vacuum tube so that the screen is lighted up once for each vibration of the standard fork. Suppose that the fork is a 100 v. d. fork and the drum is revolving at the rate of one revolution per second. Then, when the dots on the revolving screen pass under the intermittent light, the line which has one hundred dots appears to stand still and is clear and well defined, because the frequency of the dots is the same as the frequency of the lights: every time an image of this line is cast upon the retina the dots appear in the same position. The dots in all other lines appear blurred and moving, assuming more or less the appearance of gray streaks. It is therefore easy to detect the line that is wanted. The purpose of this contrivance for projecting the standard tone is merely to secure the means of regulating the speed of the drum. Thus, in the present case, the speed of the drum is regulated so that the one hundred-dot line stands still continuously. This line then becomes the standard for the visual scale in terms of which the measurements are to be made. Any other tone projected on the same screen in a similar

way will make some other line stand still and the pitch of that tone will be indicated by the number of that line, or a multiple of it; e. g., if the second tone makes the 105-line stand still, it is a tone of 105 v. d., or some multiple of that number, as 210 v. d. or 420 v. d. Which octave it is in can readily be told by the practiced ear but it is also shown by the formation of the dots.

The tone that is sung is projected on the same screen, in a similar manner, although by different means. In this case the intermittence of the light is produced by means of a manometric flame on the side of the drum opposite the vacuum tube. The manometric capsule, with the connected gas tube and the speaking tube, is seen in the Figure. The observer holds the speaking tube before his mouth in the way such tubes are usually held, and sings the tone that is to be determined. The tonal vibrations cause sympathetic vibrations of the little gas flame in the capsule so that the flame rises and recedes once for each vibration of the vocal organs. This gives an intermittent light like the one produced in the vacuum tube on the other side of the drum. When the screen is moving at the standard speed, that line of dots will stand still which has dots to correspond to the frequency of vibration of the tone, and the pitch of the tone is indicated by the number of the line that stands still. The pitch may be seen as soon as it is heard because the visual and the tonal effects are synchronous.

This summary description of the essentials of the apparatus also indicates, in a general way, the mode of procedure in experimenting. It reduces itself to this: the singer takes the key tone from the receiver and sings a tone before the speaking tube; the vibrations are projected on the moving screen and the pitch of the tone is seen on the scale.

Although the apparatus consists of several parts, only the drum with its screen is made for this specific purpose.

The other parts are such as are found in any ordinary physical or psychological laboratory, and need not be described in detail, but some further particulars about the construction and use of the screen are necessary.

The arranging of the lines into two series is important. It is necessary to have all the lines within a certain compass because the necessarily small light will not serve for more than a limited space and the spreading of the record over a large surface would therefore interfere with quick reading. This drum was made as wide as is practicable under the named restrictions. An attempt was made to put the lines in a single series and it was found that then only about thirty lines could be put on the screen and be read satisfactorily. This number of lines would cover only a part of an octave within the desired range of tones. At least seventy lines are needed. By the double series plan it is possible to put seventy-one lines on the space occupied by thirty in the single series plan; and it is easier to read on this screen of seventy-one lines than it was on the original screen of thirty lines covering the same space. The reason for this lies in the fact that by the double series plan no two adjacent lines have nearly the same dot-frequency; consequently the line on each side of the one that stands still appears as a gray streak and forms a sort of frame for the line one is reading. This has two effects: in the first place, the two series may be placed on the same screen without causing any interference; in the second place, the one series of lines makes the other clearer, so that it is possible to put the lines closer together. Having separate scales for the two series is also conducive to clearness. The same device, therefore, makes it possible to save space and add clearness and thus to provide for the desired range of tones.

Let us suppose that the speed of the drum is regulated by the 100 v. d. fork to make one revolution per second. Then, if the tone that is sung lies within the range of the

octave from 36 to 72 v. d., the line that stands still has twice as many dots as there are vibrations in the tone and, in the reading, the number on the scale is simply divided by two; e. g., if the 75-line stands still the tone is 37½ v. d. The ear readily reveals whether or not the tone lies within the low octave and these low tones can be sung only by the male voice. For tones from 73 to 146 v. d., the reading is direct from the scale. This represents the middle range for the baritone. For tones within the octave from 146 to 292 v. d., the line that stands still appears to have twice as many dots as it actually has. That makes it easy to recognize this octave and, in the reading, the number on the scale is doubled: e. g., if the 75-line stands still, the tone is 150 v. d. This octave represents the upper range for the male voice and the middle and part of the lower range for the female voice. From 292 to 584 v. d., there will be four vibrations for each dot in the line that stands still and the numbers on the scale must be multiplied by four. These multiples may be placed in small figures on the scale. The same principle of reading may be extended to higher octaves in so far as it depends upon the screen, but there is a limit set by the degree of sensitiveness of the diaphragm in the manometric capsule.

If a high tone is studied, it is well to run the drum at the rate of two revolutions per second. This can of course be regulated by the 100 v. d. fork. Higher forks may also be used for the sounding of the key-note after they have been tuned with reference to the 100 v. d. fork. The tonoscope may be used in tuning them.

It is only relatively true that a line stands still; it stands still only when there is an absolute agreement between the vibration-frequency and the dot-frequency. If the vibration-frequency is a fraction greater, the dots move gradually downward, and if it is less, they move gradually upward—and the speed of the apparent motion is always in proportion to the difference. Thus, if the tone is 125.5 v. d., no

line stands still, for the 125-line moves downward at the rate of one space per second and the 126-line moves upward at the same rate; but, if the tone is 125.1 v. d. the 125-line moves downward at the rate of only one-tenth of a space per second and the 126-line moves upward at the rate of nine-tenths of a space per second. The practiced observer can therefore readily learn to read in tenths of the scale units.

The accuracy of the reading varies with the accuracy of the singing. The accuracy increases with the steadiness of the voice, and hence with the demand for accurate measurement. The more firmly the pitch is sustained, the easier is the reading. The ordinary singer wavers in the tone that he tries to sustain even for half a second and the experimenter must select the predominating pitch in it.

The drum is heavy and carefully mounted so that it runs at a fairly uniform speed, but if the speed should tend to vary, the experimenter records such variations and a corresponding correction in the readings is made from a table which is exact to one-tenth of a vibration.

The tonoscope is not constructed for any one particular psychological experiment. It is intended to be a general measuring instrument that may be employed in a number of ways. It is simply a quick and accurate means of determining the pitch of tones—not only of the human voice, but also of instruments from which the sound may be projected upon it. In coöperation with a student, Mr. Edward Bechly, I have begun a study of certain facts about the nature of the control of the voice in singing and speaking. The results are not ready for publication but some of the measurements that we have made may be mentioned in order to indicate some purposes for which the tonoscope has been used with excellent success.

1. Sounding a single tone. This measurement presents different aspects according as the tone is taken at random, is heard from a standard, or is heard from a standard and

hummed before being sung. The absolute pitch, the intensity, the mode of vocalization, and the time relations are factors that may be varied in order to determine their effect upon the pitch of the tone.

2. Sustaining a tone. "Sustained" tones vary in pitch according to a general law. The factors that influence this may be determined.

3. Singing tones at certain pitch intervals. The standard tone may be given or it may be taken at random by the voice.

4. Singing musical scales, natural and chromatic, with numerous variations in the conditions.

5. Singing a melody. There is a shocking surprise in store for every singer who tests his ability to sing even the simplest air in true pitch. He finds that his good ear has tolerated great license in the matter of fidelity to pitch. Two-part harmony, or unison singing, may also be measured by recording for one voice on each side of the drum, but this we have not tried.

6. Speaking in a uniform pitch (a pure fiction).

7. The least producible difference in pitch. This is the measurement that is of greatest value for psychology. The least producible difference is to the study of motor processes what the least perceptible difference is to study of sensory processes.

AN ILLUSION OF LENGTH[1]

BY

C. E. SEASHORE AND MABEL CLARE WILLIAMS

Two years ago, while studying the æsthetics of geometrical forms, we required the students to produce 'double squares' by direct eye estimation. The forms were produced by means of an adjustable frame and were to be made twice as long in the horizontal direction as in the vertical. Not only was the illusion of the vertical counteracted, but there appeared to be a tendency to make the horizontal distance too short.[2]

We repeated the same test with two hundred school-children,[3] ten boys and ten girls of each age from six to fifteen inclusive, and found that this illusion of length is much stronger for children than for adults, and that it decreases with the increasing age of the children. Thus, the children of six made the double square 28% too short, the 7's 24%, the 8's 25%, the 9's 14%, the 10's 10%, the 11's 11%, the 12's 11%, the 13's 10%, the 14's 8%, and the 15's 9% too short. These figures seem fabulous, especially when it is remembered that they represent the amount by which the well-known illusion of the vertical has been outweighed. The illusion of the vertical was measured in the same series and found not to vary noticeably with the age of the children.

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1 Reprinted from THE PSYCHOLOGICAL REVIEW, VII., 592–599.

2 "Forty-eight made the horizontal line too short by an average of 15 mm. (mean variation, 8 mm.), and fourteen made it too long by an average of 10 mm. (mean variation, 6 mm.). On the whole the horizontal distance was made 4½% too short." *Univ. of Iowa Stud. in Psychol.*, 1899, II., 18.

3 *Op. cit.*, p. 33.

Continuing the study of this illusion in persons who knew little or nothing about the nature of the illusion, we made a series of measurements upon university students in introductory psychology, before the subject of illusions had been discussed in class. Twenty-nine men and thirty-four women volunteered to be tested. The so-called unconscious method was followed in so far as it was possible. Most of the observers, however, surmised that some illusion was involved in the test, and the conscious or subconscious reaction to such suspicion often entered into their judgments. The records of two men were discarded because these men asserted that they had attempted to eliminate the illusion of the vertical. The observers may evidently be divided into two classes, namely, those who introduce some correction for supposed illusions and those who do not. But as such division is relative and cannot be made satisfactorily either upon introspective testimony or upon inspection of the results, we group the records together and state some of the conclusions which may be drawn from the averages of the results.

The forms to be studied were cut out of cardboard and then placed on a neutral background upon a large drawing board, which was so placed that they were 50 cm. from the eyes and at right angles to the line of regard. Some of the forms were drawn on the background, as will be explained. Every effort was made to eliminate disturbing influences from color, brightness, limiting lines and spaces, movements in the adjustment of the cards and other known sources of error. The forms of figures may be grouped into five classes, as is shown in the accompanying table. They are: (A) parallelograms, (B) a vertical and a horizontal line, (C) and (E) two horizontal lines of different length, and (D) unequal horizontal distances limited by points.

In the A-series, rectangular forms were to be produced as follows: A1, a double square, double in the horizontal

direction; A2, a double square, double in the vertical direction; A3, a half square, the standard vertical; and A4, a square, the standard vertical. The standard for all was the side of a square, 114 mm. A card 114 mm. wide and about 275 mm. long was used, and the observer was required to produce the respective forms by placing a similar card over such portion of it that the remaining part would constitute the required form. The figures in the B-series are parallel to those of the A-series and consist of only two of the adjacent lines from each form of the parallelograms. They were to be produced as follows: B1, the horizontal line twice as long as the vertical; B2, the vertical line twice as long as the horizontal; B3, the horizontal line half as long as the vertical; and B4, the horizontal line equal to the vertical standard. The variable line was 275 mm. long, and the observer covered such portion of it with a card that the remaining part of it stood in

Form.																						
Case	A1		A2		A3		A4		B1		B2		B3		B4		C		D		E	
St.	228		228		57		114		228		228		57		114		228		228		228	
	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d
M	222	9	213	10	62	3	115	3	226	7	211	6	64	3	119	2	225	8	227	11	217	7
W	222	5	214	5	63	3	116	3	223	7	216	5	63	2	120	4	226	7	227	9	216	9
Ave.	222	7	214	7	63	3	115	3	222	7	214	5	63	2	120	3	226	7	227	10	217	8
% E.	—3	3	—6	3	+11	5	+1	3	—2	3	—6	3	+11	4	+5	3	—1	3	0	4	—5	4

Form, the general shape and the position of the forms.

Case, the designation of each case in this report.

St., the required length of the measured line or distance. The unit of measurement is the millimeter.

X, the average of the results of the estimates: (M) by men, (W) by women, and (Ave.) by all together.

d, the averages of the mean variations.

% E, the differences between the estimate and the standard, stated in percentages of the required lengths. The mean variation is also reduced to percentages.

The number of trials is two for each person upon each point in Cases A, B and D, and ten in Cases C and E. The double fatigue order was observed.

the proper ratio to the standard line. In Case C the two lines lie in the same direction (horizontal) and the measurement was made as in the B-series. The variable line was placed 10 mm. lower than the standard in order to eliminate the motive to connect them. In Case D the standard distance (horizontal) was given by two points. The observer was required to place a third point so far to the right of these that the distance between the second and the third points appeared to be twice as great as the distance between the first and the second. In Case E, the figure is the same as in Case C, but the observer was required to select the limiting point before applying the limiting card.

The final averages of the results may be seen in the accompanying table. Case A1 represents the original form of the illusion. The double square is made 3% too short. This indicates that the illusion of length exceeds the illusion of the vertical by that amount, because the two illusions stand in direct opposition in this position of the figure. If only the illusion of the vertical had been present, the double square would have been made too long. Case A2 shows the effect of the coöperation of the two illusions. Here the double square is made 6% too short. We have found, in a large number of other experiments, that the two illusions in coöperation produce an illusion equal to the sum of the two illusions acting singly; still that is not always to be expected, because the effect of one illusion partly satisfies the motive of the other. Case A3 was introduced as a check upon the method of making the comparison and the measurement. In shape this figure is identical with A2, but it differs from it in size, and was produced by varying the shorter dimension. This half-square was made 11% too wide. In all our experiments it is found that when the vertical distance is varied there is a stronger tendency to correct the known illusion of the vertical than when the horizontal distance is varied. This is because more atten-

tion is called to the presence of that illusion by the former method. That law does not apply to the illusion of length, because the observers had no means of knowing that such an illusion existed. As will be shown later, the difference in size does not account for the difference in the illusion for the half-square and the double square. Case A4 contains only the illusion of the vertical. The horizontal distance is made 1% too long, but that is not a representative figure. Some four hundred measurements upon students gave an average of 5% for the illusion of the vertical. It is difficult to say how far this tendency to correct the illusion of the vertical enters into the estimation of the double square in the present experiments. It is probable that it increases the difference between the two illusions in A1, and that it decreases the resultant of the two in A2 and A3.

We conclude from the study of the A-series, then: (1) that the illusion of length obtains for both the horizontal and the vertical positions of the double square, (2) that it is stronger than the illusion of the vertical, and (3) that the tendency to make unconscious corrections for an illusion is greater when the image of the illusion is clearest in the mind.

The B-series was so arranged as to determine whether the illusion is a linear illusion or is peculiar to the comparison of the two dimensions of rectangular areas. The relations of the dimensions of a parallelogram may be estimated, (1) by comparing a vertical margin with a horizontal margin, (2) by comparing the length of imaginary lines that cross at the center of the figure, and (3) by judging by the total impression with reference to the areal content without distinctly selecting representative lines. The first method is the easiest and the most accurate, and was followed by nearly all our observers. When the second method is employed, the illusion is increased on account of the increased vagueness of the parts to be compared. The

third method is the most valuable and probably entails the greatest illusion. Children in their quick and spontaneous judgments follow this method, as a rule, and this accounts partly for the strength of the illusion with them. When the first method is employed we have essentially the same conditions of comparison in the A-series as in the B-series. The average of the results for B1, B2 and B3 are equal respectively to the results of A1, A2 and A3. B4 shows a normal illusion of the vertical.

From the comparison of the results of these two series of tests, we conclude that the described illusion of length in surfaces is essentally a linear illusion.

Is this linear illusion contingent upon the difference in the direction of the two lines compared? In Case C both lines are placed in the same direction, and the illusion of the vertical is eliminated. All the results for lines lying in the same direction show that the illusion is present in such lines, but is very much diminished by the elimination of the difference in direction.

Case D was introduced in order to determine whether the illusion of distances in the same direction is contingent upon the presence of lines. The results are affirmative; there appears to be no illusion for the distance between the dots.

Case E was introduced in order to determine whether excessive eye movements constitute one of the motives for the illusion. In Cases A, B and C the comparison is naturally made by bisecting the long line visually, to determine whether one-half of the long line is equal to the length of the short line. When one-half of the line is regarded, the eye wanders beyond the middle point into the other half, on account of the guiding power of the line and the absence of a definite objective limit. This overrunning of the point of regard ought to produce an overestimation of the half that is regarded, and thus an overestimation of the whole line. Case E is like Case C in every respect except that there is

an additional motive for excessive eye movements in Case E; the eye is allowed to wander to the right beyond the limit to be selected. The results show that the introduction of this additional motive for excessive eye movements produces a marked increase in the illusion. Since this new eye-movement motive, which is present in the bisection of a line, is of the same nature as the additional one which increases the illusion in Case E, we conclude that it is one of the primary causes of the illusion of length. We may refer to this as the first motive.[1]

Contrast is a second primary motive which may be present and co-operate with the first motive. It is an application of the principle of relativity—a long line is compared with a short line, and, according to this principle, the long line should appear to be longer and the short line shorter than it really is. With young children this contrast is undoubtedly the strongest motive, because children have strong tendencies to overestimate differences. It has been demonstrated that illusions which have physiological causes, *e. g.*, the illusion of the vertical and the Müller-Lyer illusion, do not vary in a marked manner with mental development; but the illusions of judgment, *e. g.*, the illusions of contrast and illusions of time, vary very much with mental development.[2] Therefore it is probable that, if both the physiological and the psychical motives were present in those of the tests that were made upon children, the variation with age (mental development) is due chiefly to variation in the second motive, namely, contrast.

These two motives appear in the comparison of lines that

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[1] Incidentally we have here a crucial test of the eye-movement theory of the Müller-Lyer illusion. Cases C and E may be considered the simplest forms of the Müller-Lyer figure. All the motives demanded by current theories of this illusion, except one, are eliminated in these figures. The eye-movement motive is the only one present, and the illusion is present and varies with this motive.

[2] SEASHORE, *op. cit.*, pp. 33–35, 83, 84.

lie in the same plane and direction, but when the two lines lie in different directions or are parts of parallelograms, there appears in addition to these a third primary motive. It is a well-known fact that if a square and a double square appear side by side the latter will appear to be the narrower. This is not due to the contiguity of the two figures, for the dimensions of the double square do not appear to change when the square is removed.[1] Some would ascribe it to the principle of relativity, the two sides of the same figure being compared with each other. But it may be accounted for better by Wundt's theory of eye movements, *i. e.*, there is a stronger tendency to move the eyes in the direction of the longer lines than in the direction of the shorter.[2] This theory is in harmony with the most acceptable theory of the visual perception of space and is well supported by the fact that the total illusion of length is so much greater for parallelograms and lines at right angles to each other than for the lines that lie in the same plane and direction.[3] The results tend to show that this third primary motive, which has no connection with the first eye-movement motive, is, for adults, the strongest motive in the figures that contain it.

To determine the validity of the method employed, we made parallel measurements by the method of selection. The observers were allowed to select the required proportions from a series of cards. The results obtained by this

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[1] The theory that the apparent narrowing of the double square is due to an illusion of perspective is disproved by the fact that the figure does not appear to change when the square with which it has been compared is removed, and by the fact that the length of the figure is overestimated, instead of underestimated.

[2] Wundt, *Die geometrisch-optischen Täuschungen*, Abh. d. math.-phys. Cl. d. k. Sachs. Ges. d. Wiss., XXIV., 1899, 158.

[3] A part of this difference may be accounted for by the fact that the placing of the lines in different directions increases the difficulty of comparing and thus gives fuller sway to the motives for illusion that are present.

method support those obtained by the method of production as described above.

We also made a series of measurements by the method of selection to determine whether the ratio 2:1, employed above, is peculiarly favorable for the production of the illusion. The observers were required to estimate the length of cards in terms of their standard width. There appears to be a gradual increase in the illusion from its inception near the square up to the ratio 2½:1, which was the largest ratio tried. The illusion is not relatively stronger for the ratio 2:1 than for the adjacent ratios.

Does this illusion vary systematically with the size of the figures? The averages of the results for four observers who made eighty trials each upon each of four sizes of cards of the A2 type above, by the method of production, are as follows: standard (length in the vertical direction) 76 mm., illusion 7%; standard 114 mm., illusion 8%; standard 228 mm., illusion 7%; and standard 456 mm., illusion 8%. The cards were in all cases 75 cm. from the eyes. There is therefore no constant tendency for the combined illusions to vary with the size of the cards. But it has been held that the illusion of the vertical alone increases with the size of the object. We therefore measured the illusion of the vertical for the corresponding sizes of squares. The same four observers made eighty trials each for each size of card, by the method of production, with the following average results: standard 57 mm., illusion 3%; standard 114 mm., illusion 2%; standard 228 mm., illusion 4%; and standard 456 mm., illusion 3%. Thus there is no constant tendency for the illusion of the vertical to vary with the size of the object, and therefore the same is proved for the illusion of length.

NORMAL ILLUSIONS IN REPRESENTATIVE GEOMETRICAL FORMS

BY

MABEL CLARE WILLIAMS

The experiments here reported were made for the purpose of determining the chief motives for visual illusions in the form of common objects. The research was planned primarily with reference to the analysis of some of the illusions found in the cylinder, but its scope gradually widened and led to the study of several other fundamental geometrical forms. The aim of the experiments was somewhat broader than simply to verify or disprove a particular hypothesis. The natural-history method of collecting and classifying data is the predominating feature of the plan. The aim has been to gather a large number of data systematically, especially in the form of judgments of persons who were not aware of the purpose of the study and who were also often unaware of the existence of the illusions, thus employing the so-called unconscious method as far as possible. This method of procedure is especially well adapted for the study of normal illusions, and in fact is really necessary, because normal illusions change when attention is directed to them and knowledge of the illusions leads to unconscious corrections. The naive judgments give a true measurement of the force of the illusion and new illusions are most readily discovered by this method. It is the puzzling residuals that appear unexpectedly and for some time baffle all efforts at explanation that in the end furnish the most reliable and convincing demonstration of fact.

The observers may be classified with reference to three general types: first, adults having knowledge of the illusions but making no conscious allowance for them; second,

adults without knowledge of the illusions; and third, children without knowledge of the illusions. The degree of knowledge of the subject possessed by each observer was ascertained as nearly as possible and the records interpreted with reference to this knowledge. In computing the results, the method of the average value has been employed. In many instances the median value might be more representative but for the sake of uniformity the average value has been used.

Each of the chief factors which modify the illusions has been varied in turn: the size, distance, form, and position of the object; the different classes of observers with regard to age, sex and intelligence; the different subjective conditions, such as the degree of knowledge, attention, and practice; and the different methods of judging.

The order of sequence adopted in the study of the different forms is due to two factors: first, the natural evolution of the research and second, the aim to avoid in each series and in all the series as a whole the effect of suggestion.

In every series where more than one judgment upon a point was secured from each observer the so-called double fatigue order was employed; that is, half the number of trials on each point were made in going over the series the first time, and, upon returning in the reverse order, the other half of the trials were made.

The horizontal line was selected as the best form in terms of which to state the illusions in the other forms studied. The line in this position is the simplest and most fundamental form and it seems to be freer from illusions than any other form.

The 114 mm. standard was adopted partly to relate this research more closely to other experiments in the same laboratory and partly for the reason that in other tests this size had been found to be very satisfactory. Only the most typical or fundamental forms were studied, it being

assumed that what is true for these would also be true in some measure for their derivatives.

The forms which were studied are shown in Figure 1, and they are named and described in the following list.

EXPLANATION OF FIGURE 1.

1. The square plate. 114 mm. x 114 mm.
2. The cube. 114 mm. x 114 mm. x 114 mm.
3. The cylinder. Length, 114 mm.; diameter 114 mm.
4. The sphere. Diameter 114 mm.
5. The triangle. Base 114 mm.; altitude 114 mm.
6. The pyramid. Base 114 mm.; altitude 114 mm.
7. The cone. Base 114 mm.; altitude 114 mm.
8. The disk. Diameter 114 mm.
9. The triangle and plate. Base 114 mm.; total length 228 mm.
10. The pyramid and cube. Base 114 mm.. square; total length 228 mm.
11. The cone and cylinder. Diameter of base 114 mm.; total length 228 mm.
12. The circle. Diameter 114 mm.
13. The drawn square. 114 mm. x 114 mm.
14. The ellipse. Long axis 114 mm.; short axis 38 mm.
15. The drawn cylinder. Length 114 mm.; diameter 114 mm.
16. The line. Length 114 mm.

Forms 1, 2, 3, 4, 5, 6, 7, 9, 10, and 11 were made of tin and painted a lustreless black. Forms 8 and 14 were cut from dull black paper and pasted upon a sheet of light manilla cardboard one meter square. Form 1 was either cut from the paper or made of tin. Forms 12, 13, 15, and 16 were drawn upon a sheet of cardboard, the limiting lines being one millimeter wide. The end of the cylinder, Form 15, was represented by an ellipse the axes of which were in the ratio 3:1.

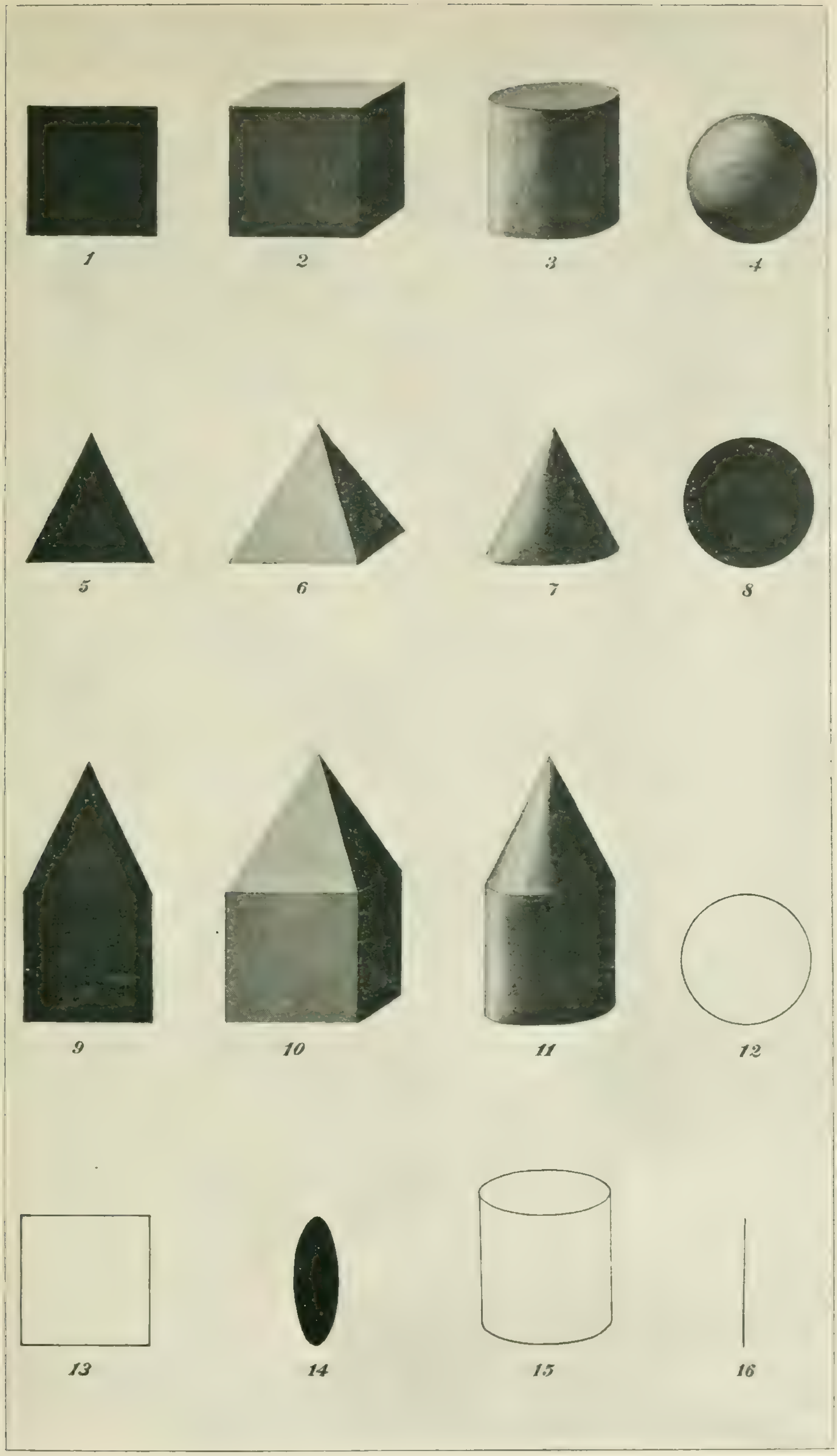

FIGURE 1

Series I

A preliminary study of the cylinder revealed a peculiar illusion which appears in the overestimation of its length. This will be called the illusion of cylinder length. The object of the experiments in Series I. was to collect measurements on this illusion in the length of the cylinder and the illusion of the vertical in the square. The experiments were made on school children in order to secure naive judgments and to discover how these illusions vary with age, sex and intelligence. They constituted one of nine systematic tests made upon one hundred and twelve pupils in the grammar school of Iowa City in the spring of 1900. The other eight tests were on different subjects, such as muscular fatigue, discriminative action, discrimination for the pitch of tones, hearing-ability, and keenness of vision. This combination of tests led the children to infer that their eyes were being tested and helped to keep them ignorant of the illusion. The experiments were made Saturday mornings. No results of this test were given out to the children.

Three measurements were made upon the cylinder and one upon the square, as follows:

Case 1. The vertical cylinder[1] at the level of the eyes: to select the one whose diameter and length are equal.

Case 2. The plate, at right angles to the line of vision: to select the one whose height and width are equal.

Case 3. The horizontal cylinder, at the level of the eyes: to select the one whose diameter and length are equal.

Case 4. The vertical cylinder, standing thirty degrees below the level of the eyes: to select the one whose diameter and length are equal.

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[1] The term "vertical cylinder" denotes a cylinder with the length in the vertical direction, and the term "horizontal cylinder" one whose length is in the horizontal direction.

The apparatus for Cases 1, 3, and 4 consisted of a series of fifteen cylinders (Fig. 1, Form 3) from which the observer was required to select the one in which the length appeared to be equal to the diameter. They were each 114 mm. in diameter but varied in length, by five-millimeter steps, from 69 mm. to 134 mm. For Case 2, the apparatus consisted of fifteen plates (Fig. 1, Form 1) each 114 mm. wide but varying in height, like the length of the cylinders, from 69 mm. to 134 mm. Both the cylinders and the plates were made of tin and were painted dull black. They were kept out of view in a case and the experimenter exhibited one at a time by placing it upon the center of a stand which was so adjusted, except in Case 4, that the middle of the form was at the level of the eyes of the observer. The stand was one foot square and was covered with gray cloth. The observer was at a distance of one and one-half meters.

The method of selection employed was the following: The cylinder whose length was equal to its diameter was presented first and the observer stated whether the length appeared to be equal to the diameter. If he said it was too long, the next shorter cylinder was presented and after that other short ones in order until the "equal" point had been passed and two consecutive cylinders had been pronounced too short; then the cylinders were presented in the opposite order until the "equal" point had been passed again and two consecutive cylinders had been pronounced too long. If, on the other hand, the observer said that the first cylinder presented was too short, the above order was reversed. Thus the judgments of the observers determined how many cylinders should be presented, and in what order, and the upper and the lower limits of the region of equality were determined twice for each observer. The procedure may be represented in the following scheme, in which the cylinders are denoted by the numbers indicating the length of the cylinder and the judgments upon

each by the letters. L denotes the judgment "too long"; E, "equal"; and S, "too short."

99	104	109	114	119	124
S	S	E	L	L	
	S	E	E	L	L

Here are two complete determinations; the first is 109 and the second is 111½. The mean between these (110, neglecting fractions) is taken as the final record. Similar evaluations were made for other sequences of judgments. The same method was employed in the selection of the square.

The results are given in Tables I*A* and I*B*, which also contain the classification of the children according to age, sex, grade, school standing, and teacher's estimate of mental ability.

In Case 1 the average illusion for the boys is 17.5% and for the girls, 18.4%. This means that if the length of a vertical cylinder is to appear to be equal to the diameter of the same, the length must be about 18% shorter than the diameter. To the average boy the length of the vertical cylinder appears to be equal to the diameter when the length is 94 mm. and the diameter 114 mm., and boys vary from each other in this judgment by an average of only 4.4%. In this overestimation of the height of a cylinder, the illusion of the vertical and the illusion of cylinder length coöperate. But, when the cylinder is laid on its side, as in Case 3, they conflict and the illusion of the vertical causes an overestimation of the diameter while the illusion of cylinder length causes an overestimation of the length in a horizontal direction. If the two illusions were of equal strength, their effects would cancel each other, but the table shows that for these children the illusion of the vertical is on the average 1.8% stronger than the illusion of cylinder length.

In Case 2 the illusion of the vertical in the square was

TABLE I. (*A*). *Boys' Records.*

Obs	*Age*	*Gr*	*St*	*Ab*	*Case 1* *E*	*d*	*Case 2* *E*	*d*	*Case 3* *E*	*d*	*Case 4* *E*	*d*
2	16	8	B	B	93	4	102	3	122	0	90	4
4	13	8	D	B	101		100	1	97		109	3
6	15	8	D	C	94	3	98	1	125	4	95	4
8	14	8	D	C	95	4	100	5	115	1	88	7
10	16	8	E	E	93	4	109	0	103	1	102	3
12	14	8	E	D	95	4	104	0	120	2	83	7
14	13	8	A	A	95	2	109	0	112	2	98	4
16	15	8	A	A	103	4	108	1	119	0	100	2
18	14	8	B	C	93	1	100	3	113	7	90	4
20	13	8	C	B	85	1	99	0	127	3	87	3
22	12	8	C	B	93	4	105	1	115	2	99	3
24	14	8	A	B	89	3	104	0	109	0	99	
26	13	7	C	B	97	3	103	1	124	0	97	3
28	12	8	C	C	95	1	107	3	103	1	91	3
30	13	8	E	D	79	5	98	4	122	3	94	
32	12	8	C	B	104	3	110	1	109	0	107	3
34	13	8	D	C	85	1	99	3	122	3	84	3
38	15	8	C	C	84	3	99	0	122	0	85	4
40		8	D	C	84	0	108	1	118	1	89	
42	14	8	D	C	95	2	105	2	108	1	97	3
44	13	8	C	B	92	3	106	3	121	2	97	3
46	13	8	C	C	87	5	100	2	120	7	88	4
48	14	8	A	A	96	0	102	0	120	2	95	2
50	15	8	E	D	99	2	99	2	109	2	94	5
52	12	8	D	C	87	5	100	3	105	2	90	2
54	12	8	B	A	99	2	99	2	112	2	100	2
56	12	8	B	A	99	0	102	0	114	0	95	2
58	13	8	A	B	98	1	104	0	113	1	92	3
60	15	8	D	C	94	3	107	3	110	2	93	1
62	14	8	D	C	90	2	105	2	107	3	89	3
64	13	8	D	C	109	3	103	4	120	3	109	3
66	12	7	C	C	92	3	100	2	112	0	88	1
68	13	7	D	C	95	4	100	2	113	4	98	3
70	15	8	C	C	104	3	102	0	114	3	99	3
72	12	8	C	C	94	3	107	0	107	0	92	0
74	14	7	D	D	98	4	105	1	109	0	84	5
76	16	7	D	D	89	3	107	3	104	3	90	4
78	14	7	C	C	80	6	97	0	123	1	82	8
80	13	8	A	A	98	1	107	3	128	1	97	3
82	14	8	A	A	98	1	109	0	114	0	99	3
84	12	7	D	C	89	5	97	3	124	0	94	0
86	12	7	C	B	99	5	107	3	125	4	97	3

TABLE I. (*A*). *Boys' Records*. Continued.

					Case 1		*Case 2*		*Case 3*		*Case 4*	
Obs	*Age*	*Gr*	*St*	*Ab*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
88	13	7	B	B	90	4	102	5	123	1	99	3
90	12	7	A	A	97	5	103	4	117	3	98	4
92	12	7	A	A	99	3	103	1	124	0	98	1
94	14	7	C	C	99	5	107	3	117	3	104	0
96	13	7	C	C	107	3	108	1	119	0	108	1
98	11	7	A	A	97	3	104	3	125	5	98	4
100	15	7	C	B	103	3	107	3	112	0	99	0
102	12	7	A	A	99	2	104	2	122	9	95	4
104	12	8	A	A	86	3	104	0	125	2	93	5
106	13	8	B	A	98	1	104	0	116	0	93	4
108	13	8	D	C	98	4	100	4	122	3	88	6
110	14	7	C	C	94	3	105	2	110	1	97	3
112	15	7	D	D	94	3	104	3	110	3	94	2
116	13	7	A	A	97	0	104	2	122	0	98	5
118	15	7	C	C	92	0	104	0	114	0	88	4
120	12	7	B	B	88	4	103	1	125	4	88	4
			Average		94	3	103	2	116	2	95	3
			Il		—20		—11		+2		—19	
			% Il		—17.5		— 9.7		+1.8		—16.7	
			% D		4.4		2.6		5.4		4.4	

Obs., the observers, by assigned numbers—boys even, and girls odd numbers.

Age, age at nearest birthday.

Gr., grade in the public school.

St., average standing according to the last monthly teacher's report.

Ab., mental ability as estimated by the teacher—five grades, A, B, C, D, and E in order, A being the highest.

E, the recorded estimate. This is the mean between two complete determinations, given in millimeters.

d, the mean variation, *i. e.*, the average deviation of the result for each trial from the average result for all the trials of the same observer, regardless of sign.

Il., the amount of the illusion, in millimeters.

% Il., the amount of the illusion, in percent of the standard distance (114 mm.)

% D., the average percent of deviation of the individual observers from the average for the group.

The minus sign signifies *below*, and the plus sign *above* the standard.

TABLE I. (*B*). *Girls' Records.*

					Case 1		*Case 2*		*Case 3*		*Case 4*	
Obs	*Age*	*Gr*	*St*	*Ab*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	13	8	C	B	89	3	100	1	118	2	85	4
15	15	8	D	D	97		107		114		97	
21	14	8	D	D	97	0	105	2	107	3	96	4
23	13	8	C	B	100	2	107	3	120	2	95	4
25	15	8	C	C	97	3	104	3	112	3	87	5
27	13	8	C	B	85	2	97	0	117	3	89	5
29	13	8	D	C	100	3	108	1	109	2	100	2
31	13	8	D	D	82	8	105	2	I15	4	89	7
33	14	8	D	D	84	3	88	1	119	3	95	4
35	15	8	A	B	93	1	100	4	112	5	93	4
37	14	8	E	E	93	4	105	2	109	0	97	3
39	14	8	E	D	84	5	104	3	94	5	74	3
41	14	8	C	C	77	3	97	5	103	2	81	0
43	16	8	D	D	83	9	98	4	117	3	80	6
47	14	8	C	C	95	2	104	0	107	0	89	3
51	13	8	C	C	97	3	105	2	112	0	95	2
53	14	8	A	A	89	3	105	2	118	2	92	0
55	15	8	C	C	98	1	104	3	118	1	94	3
57	13	8	D	C	94	5	104	3	109	3	89	3
59	13	8	B	B	104	3	109	0	118	1	102	3
61	13	8	B	C	95	4	102	3	118	1	93	4
63	12	8	A	A	95	7	105	2	119	0	94	3
65	14	8	A	A	88	4	103	1	114	0	86	2
67	14	7	D	E	109	3	109	3	118	1	107	0
69	11	7	E	E	102	0	99	0	120	1	94	5
71	14	7	D	C	95	5	102	3	118	1	90	2
73	12	7	C	B	89	3	108	1	123	4	95	4
75	11	7	D	D	96	5	99	3	109	0	90	4
77	13	7	D	D	104	5	110	2	105	2	88	5
79	11	7	A	A	85	2	99	0	115	2	105	4
81	14	7	B	B	85	6	108	1	123	1	94	5
83	16	7	D	D	92	3	104	3	118	3	92	3
85	13	7	C	C	95	2	107	3	124	0	87	8
87	12	7	A	A	99	3	104	3	114	3	94	3
89	11	7	A	A	99	2	108	1	124	3	97	3
91	13	7	B	B	98	1	103	1	122	5	98	1
93	12	7	D	D	90	4	99	3	120	2	89	5
95	13	7	B	B	85	4	99	0	124	0	95	5
99	12	7	C	C	89	5	98	1	119	3	94	5
101	13	7	C	C	90	4	107	3	120	3	94	3
103	11	6	D	C	97	3	105	2	124	3	98	5
107	14	7	D	E	92	4	103	4	114	3	90	4

TABLE I. (*B*). *Girls' Records.* Continued.

					Case 1		*Case 2*		*Case 3*		*Case 4*	
Obs	*Age*	*Gr*	*St*	*Ab*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
109	14	7	C	C	88	1	104	3	121	1	90	1
113	12	7	A	A	90	6	102	3	113	1	91	5
115	12	7	A	A	90	6	97	3	122	0	90	4
117	16	8	B	B	94	3	104	3	112	0	94	0
119	13	7	A	A	99	5	105	2	122	0	104	0
121	11	7	B	B	105	4	99	2	119	2	94	5
123	14	8	D	C	93	4	104	2	125	4	98	5
125	13	7	C	C	95	6	104	2	114	0	103	1
129	13	7	D	C	93	1	107	3	109	0	98	4
131	13	7	B	B	85	4	104	0	114	0	94	5
135	15	7	B	B	94	0	108	1	112	0	95	1
139	13	8	B	B	100	4	97	3	132		102	
			Average		93	4	103	2	116	2	93	3
			Il		—21		—11		+2		—21	
			%Il		—18.4		— 9.7		+1.8		—18.4	
			%D		4.4		2.6		4.4		4.4	

measured. It amounts to 9.7% both for boys and for girls, and the children vary from one another in this judgment by an average of only 2.6%. It may be assumed for the present that the illusion of the vertical which enters into the overestimation of the height of the cylinder is the same as that for the square, the conditions being similar and the dimensions equal. If then, in the present method of comparison, the overestimation of the height of the cylinder is caused by two illusions only, the illusion of cylinder length amounts to 8.3%, which is found by subtracting 9.7 from 18. Again, if the illusion of the vertical in Case 3 is 9.7% for the diameter and the illusion of the vertical exceeds the illusion of cylinder length by 1.8%, then the latter amounts to 7.9%. There is thus less than 1% difference in the results for these two independent determinations of the illusion of cylinder length. The records demonstrate that the illusion of cylinder length exists independently of the illusion of

the vertical and is nearly equal to this familiar illusion, from which it has not to the writer's knowledge before been distinguished.

It was supposed that the cylinder would look longer when an end was in view, as it is shown in Fig. 1, Form 3, than when neither end could be seen, as in Cases 1 and 3. But the records for Case 4 reveal no appreciable difference. That is, the illusion of cylinder length is practically the same in the three fundamental positions in which the length of the cylinder may be seen.

There is no significant difference between the records of the boys and the records of the girls. The illusion is of about equal force, the mean variations are about equal, and the variation among the individuals is about the same for the boys and the girls.

In order to determine the variation of the illusion with age, these children may be taken as a group to represent children who are old enough to be intelligent observers and still are not mature mentally. They will be compared with adults in the next series.

To determine whether or not these illusions vary with mental ability, the records are classified, first, according to class standing (Table II) and, second, according to "mental ability" (Table III). The standing represents the averages from the last monthly reports. These are divided into five grades, represented by the letters in order, A being the highest. Mental ability was estimated by the teacher who knew the children best, and is equivalent to the dividing of the children into the five groups, "very bright," "bright," "average," "dull," "very dull."

The illusion of the vertical in the square does not vary with intelligence (See Case 2 in each table).

The illusion of cylinder length shows a tendency to be stronger for the less intelligent. This conclusion rests chiefly upon Case 3, for which both tables show that, in the conflict of the two illusions involved, the illusion of the

TABLE II.

		Case 1			Case 2			Case 3			Case 4		
St	*n*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*
A	23	—16.7	3	3.5	—8.7	2	1.8	+3.5	2	4.0	—15.8	3	2.6
B	17	—17.5	3	4.4	—9.6	1	2.0	+4.4	1	4.4	—16.7	3	2.6
C	32	—18.4	3	4.4	—9.6	2	2.6	+3.5	3	4.4	—17.5	3	5.2
D	33	—15.0	4	5.2	—9.6	3	2.6	0	2	5.2	—18.4	4	4.4
E	7	—19.3	3	5.2	—9.6	2	2.6	—2.6	2	7.0	—20.2	4	6.2

n, the number of children in each group.
Other notation in this and the next table, same as in Table 1.

TABLE III.

		Case 1			Case 2			Case 3			Case 4		
Ab	*n*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*
A	23	—15.8	3	3.5	—8.7	2	2.0	+4.4	2	3.5	—15.8	3	3.0
B	27	—17.5	3	5.0	—8.7	2	3.0	+3.5	2	5.2	—15.8	3	3.5
C	41	—18.4	3	4.4	—9.6	2	2.6	+1.0	2	5.2	—18.4	3	3.5
D	16	—19.3	4	5.2	—10.5	3	2.6	—1.8	2	5.2	—21.9	5	4.0
E	5	—14.0	3	5.2	—8.0	2	2.6	—1.0	1	5.2	—14.0	3	4.4

vertical is stronger than the illusion of cylinder length for the very bright children, and the illusion of cylinder length is stronger than the illusion of the vertical for the very dull. This is also clearly corroborated in Cases 1 and 4 in both tables, for there is a variation in the combined illusion but the illusion of the vertical does not vary, as was shown in Case 2.

The figures in the %D columns show that the more intelligent children are in closer agreement with one another than the less intelligent. In other words, the illusion is more uniform for the bright than for the dull children.

The conclusions for this first series of experiments, upon the children, may be summarized as follows:

The illusion of the vertical for the square is about 10%.

The illusion of cylinder length is about 8%.

The illusion of cylinder length does not vary with the position of the cylinder.

Both the illusion of the vertical and the illusion of cylinder length are of approximately the same force for boys and girls.

The illusion of the vertical for the square does not vary with intelligence.

The illusion of cylinder length varies with intelligence.

The illusions are more constant for the bright than for the dull children.

Series II

The purpose of this series was the further development of some of the problems presented in Series I. The observers, three women and eight men, were members of the University Summer Session of 1900. They were mature students, being teachers in the public schools of the state, but their knowledge of illusions was very vague and imperfect. In general the observers knew something of the illusion of the vertical but they knew nothing in regard to the other illusions which are under consideration.[1] They were given no suggestion of the nature of the problem, either before the experimenting began or during its progress.

The study of the illusions in the plate and the cylinder was continued by repeating the four cases of Series I. The plate was also compared with lines and the cylinder with lines and surfaces. The same plates and cylinders were used as in the previous series. Twelve cases were introduced into the series as follows:

Cases 1 to 4. The same as in Series I.

Case 5. The vertical cylinder: to select a plate whose width is equal to the diameter of the cylinder.

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[1] With regard to the illusion of the vertical, the observers belong mainly to type one, as defined on p. 38, but with regard to the illusion of cylinder length and other then unknown illusions, they belong to type two.

Case 6. The plate: to mark off a vertical distance equal to the height of the plate.

Case 7. The vertical cylinder: to select a plate whose height is equal to the length of the cylinder.

Case 8. The vertical cylinder: to mark off a vertical distance equal to the length of the cylinder.

Case 9. The vertical cylinder: to select a vertical line equal to the length of the cylinder.

Case 10. The plate: to select a vertical line equal to the height of the plate.

Case 11. Vertical cylinders: to select from a series of cylinders those whose heights are equal respectively to one-half, one, two, and four times the diameter.

Case 12. Rectangular plates: to select from a series of plates those whose heights are equal respectively to one-half, one, two, and four times the width.

In Cases 1 to 4 the same method was used as in Series I. The same method was also employed in Cases 5, 7, 9, and 10, except that the dimensions of two different objects instead of two dimensions of the same object were compared. For these four cases two large backgrounds, covered with seamless gray cloth, stood at right angles to each other; the standard form was placed in the center of one of these and the form to be compared with it in the center of the other. For Cases 9 and 10, a series of fifteen wires, 2.5 mm. in diameter and varying in length from 69 to 134 mm., served as lines. A small hole, scarcely visible at the observer's distance, was drilled near one end and the wires were hung upon a projecting pin as needed.

In Cases 6 and 8 a method of production was used. Two tables were placed at right angles to each other and had erected upon them backgrounds of the same dimensions as the tops of the tables. Both tables and backgrounds were covered with gray cloth. The standard form was placed upon one table against the middle of the background and the observer indicated upon the other background with a

pointer one meter long, a distance above the table which he judged to be equal to the height of the standard form.

The apparatus for Case 11 was a series of twenty-three wooden cylinders painted black, each having a diameter of 114 mm. The lengths were 36, 40, 45, 52, 57, 64, 71, 80, 90, 101, 114, 128, 143, 163, 182, 223, 228, 256, 288, 324, 365, 407, 456 mm. These cylinders were placed upon a table in the order of size and about 50 mm. apart, and the observers simply pointed to the cylinders they selected.

In Case 12 rectangular black cardboard plates, 114 mm. wide and corresponding in height to the lengths of the wooden cylinders, were used. They were fastened invisibly to a long strip of gray cloth, 90 cm. wide, which was tacked upon the wall.

In addition to the measurements upon the forms 114 mm. in length, measurements were also made upon the forms 104 mm. and 124 mm. in length for Cases 6 to 10 inclusive. The purpose of the tests upon these forms was to secure a check upon the measurements on the regular 114 mm. standards and also to eliminate any suggestion which might rest upon the employment of forms that have equal dimensions.

All the forms were on a level with the observer's eyes, unless otherwise noted, and one and one-half meters away. Four determinations were made in each of the first ten cases and only one determination in each of the last two cases.

The averages of all the estimates of the observers, the average individual mean variations, and the percentage of illusion for the various cases are presented in Table IV. In Table V the individual estimates are given for the 114 mm. square and 114 mm. cylinder. This table is introduced to show the extent of agreement of the different observers with one another.

TABLE IV.

	Standard 104 mm.			Standard 114 mm.			Standard 124 mm.				
Case	*E*	*d*	*% Il*	*E*	*d*	*% Il*	*E*	*d*	*% Il*	*E*	*% Il*
1				100	3	—12					
2				107	2	— 6					
3				109	2	— 3.5					
4				97	3	—15					
5				123	3	+ 8					
6	108	3	+ 3.8	116	3	+ 1.8	126	3	+ 1.6		
7	121	3	+16	130	2	+14	135	1	+ 9		
8	116	3	+12	127	3	+11	136	4	+ 9		
9	114	3	+10	125	3	+10	134	1	+ 8		
10	106	2	+ 2	115	2	+ 0.9	125	2	+ 0.8		
	Standard 57 mm.			Standard 114 mm.			Standard 228 mm.			Standard 456 mm.	
11	54		— 5	93		—18	204		—11	366	—20
12	55		— 3	112		— 1.8	211		— 7	383	—16

The notation is the same as in Table I.

TABLE V.

Case	*1*		*2*		*3*		*4*		*5*		*6*		*7*		*8*		*9*		*10*		*11*	*12*
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*E*
2	99	3	107	3	114	0	102	3	115	4	113	0	122	0	129	3	105	2	103	3	91	102
4	103	2	105	2	108	1	96	3	119	0	120	3	132	5	128	4	129	5	107	4	91	114
1	103	4	107	0	109	0	94	0	131	1	125	5	134	2	138	7	134	2	113	3	102	114
6	105	3	107	3	105	2	103	4	120	6	112	5	130	2	117	3	125	3	120	3	102	114
8	101	2	110	2	110	3	96	4	125	2	117	2	134	0	126	4	132	2	124	1	91	114
10	94	5	109	0	114	1	99	3	117	1	108	3	125	2	117	3	123	3	111	2	91	114
5	97	3	107	0	108	1	92	3	121	3	115	4	124	5	123	2	127	3	112	1	81	102
3	94	0	102	0	110	2	89	3	123	1	127	5	130	2	138	2	118	1	115	1	91	114
12	102	3	106	3	98	4	91	2	134	5	112	4	135	3	122	3	133	2	122	3	81	114
14	102	5	109	2	109	3	107	3	122	3	109	3	126	2	121	3	127	3	123	2	102	114
16	97	2	111	2	110	4	94	1	125	1	121	3	132	1	133	3	125	2	116	2	102	114
Ave	100	3	107	2	109	2	97	3	123	3	116	3	130	2	127	3	125	3	115	2	93	112
Il	—14		— 7		— 5		—17		+ 9		+ 2		+16		+13		+11		+ 1		—21	— 2
%Il	—12		— 6		— 3.5		—15		+ 8		+ 1.8		+14		+11		+10		+0.9		—18	— 1.8
%D	2.6		1.8		2.6		4		4		4.4		3		5.2		4.6		4.6		5	3

The notation is the same as in Table I. In the first column the even numbers refer to the men and the odd numbers to the women. The individual records are presented in the order in which they were made.

The plate was studied in Cases 2, 6, 10, and 12. From Case 2 there is obtained a measurement of the illusion of the vertical. This is shown in the selection of a plate whose height is 6% less than the width; that is, a plate 107 mm. tall and 114 mm. wide was judged to be square. The result for the same experiment upon the school children was —9.7% (Series I, Case 2). In Case 6, where the height of the plate was marked off with a pointer, and in Case 10, where the line equal to the height of the plate was selected, the illusion of the vertical appears in both the standard and compared forms and is therefore fairly eliminated. In Case 6 there is a residual of +1.8% and in Case 10 of +0.9%. These residuals are due to other illusions which will be discussed in a subsequent series. For the 114 mm. standard in Case 12, the illusion of the vertical is smaller than usual. This is probably due to the use of a long series of parallelograms, and also to the fact that the observers reacted against the illusion of the vertical.

The tests upon standards 57, 228, and 456 of Case 12 are interesting, since they represent the balancing of two illusions. With these forms there is in addition to the illusion of the vertical, the illusion of length, according to which the longer dimension of a parallelogram is overestimated.[1]

When the form is longer in the horizontal dimension, as with standard 57, the illusion of the vertical and the illusion of length conflict, the former tending to cause the selection of a plate whose height is too small and the latter one whose height is too great. The result is —3%; that is, the illusion of the vertical is stronger than the illusion of length by 3% in this instance. When the height of the form is two and four times the width (Standards 228 and 456 respectively), the illusion of the vertical coöperates with the illusion of length, the effect of each being to cause

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[1] See SEASHORE and WILLIAMS, *An Illusion of Length*, Psychol. Rev., 1900, VII., 592. Reprinted in this volume, p. 29.

the selection of a plate which is too short vertically. The forms actually selected were too short by 7% and 16% respectively. The sum of the two illusions apparently increases with the length of the form. In these experiments there is no way of determining whether this variation is in one or both of the illusions.

The illusion for the vertical cylinder is 12% for Case 1; that is, the length and diameter of a cylinder appear equal when the length is 12% less than the diameter. The child observers selected a cylinder which was 18% too short. (Series I, Case 1). These amounts represent the coöperation of the illusion of the vertical and the illusion of cylinder length. The former illusion is 6% for the plate and it may be assumed that it is the same for the length of the cylinder. The illusion of cylinder length then amounts to 6%.

It was stated in the discussion of Series I that the illusion of the vertical and the illusion of cylinder length conflict when the cylinder is laid upon its side as in Case 3. The result for this case is —3.5%; that is, the illusion of cylinder length outweighs by 3.5% the illusion of the vertical. If the illusion of the vertical is 6%, then the illusion of cylinder length is 6% plus 3.5% or 9.5%. In order to determine the variation of the illusion of cylinder length with the age of the observers, these two statements of the illusion for adults, 6% and 9.5%, should be compared with the corresponding statements of the illusion for children, 8.3% and 7.9%, as shown in Series I.

In both Case 4 and Case 1, the length of the vertical cylinder was compared with the diameter but the observers occupied different positions with reference to the cylinder. In Case 4 they looked down upon it at an angle of thirty degrees. The illusion is 3% greater for Case 4 than for Case 1. This is probably due to an underestimation of the diameter on account of the Müller-Lyer effect. The illusions for the cylinder are of greater force in Case 11,

standard 114, than in Case 4. With the other three standard cylinders of Case 11, the illusion of length, the illusion of the vertical, and the illusion of cylinder length enter. In general these illusions, taken together, seem to increase with the increase in the length of the cylinder.

In the discussion of the cylinder up to this point, only the ratio of the length and diameter has been considered. In Case 5 the diameter of the cylinder was compared with plates with the result that a plate 8% wider than the horizontal diameter of the cylinder was selected as equal to it. This means that there is an overestimation in the diameter (width) of the cylinder which does not appear in the width of the plate. For the present this illusion will be called the illusion of cylinder diameter but it will be explained and renamed in the following series of experiments. Now, it is evident that if the diameter of the cylinder is overestimated, the results in those cases in which the length and diameter of the cylinder are compared do not represent the full force of the illusions involved. For instance, the illusion of length in the cylinder in Case 1 is of sufficient strength to exceed by 6% the illusion of cylinder diameter, which is 8%. The illusion of cylinder diameter would cause too long a cylinder to be selected but, eliminating the illusion of the vertical, a cylinder 6% too short is selected. The illusion in the length of the cylinder, exclusive of the illusion of the vertical, then, really amounts to 14% in Case 1. It will be observed that the illusion of cylinder length is only a part of the illusion in the length of the cylinder. A further analysis of this fact will be given in the next series. In Case 7 the height of the plate was compared with the length of the cylinder. By this method the illusion in the length of the cylinder is found to be 14%. The results obtained by these two radically different methods are the same. Two more independent statements of the illusion in the length of the cylinder are obtained from Cases 8 and 9. These are 11% and 10%

respectively for the two cases. These figures would be larger if the methods used did not involve so many complications. In Case 8 the presence of the base line, and in Case 9 the presence, in the wire, of a new illusion to be discussed later, reduces the force of the illusions.

The illusion of cylinder length may be estimated to be about 8%, and, taking all the above data into consideration, the total illusion in the length of the cylinder, exclusive of the illusion of the vertical, is 14%. There is therefore a residual of about 6% in the length of the cylinder.

All the essential features found in the experiments with the 114 standard (the form in which the two dimensions are equal) are confirmed by the results for the experiments upon the two forms in which the dimensions were not equal (Standards 104 and 124 of Cases 6 to 10 inclusive). The difference in the proportions of the two dimensions of the form has however a marked effect, as may be seen in Table IV.

The general conclusions drawn from the results of this series of experiments, under the conditions described, may be summarized as follows:

The illusion of the vertical in the square is about 6% for this type of observers.

The illusion of the vertical in the square is not so strong for the adults as for the children. (Series II, Case 2; Series I, Case 2).

The illusion of cylinder length is apparently as strong for adults as for children.

A new illusion appears in the overestimation of the height of the plate. (Cases 6 and 10).

The total effect of the illusion of the vertical and the illusion of length increases with the length of the parallelogram. (Case 12).

The total effect of the illusion of the vertical, the illusion of cylinder length, and the illusion of length increases with the length of the cylinder. (Case 11).

The illusion of cylinder length, as determined in two different ways, is found to be stronger than the illusion of the vertical.

The diameter of the cylinder is overestimated by about 8%. (Case 5).

There is a new illusion in the length of the cylinder which appears in an overestimation of about 6%.

The difference in the proportions of the two dimensions of an object influences the perception of the magnitude of one dimension. (Standards 104 and 124 of Cases 6 to 10 inclusive).

Series III

The experiments in Series III were made in the fall of 1900. The purpose of this series was, first, to establish further the illusions in the cylinder and square, which were brought out in the previous series, by studying them under more varied conditions and with different observers; and, second, to obtain data upon two new forms, the cube and drawn cylinder (Fig. 1, Forms 2 and 15).

The standard cube and cylinder were placed upon a small support in the center of a background of light manilla cardboard 1 meter square. The plate was cut from black paper and pasted at the center of a sheet of cardboard and the drawn cylinder was outlined at the center of another sheet. These backgrounds were of the same color and size as the one upon which the standard forms were placed.

The forms used for comparison with the standard forms were lines and plates (Fig. 1, Forms 16 and 1). A series of twelve black lines, 1 mm. in width and varying in length by 5 mm. steps, from 94 mm. to 149 mm., were drawn upon each of four manilla cardboard sheets, 1 meter square. The lines were drawn in no definite order with regard to length, but they were 50 mm. apart and parallel.

The arrangement of the lines was different in the four series.

There were four series of twelve rectangular plates each. These plates were all 114 mm. wide, but the lengths varied by 5 mm. increments from 94 mm. to 149 mm. in each series. They were fastened upon a background 2 meters square, covered with the light manilla cardboard. The four series were placed in as many rows; in the first the smaller plates were in the center and the larger ones at the ends and they varied in the vertical dimension; in the second row the smaller plates were also in the central part but the horizontal dimension varied; in the third row the larger plates occupied the central position and their vertical dimensions varied; and in the fourth row the larger plates were in the center and their horizontal dimensions varied. The plates were about 45 mm. apart in the row and the rows about 30 cm. apart. The height of this large background was adjustable and it was so arranged that the particular row from which the plate was to be selected was on a level with the eyes of the observer. By having several series of plates and lines the tendency of the observers to select from memory was obviated.

The background for the standard forms and that for the compared forms were placed at right angles to each other. The observer was seated upon a revolving office stool at a distance of 1 meter from the objects observed and was required to turn upon the stool so that the whole body moved through an angle of ninety degrees. The standard form was first placed before the observer, then a series of lines or plates with which the standard was to be compared was shown. The observer indicated with a long pointer the compared form which he selected.

Measurements were also made upon the cube, the cylinder, and the plate when these forms were so placed that the observer looked down upon them at an angle of thirty degrees. In the case of the cube and cylinder this afforded a view of the top. The forms with which the standard

TABLE VI.

Case	*Dimension of Standard Form Measured*	*Compared Form*	*Direction*	*No. in Fig. 1*
1	Height of cube	Lines	Vertical	2
2	Width of cube	Lines	Horizontal	2
3	Length of cylinder	Lines	Vertical	3
4	Diameter of cylinder	Lines	Horizontal	3
5	Height of cylinder	Plates	Vertical	3
6	Diameter of cylinder	Plates	Horizontal	3
7	Height of cube	Plates	Vertical	2
8	Width of cube	Plates	Horizontal	2
9	Length of cylinder	Plates	Horizontal	3
10	Diameter of cylinder	Plates	Vertical	3
11	Length of cylinder	Lines	Horizontal	3
12	Diameter of cylinder	Lines	Vertical	3
13	Length of drawn cylinder	Lines	Vertical	15
14	Diameter of drawn cylinder	Lines	Horizontal	15
15	Length of drawn cylinder	Plates	Vertical	15
16	Diameter of drawn cylinder	Plates	Horizontal	15
17	Length of drawn cylinder	Lines	Horizontal	15
18	Diameter of drawn cylinder	Lines	Vertical	15
19	Length of drawn cylinder	Plates	Horizontal	15
20	Diameter of drawn cylinder	Plates	Vertical	15
21	Length of cylinder*	Lines	Vertical	3
22	Length of cylinder*	Plates	Vertical	3
23	Height of cube*	Lines	Vertical	2
24	Height of cube*	Plates	Vertical	2
25	Height of plate	Plates	Vertical	1
26	Height of plate	Lines	Vertical	1
27	Height of plate*	Plates	Vertical	1
28	Height of plate*	Lines	Vertical	1
29	Diameter of cylinder*	Lines	Horizontal	3
30	Diameter of cylinder*	Plates	Horizontal	3

* The observer looked down upon these forms at an angle of 30 degrees.

forms were compared when in this position were on a level with the observer's eyes.

Except as just noted, only one side of the cube was visible and neither end of the cylinder. Eight trials were made on each case; the experiment lasted one hour a day for each observer and four days were required for some of the observers. The order of sequence of the cases was deter-

TABLE VII.

Case	*A*	*Vl*	*C-L*	*M-L*	*% Il*
1*	+	+	0	0	+12
2*	+	+	0	0	+14
3*	+	+	+	0	+15.7
4*	+	+	0	0	+12
5†	0	+	+	0	+ 9
6	0	+	0	0	+ 6.1
7†	0	+	0	0	+ 3.7
8	0	+	0	0	+ 5.2
9	0	+	+	0	+12.3
10†	0	+	0	0	0
11*	+	+	+	0	+18.5
12*	+	+	0	0	+10.4
13*	+	+	+	0	+17.5
14*	+	+	0	0	+12.1
15†	0	+	+	0	+11.7
16	0	+	0	0	+ 5.2
17*	+	+	+	0	+20
18*	+	+	0	0	+ 9.5
19	0	+	+	0	+16.7
20†	0	+	0	0	+ 1.1
21*	+	+	+	0	+14.8
22†	0	+	+	0	+ 9
23*	+	+	0	0	+11.3
24†	0	+	0	0	+ 3
25†	0	0	0	0	0
26*	+	0	0	0	+ 7
27†	0	0	0	0	0
28*	+	0	0	0	+ 6
29*	+	+	0	—	+13
30	0	+	0	—	+ 3.5

A, area illusion.

Vl, volume illusion.

C-L, cylinder-length illusion.

M-L, Müller-Lyer illusion.

% Il, percentage of illusion.

*In these cases an allowance of 1% has been made to eliminate the error due to the series of lines.

†An allowance of 5% has been made for the plates for a similar reason.

TABLE VIII.

	Obs 2 (*4 trials*)		*Obs 4* (*8 tr'ls*)		*Obs 6* (*8 tr'ls*)		*Obs 8* (*8 tr'ls*)		*Obs 10* (*8 tr'ls*)		*Obs 12* (*6 tr'ls*)		*Obs 14* (*8 tr'ls*)		*Obs 16* (*2 tr'ls*)	
Case	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	130	2	141	8	133	4	118	4	117	4	134	3	119	5	129	5
2	133	3	130	8	136	3	125	4	115	2	137	6	134	6	129	5
3	132	3	144	3	137	6	120	5	123	6	140	2	128	5	132	3
4	133	2	128	8	133	4	122	4	118	3	132	5	135	7	122	3
5	124	0	138	5	124	1	122	3	123	3	134	5	133	5	132	3
6	118	2	127	5	119	5	115	2	121	4	119	3	123	4	117	8
7	119	0	128	6	116	3	117	2	119	2	126	4	130	3	129	0
8	115	2	127	6	123	2	117	3	118	3	122	2	127	4	117	8
9	119	0	129	6	131	2	123	2	129	4	131	5	137	3	124	0
10	118	2	130	4	113	1	117	3	120	3	126	2	128	4	117	3
11	135	2	137	3	140	4	128	3	124	5	142	6	138	8	129	10
12	128	2	137	8	130	4	119	5	118	5	133	4	121	5	124	0
13	133	2	135	9	141	5	131	3	122	3	137	3	131	6	142	3
14	137	4	136	7	134	5	128	4	117	3	132	4	129	6	124	0
15	130	4	134	5	130	5	131	2	130	2	138	4	133	8	139	0
16	120	2	127	4	118	3	118	2	122	2	116	2	127	6	122	3
17	138	4	133	7	138	7	130	3	126	4	140	5	145	3	144	0
18	130	2	137	6	131	2	123	2	113	2	127	5	118	3	129	5
19	124	0	130	7	133	2	126	2	135	3	129	3	136	3	144	0
20	120	2	128	5	115	3	122	3	119	3	120	2	121	3	119	0
21	132	3	139	6	138	3	116	4	120	4	142	4	126	6	134	0
22	124	0	135	6	129	3	117	2	126	2	137	2	132	4	139	0
23	129	0	142	6	126	4	113	3	117	4	138	3	122	6	124	0
24	119	3	132	4	117	3	114	3	121	2	133	4	128	4	124	5
25	117	3	117	6	113	1	120	1	117	2	123	4	123	2	127	3
26	129	0	125	6	125	3	118	3	115	3	131	4	119	4	124	0
27	117	5	114	6	111	2	115	2	115	4	127	5	124	3	127	3
28	124	3	120	8	122	4	115	7	112	4	134	0	117	3	122	3
29	129	5	132	5	137	2	118	3	115	3	137	8	129	2	119	0
30	117	3	121	4	113	2	110	2	118	3	124	5	123	4	112	3

mined partly to avoid disturbing influences in the serial order and partly by the convenience in manipulating the apparatus. The double fatigue order was observed. Vertical distances were compared with vertical and horizontal with horizontal.

The various cases introduced in Series III are described in Table VI. The dimension of the form measured, the form with which it was compared, and the direction of the

TABLE VIII. Continued.

Obs 18 (2 tr'ls)		Obs 20 (2 tr'ls)		Obs 22 (2 tr'ls)		Obs 1 (1 tr'l)	Ave					
E	*d*	*E*	*d*	*E*	*d*	*E*	*E*	*d*	*Il*	*% Il*	*% D*	*Case*
122	3	134	5	132	3	134	129	4	+15	13.1	5.2	1
129	5	129	5	139	5	139	131	5	+17	15.0	4.4	2
134	0	134	5	137	8	134	133	4	+19	16.7	4.4	3
127	3	124	0	139	10	134	129	4	+15	13.1	4.4	4
122	3	124	5	139	5	144	130	3	+16	14.0	6.1	5
117	3	104	0	134	5	134	121	4	+ 7	6.1	5.2	6
122	3	124	5	124	5	139	124	3	+10	8.7	4.4	7
119	0	114	0	124	0	129	120	3	+ 6	5.2	3.5	8
119	5	124	0	129	5	139	128	3	+14	12.3	4.4	9
114	5	114	0	119	10	129	120	3	+ 6	5.2	4.4	10
127	3	139	5	144	5	144	136	5	+22	19.3	5.2	11
122	3	129	5	132	3	129	127	4	+13	11.4	4.4	12
137	8	139	5	139	10	129	135	5	+21	18.4	3.5	13
127	3	124	0	137	3	124	129	4	+15	13.1	4.4	14
129	0	129	0	139	0	139	133	3	+19	16.7	3.5	15
117	3	104	0	124	0	119	120	2	+ 6	5.2	4.4	16
137	3	139	5	149	0	139	138	4	+24	21.0	4.4	17
124	10	119	0	137	8	119	126	4	+12	10.5	5.2	18
134	0	132	3	134	0	139	133	2	+19	16.7	3.5	19
124	5	112	3	129	10	129	121	4	+ 7	6.1	3.5	20
127	8	137	3	129	0	139	132	4	+18	15.8	6.1	21
124	5	124	5	132	3	144	130	3	+16	14.0	5.2	22
124	0	137	3	132	3	129	128	3	+14	12.3	6.1	23
117	3	119	5	119	0	129	123	3	+ 9	8.0	5.2	24
114	0	117	3	119	0	129	120	2	+ 6	5.2	3.5	25
119	5	132	3	129	5	114	123	3	+ 9	8.0	4.4	26
124	5	114	0	119	0	124	119	3	+ 5	4.4	4.4	27
124	0	132	3	132	3	114	122	3	+ 8	7.0	5.2	28
134	0	124	0	132	3	129	130	3	+16	14.0	5.2	29
127	3	112	3	124	0	119	118	3	+ 4	3.5	4.4	30

Notation same as in Table I.

linear dimensions compared are given. The averages of the individual observers are found in Table VIII. This table reads across from left to right for each case and from the top down for each observer. An analysis of the illusions involved in the forms is found in Table VII, which is placed opposite Table VI for convenience in comparison. In each case the appropriate sign is given for each illusion

that enters; a plus sign signifies that the compared dimension selected should be larger than the standard dimension, and a minus sign signifies that the compared dimension selected should be smaller than the standard dimension. The same illusion may have a plus or a minus sign, depending upon the method of comparison. A plus sign in the Il. column indicates that the compared dimension was selected larger than the standard dimension.

The rather large mean variations in these tables are due to the method employed, a discussion of which will be given in connection with a later series. The very large variation given under % D is accounted for by the various degrees of knowledge and preparation represented by the observers, and by the fact that the force of the same illusion often varies greatly with different individuals.

The conclusions drawn from Series III are based upon the results obtained from twelve representative observers. They will be mentioned individually, as they represent different degrees of training and knowledge of the subject of visual illusions. Observer 2 had an extensive knowledge of the subject and was an observer of long experience; he made a strong effort not to let himself be influenced by his theories. Observers 4 to 14, even numbers inclusive, represent a class of second year students in psychology. They had practically equal knowledge of the illusions and were all about equally experienced as observers. Observer 16 was an instructor in philosophy. Observer 18 was an instructor in mathematics trained in estimating distances by the eye but he had no definite knowledge concerning any illusions except the illusion of the vertical. Observer 20, an instructor in the department of civil engineering, had never studied illusions but was a skilled draughtsman. Observer 22 was a first year student in civil engineering; he knew nothing whatever about illusions and his judgments may be regarded as more naive than the judgments of any of the other observers. Observer 1 may be classed with

observers 4 to 14 mentioned above. To none except Observer 2 was the information given that the test was upon visual illusions and, since the attention was not called to this fact, they as a rule made no conscious allowance for the illusions. With regard to the illusion of the vertical, all but Observer 20 belong to the first type of observers; but with reference to the other illusions, the Müller-Lyer illusion excepted, all but Observer 2 belong to the second type.

In the interpretation of the records, allowance must be made for an error which occurs as the result of placing the lines and plates in a series. When a line is grouped with several other lines, as in the charts described on p. 60, there is a tendency to see it shorter than it really is, regardless of direction. The amount of this underestimation has been carefully measured and will come up for discussion in the proper place under Series VII. It is necessary at this point only to note that the error in the series of lines amounts to 1%. This must be deducted from the results in all those cases in which the standard forms were compared with lines. The error in the series of plates is similar in nature but is of greater force. Its measurement is obtained from Case 25 of the present series. (See Table VIII). Here a plate is selected from the series of plates which appears equal to the height of the standard plate. Now if each form stood alone in such a comparison, there should be no illusion, but as the standard plate stands alone and the other plate is one of a series of plates, there enters an illusion of about 5%. It will be necessary to deduct this amount from all those cases in which the standard forms were compared with the height of the plates. This 5% may not be the same for all the cases but, for the present purpose, it will be necessary to assume that the error due to the series of plates is constant. These corrections have been made in Table VII by subtracting 1% for lines and 5% for plates from the original results as given in Table

TABLE IX.

Dimension of Standard Form Measured	Compared with Lines Vertical %Il	Case	Horizontal %Il	C'e	Compared with Plates Vertical %Il	C'e	Horizon'l %Il	C'e
Height of plate	7.0	26*			0	25†		
H'ght of plate, 30° down	6.0	28*			0	27†		
Cube	12.0	1*	14.0	2*	3.7	7†	5.2	8
Cube, 30° down	11.3	23*			3.0	24†		
Length of cylinder	15.7	3*	18.3	11*	9.0	5†	12.3	9
L'gth of cyl'r, 30° down	14.8	21*			9.0	22†		
Length of drawn cyl'r	17.4	13*	20.0	17*	11.7	15†	16.7	19
Diameter of cylinder	10.4	12*	12.0	4*	0	10†	6.1	6
Diam. of cyl'r, 30° down			13.0	29*			3.5	30
Diam. of drawn cylinder	9.5	18*	12.0	14*	1.1	20†	5.2	16

*In these cases an allowance of 1% was made, to eliminate the error due to the series of lines.

†An allowance of 5% was made for the plates, for a similar reason.

VIII. From the data available there is no definite means of ascertaining whether or not the width of the plate is affected when the plate is one of a series, therefore no allowance will be made in those cases where the width of the plate was compared with the standard form. In Table IX, which is a summary of Table VIII, the appropriate deduction of 1% for the lines and 5% for the plates has also been made, and it is upon this table that the discussion of this series is based.

The results of the measurements upon the standard plate, cube, cylinder, and drawn cylinder, when these forms are on a level with the observer's eyes, will be discussed first; then the corresponding measurements upon the forms when thirty degrees below the level of the eyes will be considered.

The plate will be examined first. In case 26 its height was compared with a vertical line. The result is +7%; that is, a line must be 7% higher than the plate if the two forms are to appear equal in height. In other words, if the line and plate are actually equal in height, the plate ap-

pears to the observers to be 7% higher than the line. The question—Why is this? at once arises. The effect of the illusion of the vertical is practically eliminated, for it appears in both forms and so does not change the relative sizes of the line and plate to any great extent. The forms differ from each other only in one respect; the plate has area while the line is practically without area. The difference between these forms must in some way be due to this fact. The illusion of 7% in the plate is an illusion due to area; it is on account of the width of the plate that its height appears greater than the height of the line. Hence we name this the area illusion. It is more thoroughly established in the following series and its application to the series now under discussion is therefore justified.

The cube (Fig. 1, Form 2) was introduced for the first time in Series III. In Case 1 its height was compared with a vertical line and in Case 2 its width with a horizontal line. There resulted an overestimation of 12% and 14% respectively for the two cases. Part of this is evidently due to the area illusion brought out in connection with the plate. When the cube is compared with the plate, as in Cases 7 and 8, the area illusion appears in both forms and is therefore to be disregarded. Yet in Case 7, where the height of the cube is compared with the height of the plate, there is required a plate 3.7% higher than the cube to appear equal to it; and when the width of the cube is compared with the width of the plate (Case 8), a plate 5% wider is required. The cube was so placed that only one face of it was visible to the observer; it thus had the same appearance to him as the plate, although he was fully aware that it was a cube. Now, the cube differs from the plate only in that it has the three dimensions of space; therefore any difference between the results for the two forms must be regarded as due to this fact. In other words, if the cube is judged to be larger than the plate it is because it has volume. The percentages obtained in Cases 7 and 8 and

the residuals in Cases 1 and 2 after the area illusion has been eliminated are statements of an illusion due to volume; hence we name it the volume illusion.

A line has relatively neither volume nor area. A vertical line appears equal to the height of the cube only when it is 12% taller than the cube (Case 1) and a horizontal line appears equal to the width of the cube only when it is 14% longer than the width of the cube (Case 2). These are statements of the area and volume illusions combined. The plate, which has area, appears equal in height to the height of the cube when it is 3.7% taller than the cube (Case 7) and it appears equal to the cube in width only when it is 5.2% wider than the cube (Case 8). These percentages are statements of the volume illusion in the cube. The area illusion for the height of the cube is 12% minus 3.7% or 8.3% (Case 1 minus Case 7), and for the width of the cube it is 14% minus 5.2% or 8.8% (Case 2 minus Case 8); that is, the area illusion is the difference between the results for the comparisons of the cube with lines and with plates.

Turning now to the cylinder (Fig. 1, Form 3), it is seen that when a vertical line is selected which appears equal to the length of the vertical cylinder, there is an illusion of 15.7% (Case 3); and when a horizontal line is selected which appears equal to the length of the horizontal cylinder, there is an illusion of 18.3%. Further, when the height of the plate is compared with the length of the vertical cylinder (Case 5), and the width of the plate with the length of the horizontal cylinder (Case 9), the illusions are reduced to 9% and 12.3% respectively for the two cases. That is, when the forms compared are vertical, a line must be 15.7% higher than the length of the cylinder in order to appear equal to it, whereas a plate need be but 9% higher; and when the forms compared are horizontal, a line must be 18.3% longer than the length of the cylinder in order to appear equal to it, whereas a plate need be but 12.3% wider.

This difference between the line and plate is about 6% for the vertical position and the same for the horizontal position of the forms. (Compare Case 3 with 5 and Case 11 with 9). This 6% is the area illusion in the length of the cylinder, for the motive of the area illusion found in the plate is present in the cylinder. The motive for the volume illusion found in the cube is also present in the cylinder, but in these measurements its force for the length of the cylinder is not determined directly, since it appears in combination with the illusion of cylinder length, for which no determination was made in the present series. However, in Series II this illusion was found to amount to 8% and it is probably about the same in the present series. The area illusion in the length of the cylinder is 6%; and in Case 3, where the vertical line is compared with the vertical cylinder, three illusions enter, exclusive of the illusion of the vertical, namely, the illusion of cylinder length, the area illusion and the volume illusion. The combined force of these illusions is 15.7%. Subtracting 8% for the illusion of cylinder length and 6% for the area illusion, there is a residual of 1.7% which is the volume illusion in the length of the vertical cylinder. Similarly, it is 4.3% for the length of the horizontal cylinder, when the length of the cylinder is compared with a horizontal line (Case 11). When the plate is compared with the length of the cylinder, only the illusion of cylinder length and the volume illusion enter (Cases 5 and 9). If the illusion of cylinder length is 8%, the volume illusion for the length of the vertical cylinder is 1% (Case 5), and for the length of the horizontal cylinder it is 4.3% (Case 9). Of these four statements of the volume illusion for the length of the cylinder, the two upon the length of the horizontal cylinder agree with each other and those upon the length of the vertical cylinder agree with each other, but the two determinations of the illusion for the vertical cylinder are smaller than those for the horizontal cylinder. This is due

to the fact that not all of the illusion of the vertical is excluded in the tests upon the vertical cylinder, a fact which will be fully explained in the next series; and perhaps also to unconscious correction for the illusion of the vertical.

The illusions in the diameter of the cylinder may be considered next. In Case 12 the vertical line was compared with the diameter of the horizontal cylinder and in Case 4 the horizontal line with the diameter of the vertical cylinder; the results are 10.4% and 12% respectively. This agrees very well with the tests of a similar nature upon the cube (See Cases 1 and 2), and as the same motives for illusion are present in both forms, the explanation given for one is adequate for the other. These percentages, then, are statements of the combined area and volume illusions for the diameter of the cylinder. In Case 10 the height of the plate was compared with the diameter of the horizontal cylinder. The result should give a statement of the volume illusion, but as recorded in Table IX it is zero. It will be remembered that this is one of the cases in which an allowance was made for the series of plates, and in this instance it would seem that this 5% was too great an allowance. Again, throughout Table IX there is brought out a general tendency for the illusion to be somewhat stronger when the dimensions compared are horizontal than when they are vertical. The reason for this will be given in the next series. It may be, then, for these two reasons, namely—too great an allowance for the error in the series of plates and the smaller illusion when the compared forms are vertical, that the volume illusion for the diameter of the cylinder is not brought out in Case 10. That it is present is indicated in two ways: first, by the fact that the 10.4% of Case 12 is too great for the area illusion alone, consequently something of the volume illusion must be involved; and second, when the width of the plate is compared with the diameter of the

vertical cylinder (Case 6) the volume illusion for the diameter of the cylinder is 6%. If the volume illusion is 6% for the diameter of the cylinder, then the area illusion for the diameter of the cylinder in Cases 12 and 4, which represent the combined effect of the two illusions, is 4.4% and 6% respectively for the two cases.

In connection with Series II it was found that the diameter of the cylinder was overestimated by 8%, and this illusion was termed the illusion of cylinder diameter. It is evident that the volume illusion of the present series and the illusion of cylinder diameter of Series II are precisely the same illusion; in other words, the overestimation of cylinder diameter is due to the volume illusion.

If the measurements upon the length of the drawn cylinder are compared with the corresponding measurements upon the length of the metal cylinder, it will be observed that the same general laws hold for the one as for the other. (Compare Cases 13 and 3, 17 and 11, 15 and 5, 19 and 9). The diameter of the drawn cylinder is also judged to be practically the same as the diameter of the metal cylinder. (Compare Cases 18 and 12, 14 and 4, 20 and 10, 16 and 6). These facts show that the same illusions are present in both the real and the drawn cylinders. In other words, the illusions persist as long as the form is perceived as a cylinder, although in reality it may not be a cylinder. The significance of this will be brought out in the next series.

The tests considered in the foregoing discussion were made with the forms on a level with the eyes of the observers. Corresponding measurements were made upon some of the forms when these were placed thirty degrees below the line of vision. For the height of the plate the angle seems to make no difference in the results. (Compare Cases 26 and 28, and 25 and 27). The same may be said of the cube (Cases 1 and 23, and 7 and 24), and also for the length of the cylinder (Cases 3 and 21, and 5 and 22). When measurements are made upon the diameter of the

cylinder in this position, a Müller-Lyer effect should appear causing an underestimation of the diameter. For some reason this is not brought out in Case 29, but it appears in Case 30. (Compare Cases 4 and 29, and 6 and 30). On the whole the measurements upon the forms in this second position support those made upon the forms in the first position.

The illusions involved in the standard forms in this series are shown in the analysis of them given in Table VII. The following general conclusions may be drawn:

The height of the plate is overestimated; this is the area illusion.

The area illusion also appears in the height and width of the cube.

It also appears in the length and diameter of the cylinder and in the length and diameter of the drawn cylinder.

In addition to the area illusion, the height and width of the cube are overestimated. This is the volume illusion.

The volume illusion appears in the length and diameter of the cylinder and it is also present in the length and diameter of the drawn cylinder.

The Müller-Lyer illusion sometimes affects the perception of the diameter of the cylinder.

Series IV

Series IV followed closely upon Series III, the records being made in January, 1901. The aim of the series was three-fold: first, to obtain measurements upon the illusion of the vertical for some typical geometrical forms; second, to test an hypothesis which would explain the volume illusion; and third, to study the illusions in some new forms. In addition to the plate, cube, cylinder, and drawn cylinder of Series III, the standard forms consisted of a sphere, a disk, a circle, a drawn square, and an ellipse. (See Fig. 1, Forms 4, 8, 12, 13, and 14, respectively). Each

form was compared with lines; the width or horizontal dimension of each was compared with a horizontal line, and the height or vertical dimension with a vertical line and a horizontal line.

The same method as for the lines in the previous series was employed. Six different series of lines were used with which to compare the standards, thus providing against the observer's selecting from memory. The backgrounds upon which the lines were drawn were also frequently turned so that the position of the top and bottom was reversed; and a different series of lines was used for each determination. The relative position of the standard and compared forms was the same as in the previous series. Two determinations were made for each of thirty-two cases with each of twenty observers.

The list of cases introduced into the series and the positions of the standard and compared forms is found in Table X.

In Table XI the signs for the several illusions which influence the records are given, and also the percent of illusion. In this table allowance was made for the 1% error due to the series of lines as explained in the previous series. The interrogation point is placed where there is some doubt as to the presence of the illusion. The difference in the signs for the illusion of the vertical will be explained later. The discussion of the series is based chiefly on Table XI.

In Table XII the individual estimates of the twenty observers are given. There were two trials for each case with each observer and the average of these forty trials is given, together with the amount of the illusion in millimeters and in percentages; also the % D. The individual mean variations are not given, since they can easily be made out from the table. In this table no deduction is made for the 1% error due to the series of lines.

The twenty observers in this series were selected from

TABLE X.

Case	Dimension of Standard Form Measured	D. St. F.	D. Comp. Line	No. in Fig. 1
1	Height of plate	Vertical	Vertical	1
2	Width of plate	Horizontal	Horizontal	1
3	Height of plate	Vertical	Horizontal	1
4	Height of cube	Vertical	Vertical	2
5	Width of cube	Horizontal	Horizontal	2
6	Height of cube	Vertical	Horizontal	2
7	Height of set-in cube	Vertical	Horizontal	2
8	Width of set-in cube	Horizontal	Horizontal	2
9	Height of set-in cube	Vertical	Vertical	2
10	Diameter of sphere	Horizontal	Horizontal	4
11	Diameter of sphere	Vertical	Horizontal	4
12	Diameter of sphere	Vertical	Vertical	4
13	Diameter of disk	Vertical	Vertical	8
14	Diameter of disk	Horizontal	Horizontal	8
15	Diameter of disk	Vertical	Horizontal	8
16	Long axis of ellipse	Vertical	Vertical	14
17	Long axis of ellipse	Vertical	Horizontal	14
18	Long axis of ellipse	Horizontal	Horizontal	14
19	Height of drawn square	Vertical	Vertical	13
20	Width of drawn square	Horizontal	Horizontal	13
21	Height of drawn square	Vertical	Horizontal	13
22	Diameter of circle	Vertical	Vertical	12
23	Diameter of circle	Horizontal	Horizontal	12
24	Diameter of circle	Vertical	Horizontal	12
25	Length of cylinder	Horizontal	Horizontal	3
26	Diameter of cylinder	Vertical	Horizontal	3
27	Length of cylinder	Vertical	Horizontal	3
28	Diameter of cylinder	Horizontal	Horizontal	3
29	Length of drawn cylinder	Horizontal	Horizontal	15
30	Diameter of drawn cylinder	Vertical	Horizontal	15
31	Length of drawn cylinder	Vertical	Horizontal	15
32	Diameter of drawn cylinder	Horizontal	Horizontal	15

D. St. F., the direction of the dimension measured in the standard form.

D. Comp. Line, the direction of the line with which the standard was compared.

The number of the form in Fig. 1 is given in the last column.

TABLE XI.

Case	*A*	*Vl*	*C-L*	*M-L*	*V*	*% Il*
1	+	0	0	0	—	+ 2.5 (+3.5)
2	+	0	0	0	0	+ 7.7
3	+	0	0	0	+	+13.0
4	+	+	0	0	—	+ 5.0 (+7.4)
5	+	+	0	0	0	+10.4
6	+	+	0	0	+	+14.0
7	+	+	0	0	+	+14.0
8	+	+	0	0	0	+ 9.5
9	+	+	0	0	—	+ 6.0 (+7.5)
10	+	+	0	—?	0	+ 9.5
11	+	+	0	—?	+	+10.4
12	+	+	0	—?	·	+ 2.5 (+7.5)
13	+	0	0	—?	—	— 2.0 (+3.0)
14	+	0	0	—?	0	+ 6.0
15	+	0	0	—?	+	+ 7.0
16	+	0	0	—	—	— 3.6 (—0.2)
17	+	0	0	—	+	+ 6.0
18	+	0	0	—	0	+ 3.4
19	+	0	0	0	—	+ 3.4 (+6.9)
20	+	0	0	0	0	+ 9.5
21	+	0	0	0	+	+12.0
22	+	0	0	—?	—	— 1.0 (+4.0)
23	+	0	0	—?	0	+ 6.0
24	+	0	0	—?	+	+ 7.0
25	+	+	+	0	0	+16.5
26	+	+	0	0	+	+15.0
27	+	+	+	0	+	+20.0
28	+	+	0	0	0	+12.0
29	+	+	+	0	0	+17.4
30	+	+	0	0	+	+13.0
31	+	+	+	0	+	+18.3
32	+	+	0	0	0	+11.3

A, area illusion.
Vl, volume illusion.
C-L, cylinder-length illusion.
M-L, Müller-Lyer illusion.
V, illusion of the vertical.
% Il, percent of illusion.
1% for each case was deducted from the results as given in Table XII to eliminate the error due to the series of lines.

TABLE XII.

Case	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*
Obs 1	119	129	129	119	134	129	134	134	139	129	124	114	109	114	119	114
	119	129	129	124	139	129	134	134	124	119	119	114	109	114	114	109
3	124	129	124	109	119	119	124	114	124	119	129	119	109	119	119	104
	134	134	139	124	134	144	134	124	124	139	134	124	139	139	144	129
5	114	109	129	114	124	109	114	119	109	104	109	104	104	104	109	94
	104	104	104	104	109	104	104	109	104	99	104	94	94	99	99	94
2	104	119	119	114	134	129	124	114	109	119	114	109	104	109	114	104
	104	114	114	104	114	119	114	114	104	114	119	104	104	114	114	104
7	149	129	134	124	134	144	144	134	129	124	134	129	114	124	134	124
	124	129	144	134	134	144	139	134	139	129	134	124	119	124	124	119
4	134	129	139	129	134	144	139	134	129	124	134	129	114	129	129	104
	124	129	134	124	134	144	144	129	134	129	129	124	119	119	134	109
6	124	129	134	134	119	134	139	134	129	129	134	129	129	129	134	104
	119	114	134	124	134	139	124	129	124	129	124	129	114	119	129	109
9	109	114	149	129	124	149	144	124	114	139	144	124	109	134	124	109
	109	129	139	114	139	139	139	134	119	134	144	114	109	129	134	109
8	119	119	129	114	124	124	129	124	119	I14	114	114	114	124	124	124
	119	129	134	129	129	129	124	134	134	129	129	124	119	124	129	119
10	114	129	134	119	129	124	129	124	114	129	124	109	109	124	124	109
	114	129	134	119	134	139	139	129	129	129	129	114	114	134	129	114
12	124	139	139	139	129	134	134	134	124	134	139	129	124	144	124	129
	129	144	139	139	139	144	139	144	124	144	139	129	134	139	134	129
14	124	139	129	139	129	139	144	144	139	149	149	149	129	134	144	124
	134	144	149	139	144	144	144	144	139	149	149	149	139	149	144	134
11	109	119	119	119	124	129	129	119	114	129	129	119	109	124	119	109
	129	129	144	129	134	134	139	129	129	124	129	124	119	129	119	114
16	129	114	134	119	119	134	124	119	114	129	129	114	114	129	124	109
	119	129	134	119	119	129	134	124	119	129	129	124	114	124	129	109
18	114	119	119	119	119	114	124	124	114	119	129	114	109	119	124	114
	114	124	124	114	129	119	119	124	114	124	114	114	114	109	126	104
13	119	119	119	114	114	119	129	119	114	129	124	119	104	114	109	104
	114	114	119	119	119	124	129	124	124	124	119	114	114	114	114	104
20	109	134	129	114	124	129	134	134	124	134	134	109	114	134	129	114
	119	129	129	124	129	129	134	124	119	129	134	114	114	134	124	114
15	104	114	124	104	109	124	129	114	114	109	114	94	94	114	114	109
	104	114	124	114	114	124	119	114	114	109	114	94	104	109	114	94
22	114	114	114	119	129	139	129	119	119	114	119	109	109	99	114	104
	114	114	124	114	119	124	119	119	114	114	114	109	104	109	104	114
24	109	114	119	119	129	129	134	129	119	124	119	119	109	109	109	104
	114	129	139	119	129	129	129	129	119	129	129	119	104	119	124	109
Ave	118	124	130	121	127	131	131	126	122	126	127	118	113	122	123	111
Il	4	10	16	7	13	17	17	12	8	12	13	4	—1	8	9	—3
%Il	3.5	8.7	14	6	11.4	15	15	10.5	7	10.5	11.4	3.5	—0.9	7	8	—2.6
%D	7	7	7	6	6	7	6	6	7	7	8	8	7	9	7	8

Where no sign is given in the Il and the %Il columns the plus sign is understood. Other notation same as in Table I.

TABLE XII. Continued.

17	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	*32*	*Obs*
109	109	114	119	129	109	114	119	134	139	139	129	139	129	134	114	1
114	114	119	119	119	109	129	114	129	134	134	134	134	119	129	124	
124	129	124	119	114	124	124	139	139	144	149	144	144	139	139	134	3
129	129	129	139	134	124	134	129	139	149	139	139	134	134	129	129	
99	99	109	114	114	94	99	104	114	114	119	99	129	104	119	114	5
104	104	109	104	114	94	119	104	124	104	124	109	124	99	114	99	
114	109	109	119	109	109	119	119	119	119	134	119	129	119	134	119	2
124	119	109	114	119	104	114	114	124	119	129	124	129	119	129	119	
129	124	119	124	134	104	124	124	134	149	149	149	134	129	149	129	7
129	124	134	134	139	119	119	119	149	144	149	139	134	144	144	129	
119	114	119	134	139	114	129	129	139	134	144	129	144	139	149	129	4
119	114	129	134	139	124	129	129	144	134	144	129	144	139	149	139	
119	119	139	129	139	119	129	129	139	139	144	134	134	139	149	129	6
129	114	104	129	144	119	129	129	134	139	144	134	144	129	144	129	
119	119	109	139	144	114	129	139	139	144	149	134	144	134	144	134	9
129	129	119	129	139	109	129	129	144	134	149	134	134	129	144	129	
124	119	119	124	129	119	129	124	139	129	129	124	139	134	129	124	8
129	129	119	134	129	114	124	124	129	134	134	129	129	129	134	134	
124	124	114	124	129	114	124	119	119	124	129	129	134	129	134	129	10
124	129	124	129	134	114	129	129	134	134	144	129	134	134	129	129	
134	134	124	139	134	119	129	129	139	119	144	129	144	129	139	124	12
139	139	129	139	129	129	134	134	134	139	144	134	139	119	134	134	
124	134	134	144	149	129	134	144	144	149	144	139	144	139	144	144	14
149	139	139	144	149	134	139	144	149	149	149	149	149	144	149	139	
124	114	129	129	129	114	129	124	139	144	139	144	139	144	144	144	11
129	129	119	129	129	124	134	134	149	139	144	139	134	144	144	144	
114	114	119	114	129	114	114	114	134	129	144	124	129	129	129	129	16
129	114	129	129	134	114	114	119	134	129	134	129	134	129	139	124	
124	109	109	114	124	104	109	114	129	134	129	134	129	119	119	129	18
109	119	109	129	124	109	124	119	129	124	129	129	129	124	124	124	
109	109	114	109	114	114	119	114	119	114	124	119	144	129	124	124	13
114	114	114	109	114	114	114	129	129	124	129	119	124	119	129	129	
129	124	119	134	134	114	119	124	134	139	144	134	139	139	134	144	20
129	119	119	139	134	114	129	129	144	139	139	139	144	149	144	134	
109	114	104	119	124	104	114	119	129	124	134	119	119	114	134	114	15
114	109	104	114	119	109	114	114	129	124	129	114	124	119	134	114	
114	119	114	119	124	114	104	114	129	114	139	124	134	124	134	124	22
129	124	114	114	124	114	109	109	129	124	139	109	129	129	144	134	
114	114	119	119	114	104	104	104	124	119	129	114	134	139	134	124	24
119	114	114	124	134	104	114	114	134	129	139	124	139	134	129	139	
122	119	119	126	129	114	122	123	134	132	138	129	135	130	136	128	
8	5	5	12	15	0	8	9	20	18	24	15	21	16	22	14	
7	4.4	4.4	10.5	13	0	7	8	17.5	16	21	13	18.4	14	19.3	12.3	
7	6	6	5	7	6	7	7	6	8	6	7	7	7	7	6	

TABLE XIII.

Dimensions of Standard Forms Measured in Terms of Lines	*Direction*	*Series III 12 Observers* Case	% Il	*Series IV 20 Observers* Case	% Il
Height of plate	Vertical	26	+ 7.0	1	+ 2.5
Height of cube	Vertical	1	+12.0	4	+ 5.0
Width of cube	Horizontal	2	+14.0	5	+10.4
Length of horizontal cyl'r	Horizontal	11	+18.3	25	+16.5
Diameter of vertical cyl'r	Horizontal	4	+12.0	28	+12.0
L'gth of horiz'l drawn cyl'r	Horizontal	17	+20.0	29	+17.4
Diam. of vert'l drawn cyl'r	Horizontal	14	+12.0	32	+11.3

the class in elementary psychology. The degree of knowledge of the illusion possessed by each observer was so nearly the same that a discussion of the observers individually will be unnecessary. The subject of visual illusions had been studied in class but a short time before and the results bring out a tendency to allow for the illusion of the vertical in those cases in which the two compared forms were both vertical. The illusion of the vertical was the only illusion studied in class which would apply to the present series. The reaction of the observers against this illusion and its significance will be considered later.

Seven of the cases were repeated from Series III. The results of these cases are given in Table XIII to facilitate a comparative study. The two series were made at different times and with a quite different set of observers but the methods of procedure were the same. It is readily noticed that, exclusive of the measurements upon the height of the cube and plate, there is a very satisfactory agreement in the results for the two series. The explanation of the two exceptions just noted will be given subsequently. Had there been a considerable disparity in the results, this could have been explained by the fact that the force of the same illusion often varies greatly with different individuals. Since, however, the results are in close agreement, this may be taken as an indication of the reliability of the tests

and the relative constancy of the illusions involved. The existence of the illusions as they are indicated for the forms in Table XIII, may be regarded as established beyond reasonable doubt. These forms have been systematically discussed under Series III, and a repetition of that discussion is therefore unnecessary here.

One of the main purposes of Series IV was the study of the illusion of the vertical. To obtain measurements of this illusion in the various forms, the same dimension, alternately in the vertical and in the horizontal position, was compared with a horizontal line; the difference between the two results gave the desired measurements. The illusion as determined for the different forms is as follows:

The plate	5.2%	Case 3 minus Case 2
The set-in cube	4.5%	Case 7 minus Case 8
The cube	3.6%	Case 6 minus Case 5
Length of the vertical cylinder	3.5%	Case 27 minus Case 25
Diameter of the horizontal cylinder	3.0%	Case 26 minus Case 28
Diameter of the horizontal drawn cylinder	2.7%	Case 30 minus Case 32
Long axis of the ellipse	2.6%	Case 17 minus Case 18
The drawn square	2.5%	Case 21 minus Case 20
Diameter of the disk	1.0%	Case 15 minus Case 14
Diameter of the circle	1.0%	Case 24 minus Case 23
Diameter of the sphere	0.9%	Case 11 minus Case 10
Length of the vertical drawn cylinder	0.9%	Case 31 minus Case 29

The results indicate that the illusion of the vertical varies with the form; in general, excluding the circle and related forms in which it is checked, it is stronger for the less complex forms. The line was not studied in this series but in the other series the illusion of the vertical is on the whole about 1% stronger in the line than in the plate. The line with an illusion of 6% would therefore head this list of forms which are arranged in the order of the strength of the illusion. These figures are low because the observers had just heard the lectures upon geometrical illusions, as was stated, and the subject was fresh in their minds. The

average is also lowered by the exceptional reaction of a few against the illusion.

A most conclusive, systematic corroboration of this law of the variation of the illusion of the vertical with the form is found in the fact that throughout Series IV the results are smaller when both the standard and compared forms are vertical than when they are both horizontal. This is seen by comparing Cases 1, 4, 9, 12, 13, 16, 19, and 22, in which the forms were vertical, with Cases 2, 5, 8, 10, 14, 18, 20, and 23, in which the forms were horizontal. In the investigation up to this point, whenever the illusion of the vertical entered into the standard and the compared forms, its effect was regarded as eliminated from the results. This, however, appears in view of the present series, not to have been justifiable. Now, it is readily seen that when the vertical line is compared with the height of some standard form, for which the force of the illusion of the vertical is less than for the line, not all of the illusion of the vertical is eliminated from the result; only so much is eliminated as occurs in the standard form and there is still present the amount by which the illusion in the line is greater than in the standard form. For instance, the illusion of the vertical is 6% for the line, and, according to the present series, it is 1% for the circle. There is a difference of 5% in its force for the two forms. The consequence of this difference is that a line 5% shorter is selected as equal to the height of the circle, whereas, if the illusion were of the same strength for the two forms, so far as the illusion of the vertical alone is concerned, a line of the same height as the vertical diameter of the circle would be selected. The amount by which the illusion of the vertical for a given form is less than the same illusion for the line must be added to the result of the comparison of the vertical line with the height of that form. In order to eliminate the effect of the illusion of the vertical when two vertical dimensions are compared, it is neces-

sary to introduce corrections for the variation with form. More specifically, in Case 4, in which the height of the cube was compared with a vertical line, 2.4% must be added to the result, this 2.4% being the difference between the illusion of the vertical for the cube, (3.6%), and for the line, (6%). Case 4 then reads 7.4%. In like manner, 1% must be added to Case 1, 1.5% to Case 9, 5% to Case 12, 5% to Case 13, 3.4% to Case 16, 3.5% to Case 19, and 5% to Case 22. After these additions have been made the cases read as indicated in the parentheses in Table XI.

The records of Series III also indicated a difference in the force of the illusion of the vertical for the standard and compared forms and attention was called at the time to the fact that not all the illusion of the vertical was eliminated from the result. (Table IX. Cf. Cases 13 and 17, 3 and 11, etc.).

The variation with form introduces very grave complications in measurement. There is need of extensive experiments to determine the general laws for this variation more comprehensively and in detail in order that it may be taken into consideration in all measurements in which it is involved.

If this difference in the force of the illusion of the vertical for the standard and compared forms were the only disturbing factor, then, due allowance having been made for it, the measurements taken when these forms were both in the vertical position should agree with those taken when both the standard and compared forms were in the horizontal position. That is, Case 1 should agree with Case 2, Case 4 with Case 5, Case 9 with Case 8, Case 12 with Case 10, Case 13 with Case 14, Case 16 with Case 18, Case 19 with Case 20, and Case 22 with Case 23. The differences in the results for these eight pairs of cases are respectively, 4.2%, 3%, 2%, 2%, 3%, 3.6%, 2.6%, and 2%. An adequate explanation for these residuals may be found in the hypothesis that in all those cases in which two vertical distances were

compared, the observers made an allowance for the illusion of the vertical by selecting a shorter line, and this allowance may have been in either the standard or the compared form.

There has frequently been occasion to note, in the course of this research, that an observer often makes an allowance for, or reacts against, a known illusion without being in the least conscious of such a reaction or allowance. Careful questioning of each observer in this series met with the response that no allowance had been made consciously. But that such allowance did play a part is most clearly brought out in the cases noted. It is important to guard against this unconscious allowance for an illusion, although there is often no means of determining whether or not it is present. It is well to ascertain, if possible, the exact amount of preparation or foreknowledge of the observer; the more naive he is the better. The fact that introspection reveals no trace of this unconscious allowance points to the need of experimenting by objective methods upon observers in a naive state of mind.

A characteristic feature of this unconscious reaction against an illusion is the fact that there is often exhibited in it a lack of logical consistency. For instance, throughout Series IV the observers apparently made allowance only in those cases in which two vertical distances were compared. (Cases 1, 4, 9, 12, 13, 16, 19, and 22). The allowance, if made at all, should have been made in those cases in which the standard form was vertical and the compared form horizontal. This would tend to show that the allowance was not consciously made. The principle that vertical distances are overestimated was familiar to the observers and this principle was more strongly suggested in those cases where the allowance was made than in any of the other cases. This inconsistency was due partly to the observers' lack of training in the experimental method. This unconscious allowance was not made by every individual observer in Series IV, but the averages of the estimates of the ob-

servers as a class indicate plainly the presence of the general tendency.

In connection with Series III, a possible explanation of the volume illusion was suggested by the comparison of the judgments upon the plate and the cube. The forms were placed before similar backgrounds, and as only one face of the cube was visible and this of exactly the same size as the plate, the objective conditions in the perception of the two forms were almost identical. The side of the cube was judged to be larger than the plate and this was explained as being due to the volume of the cube. In every normal perception there is a subjective and an objective element. Since the objective conditions in the two perceptions mentioned were so similar, it is plain that the volume illusion must be explained from the subjective side of the perception. Accordingly a crucial test of it was introduced, based upon the following hypothesis, stated in terms of representative forms: The face of a cube looks larger than a square plate of the same size on account of the association of volume with the former. That is, experience has taught the observers that voluminal objects are larger than those without volume and his judgments are influenced by this knowledge. From this point of view, then, the volume illusion is not due to physiological causes, but is of entirely subjective origin and to be classed as an illusion of association.

The experiment for the testing of this theory was arranged according to the following plan: It is plain that if objectively the plate and cube were indistinguishable and the observer had no means of knowing that two separate forms were being studied, the results of a sufficient number of trials upon each of the two forms, other things being equal, would be the same. If, on the other hand, the same tests were made with the forms still objectively indistinguishable but with this subjective difference that the observer knew when he was looking at the plate and at the cube, any difference between the results for these two forms would be

due to this fact. The nature of this difference in the subjective element in the perception of the two forms is indicated by a study of the forms themselves; the plate has area alone, the cube has area and volume. The two forms differ in no other respects. The area of the face of each is directly perceived by the sense of sight; the volume of the cube is not, but the observer knows that he is looking at a cube. The idea of volume is present in the form of the judgment "there is more to the cube than to the plate" and the effect of this is brought out in the experiments.

The height of the plate was compared with a vertical line in Case 1 and with a horizontal line in Case 3, while in Case 2 the width of the plate was compared with a horizontal line. Cases 9, 7, and 8 respectively represent parallel measurements upon the cube. The cube was closely fitted into an opening in a background, like the one upon which the plate was fastened, so that only the front face was visible and this was flush with the plane of the background. Placed in this way the plate and the cube could not be distinguished by the observer. But when each form was presented to him he was told whether he was looking at the plate or at the cube. In fact he saw the cube adjusted in place each time. In this way the conditions necessary to test the theory that the volume illusion is due to the idea of volume were met.

When in Case 1 the height of the plate was compared with a vertical line, the result was 2.5%. The result of the same measurement upon the set-in cube was 6%. There is a difference of 3.5% between the two cases. When the height of the plate is compared with a horizontal line, Case 3, the result is 13%. The same test upon the height of the set-in cube, Case 7, gave 14% as a result, or a difference of 1%. The result of the comparison of the width of the plate with a horizontal line (Case 2) is 7.7%, and the same for the set-in cube (Case 8) is 9.5% or a difference of 1.8%. These differences of 3.5%, 1%, and 1.8% represent the effect of the idea of volume. The objective conditions in the three

pairs of cases were the same. The subjective conditions differed in that in connection with the cube there was the idea of volume, but with the plate there was no such idea present and herein lies the reason that the cube was judged to be larger than the plate. The hypothesis above stated, then, stands a crucial test. The illusion of volume is an association illusion, due to the presence of the idea of volume. It does not necessarily follow from this that the illusion is due to any conscious judgment upon the part of the observer. On the contrary it is quite probable that in his experience the tendency to interpret voluminal objects as larger than those without volume has become an habitual and well fixed unconscious process.

The 3.5%, 1%, and 1.8% given above are, however, not expressions of the full force of the volume illusion. One of the chief conditions of the presence of the illusion is that the motives be unrestricted. The illusion is stronger when the image of volume in the form is clear than when it is vague.[1]

In both Series III and IV, it has been shown that the results upon the drawn cylinder are practically the same as those upon the metal cylinder. This fact is another strong proof that the volume illusion is due to the idea of volume. The drawn cylinder is perceived as a solid; that is, it is thought of as having volume. The idea of volume is present and accordingly the same illusions appear as in the regular cylinder; the volume illusion appears even though the volume of the form is only suggested.

So much for the volume illusion.[2] The area illusion has

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[1] The conditions of this test are parallel to those upon the weight illusion in which it was demonstrated that the illusion is stronger when the size of the object that is lifted is seen than when it is merely remembered. SEASHORE, *Measurements of Illusions and Hallucinations in Normal Life*. Studies from the Yale Psychol. Lab. 1895, III, p. 11.

[2] The presence of the volume illusion in the wires used in Series II may explain the discrepancy noted for those cases (Vid. p. 59).

been shown to be connected in some way with the presence of area in the form. The analogy between the conditions for the area illusion and the conditions for the volume illusion is complete; therefore the area illusion is an association illusion and of the same general nature as the volume illusion.

Turning now to the new forms, the sphere will be considered first. In Case 10 the horizontal diameter of the sphere was compared with a horizontal line, and in Case 12 the vertical diameter with a vertical line. The results are +9.5% and +2.5% respectively. These results should be compared with the corresponding measurements upon the width (Case 5), and height (Case 4), of the cube, the results of which are +10.4% and +5% respectively. It is seen that the illusions in the sphere are almost as strong as those in the cube. In this latter form the overestimation of the height and width was explained by the area and volume illusions. The sphere and cube are both typical geometrical solids and the nature of the area and volume illusions is such that they are applicable to all such objects. The overestimation of the size of the sphere is caused, then, by the area and volume illusions. However, in the sphere there may be involved something of the Müller-Lyer illusion with a tendency to bring about a slight underestimation of the diameter. It is possible that for this reason the measurements upon the sphere are somewhat smaller than those upon the cube. The sphere also has smaller area and volume than the cube and this may have reduced the force of the illusions slightly. The 9.5% of Case 10 is a statement of the combined area and volume illusions for the sphere, or better, it is the amount by which these illusions outweigh the Müller-Lyer effect. In Case 12 the result obtained is not representative of the full force of the illusions, for it is one of those cases in which the observers made allowance, and also one in which the illusion of the vertical was less for the standard than for the compared form.

The disk bears the same relation to the plate as the sphere to the cube. The area illusion would tend to cause an overestimation of the diameter of the disk. If the Müller-Lyer illusion enters, it has an opposite effect. As in the case of the sphere, in the measurements upon the vertical diameter, correction must be made for the allowance of the observers and also for the variation of the illusion of the vertical with the form. In Case 14, the result of which is +6%, there is obtained a statement of the amount by which the area illusion in the disk outweighs the Müller-Lyer illusion. The effect of the latter illusion cannot be determined from these records.

A measurement of the illusions in the ellipse is obtained from Case 18, where the horizontal line was compared with the long axis in a horizontal position. The result is +3.4%, which represents the amount by which the area illusion is stronger than the Müller-Lyer illusion.

The drawn square is but a variation of the plate and the same illusions are involved in it as in the plate. It was inserted into this series partly as a control upon the plate and partly to determine whether the mode of definition of the area had any effect upon the resulting illusions. The black area of the plate, being throughout in contrast to the background, was more clearly defined than the area of the drawn square which was merely limited by lines. By comparing Case 20 with Case 2, and Case 19 with Case 1, it is seen that the drawn square is judged to be about 1% larger than the plate, both in height and width. A possible explanation of this is that the apparent size of the plate is reduced by irradiation. The principle of irradiation has not been taken into account before as its effect was practically eliminated. There may also be physiological reasons for the difference, since the eye movements are quite different in the perception of the two forms.

The circle and the disk stand in the same relation to each other as the plate and drawn square. In Case 23 the

horizontal diameter of the circle was compared with a horizontal line; the result is the same as for the disk, +6%. For some reason the effect of irradiation is not brought out in the results.

The results of Series IV seem to warrant the following statements:

The main conclusions of Series III are corroborated by fresh evidence. (Table XIII).

The illusion of the vertical varies with the form, decreasing with the increase in the complexity of the form. (See order of forms and exceptions to this rule, p. 81).

Knowledge of an illusion causes the observer to introduce a correction for it unconsciously; such unconscious systematic allowance for the illusion of the vertical is demonstrated.

The volume illusion is shown to be an association illusion due to the idea of volume; the volume may be either real, or suggested, as in a drawing. The volume illusion varies with the prominence of the idea of volume.

By analogy, the area illusion is an assocation illusion due to the idea of area.

In the new forms, the area illusion and the volume illusion are demonstrated for the sphere, and the area illusion for the disk, ellipse, drawn square, and circle.

Series V

The tests in this series were made in the summer of 1901. The purpose was to obtain, by different methods, further measurements upon the illusion of the vertical for the line, and also to determine the area and volume illusions in some new forms. The method employed in all but the first four cases was the same as that used in Series IV. The standard forms consisted of a line, a cone, a pyramid, a triangle, a cylinder, a cube, a plate, a cone and cylinder, a pyramid and cube, and a triangle and plate.

(Fig. 1, Forms 16, 7, 6, 5, 3, 2, 1, 11, 10, and 9 respectively).

The observers were five members of the class in experimental psychology during the summer session of the University, one graduate student (Obs. 6), and an assistant-professor (Obs. 2). With reference to the illusion of the vertical these observers were of the first type, but with reference to the area and volume illusions they were of the second type.

Seventeen cases were introduced into the series and eight determinations upon each case were made for each observer in the double fatigue order. The different cases are stated in Table XIV; in Table XV the signs of the illusions involved and the results are given. As before, 1% has been subtracted to eliminate the error due to the series of lines in Cases 5 to 11 and 15 to 17 inclusive. The averages of the eight trials for each observer are given in Table XVI.

One purpose of this series was to measure the effect of the methods employed upon the illusion of the vertical, and the first five cases were arranged to test this. The methods and results for these cases are as follows:

Case 1. Method of production: to indicate on a horizontal line a distance equal to the standard vertical line. Two lines one millimeter wide were drawn at right angles upon a large sheet of light colored cardboard. The vertical line was 114 mm. in length and the horizontal line much longer. The problem was to cover with a strip of cardboard as much of the extra length of this line as was necessary to make the portion in sight appear equal to the standard vertical line. The only illusion known to enter is the illusion of the vertical, with a tendency to produce an overestimation. The result is +3.5%.

Case 2. Method of production: to indicate on a vertical line a distance equal to the standard horizontal line. This is the reciprocal of Case 1, the standard line now being

TABLE XIV.

Case	
1	To lay off distance on horizontal line equal to vertical line.
2	To lay off distance on vertical line equal to horizontal line.
3	To select horizontal line equal to vertical line.
4	To select vertical line equal to horizontal line.
	To select a horizontal line equal to:—
5	Length of line, vertical.
6	Altitude of cone, vertical.
7	Diameter of base of cone, horizontal.
8	Altitude of pyramid, vertical.
9	Base of pyramid, horizontal.
10	Altitude of triangle, vertical.
11	Base of triangle, horizontal.
12	Length of cone and cylinder, vertical.
13	Length of pyramid and cube, vertical.
14	Length of triangle and plate, vertical.
15	Length of cylinder, vertical.
16	Height of cube, vertical.
17	Height of plate, vertical.

horizontal; the method was otherwise the same. The effect of the illusion of the vertical is to produce an underestimation. The vertical line is made 2.6% too short. In these first two cases the observers probably introduced some correction for the illusion of the vertical, especially in Case 2, where by varying the vertical dimension more attention is called to the illusion.

Case 3. Method of selection: to select a horizontal line equal to the standard vertical line. Two lines were drawn at right angles upon each of twelve cards of a light tint and about 28 cm. square. One of the lines on each card was the standard, 114 mm. in length; the other line varied on the different cards by five-millimeter steps from 94 mm. to 149 mm. The observer was told to select the card upon which the two lines appeared equal, according to the method used in Series I. In this case the horizontal line selected should be too long; it was selected too long by 4.4%.

Case 4. Method of selection: to select a vertical line

Table XV.

Case	*A*	*Vl*	*C-L*	*M-L*	*V*	*L*	*% Il*
1	0	0	0	0	+	0	+ 3.5
2	0	0	0	0	—	0	— 2.6
3	0	0	0	0	+	0	+ 4.4
4	0	0	0	0	—	0	— 4.4
5	0	0	0	0	+	0	+11.0*
6	+	+	0	—	+	0	+16.0*
7	+	+	0	— ?	0	0	+ 7.7*
8	+	+	0	—	+	0	+13.0*
9	+	+	0	— ?	0	0	+ 8.6*
10	+	0	0	—	+	0	+11.0*
11	+	0	0	— ?	0	0	+ 7.0*
12	+	+	+	—	+		+11.4
13	+	+	0	—	+	—	+12.0
14	+	0	0	—	+		+10.5
15	+	+	+	0	+	0	+20.0*
16	+	+	0	0	+	0	+16.5*
17	+	0	0	0	+	0	+15.0*

A, area illusion.
Vl, volume illusion.
C-L, illusion of cylinder length.
M-L, Müller-Lyer illusion.
V, illusion of the vertical.
L, illusion of length.
% Il, percentage of illusion.
*In these cases an allowance of 1% has been made to eliminate the error due to the series of lines.

equal to the standard horizontal line. This repeats Case 3 with the positions of the standard and variable lines reversed. The illusion is 4.4%, as in Case 3.

Case 5. Method of selection: to select from the series of horizontal lines the one equal to the standard vertical line. The method of procedure was the same as in Series IV. From the result, which is +11%, it is seen that the apparent illusion of the vertical is much stronger with this method than with the two just described. The reason for this is brought out in connection with Series VII, where the effect of turning around from one form to the other is considered.

TABLE XVI.

	Obs 1		*3*		*5*		*2*		*4*		*6*		*8*		*Ave*					
Case	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*Il*	*% Il*	*% D*	*Cc*
1	113	2	118	2	117	2	127	2	115	3	117	3	120	5	118	3	+ 4	3.5	1.3	1
2	111	2	111	4	110	1	112	3	110	3	108	2	114	3	111	2	— 3	2.6	0.9	2
3	117	2	119	0	118	2	121	2	119	0	120	2	118	2	119	1	+ 5	4.4	0.9	3
4	112	3	109	0	109	0	110	2	108	1	108	1	110	4	109	1	— 5	4.4	0.9	4
5	120	1	132	4	133	6	125	3	127	4	129	8	130	3	128	4	+14	12.0	1.3	5
6	125	6	135	5	145	2	131	3	136	2	133	7	128	8	133	5	+19	17.0	4.4	6
7	117	3	128	4	132	3	122	3	127	4	121	8	124	8	124	5	+10	8.7	3.5	7
8	117	3	135	4	142	3	128	3	133	6	130	5	123	5	130	4	+16	14.0	5.2	8
9	116	3	130	3	130	6	122	3	125	2	123	6	127	5	125	4	+11	9.6	3.5	9
10	118	5	134	4	131	3	127	3	140	3	129	6	120	6	128	4	+14	12.0	5.2	10
11	114	4	122	4	128	4	121	2	131	5	122	4	120	7	123	4	+ 9	8.0	3.5	11
12	264	3	234	3	268	3	256	3	256	3	260	12	237	9	254	5	+26	11.4	4.4	12
13	264	8	239	5	269	1	257	2	249	6	267	8	243	12	255	6	+27	12.0	4.4	13
14	255	8	234	7	262	11	255	2	250	8	269	5	242	10	252	7	+24	10.5	4.0	14
15	128	4	145	2	147	3	132	5	148	2	140	5	138	4	138	4	+24	21.0	5.2	15
16	122	3	138	4	142	6	132	3	143	2	128	4	138	5	135	4	+21	17.5	5.2	16
17	118	2	135	3	140	9	129	0	143	2	128	4	130	3	132	3	+18	16.0	5.2	17

Notation same as in Table I.

Another of the main purposes of this series was to study the area and volume illusions in some new forms. These forms will be considered in order.

In the cone the area and volume illusions enter, tending to produce an overestimation of its size. The Müller-Lyer illusion is in all probability very strong for the altitude, but it is uncertain whether or not it has any effect upon the diameter of the base. The +16% of Case 6, where the altitude was compared with a horizontal line, represents the final balancing of the illusion of the vertical (+), of the area illusion (+), of the volume illusion (+), and of the Müller-Lyer illusion (—). That is, the three illusions outweigh the fourth by 16%. In Case 7 the diameter of the base of the cone was compared with a horizontal line. The area and volume illusions enter and there is probably a little of the Müller-Lyer effect. The line was selected 7.7% too long as a result of these illusions.

The illusions in the pyramid correspond to those in the cone. When the altitude is compared with a horizontal line (Case 8), the illusions amount to 13%, and when the base is compared with the line the illusions amount to 8.6%. The altitude of the cone is judged to be 3% greater than the altitude of the pyramid, and the base of the cone 1% less than the base of the pyramid.

In Case 10 the altitude of the triangle was compared with a horizontal line. The illusion of the vertical enters to produce an overestimation and also the area illusion with a like tendency. The Müller-Lyer illusion is doubtless involved, causing an underestimation. The final result is +11%, which represents the amount by which the illusion of the vertical and the area illusion exceed the Müller-Lyer illusion. The base of the triangle was compared with the horizontal line in Case 11. The Müller-Lyer illusion, although much reduced, is probably present. The line is selected 7% too long, which represents the area illusion minus the Müller-Lyer illusion. The base of the triangle is judged to be 0.7% less than the base of the cone and 1.6% less than the base of the pyramid. These differences are due to the volume illusion in the cone and pyramid, but they do not represent the full force of these illusions, for the Müller-Lyer illusion is involved. This illusion is stronger for the base of the cone and pyramid than for the base of the triangle, as is shown by the fact that the force of the area illusion (Case 11) is about normal, but the volume illusion in the cone and pyramid is too small.

Determinations upon the cylinder, cube, and plate were repeated so as to have measurements upon them singly in the same series in which they appeared in combinations. By comparing Cases 15, 16, and 17 of this series with Cases 27, 6, and 3 respectively of Series IV, a very close agreement will be observed.

The cone was placed upon the cylinder and the total length of the two forms was compared with a horizontal

line in Case 12. The purpose of these tests upon some of the forms in combination was to bring out variations in the illusions as a result of combining the forms. It was also desirable for purposes of comparison to make analytical studies of forms which were in a way typical of some natural objects which were being studied. The result obtained for the cone and cylinder in combination represents the effect of at least five illusions, but one of which, the Müller-Lyer illusion, causes an underestimation. The form is overestimated on account of the illusion of the vertical, the area illusion, the volume illusion, and the illusion of cylinder length. It is also possible that a sixth illusion enters; this is the illusion of length, according to which double distances are overestimated, and its ultimate effect, if it is involved, is to cause the selection of a shorter line, (—). This illusion enters into any form in which there is a motive to bisection, and it is weakest for those forms which are most easily bisected.[1] In judging the length of such a form as the cone and cylinder, the observer naturally estimates the distance by halves, and by so doing he introduces the conditions under which the illusion of length operates, and the length of the form is then overestimated. But when he bisects the linear dimension of the standard form measured, he must also bisect the compared line, and immediately the illusion of length enters into this form also, and it is likewise overestimated. However, it is easier to estimate the standard form in halves than the compared form, for the former actually consists of two pieces placed one upon the other, while the latter is simply a straight line. The illusion of length, then, would be greater in the line, because it is bisected with greater difficulty, hence a shorter line must be selected to cancel the effect of the overestimation. The influence of the illusion of length is therefore represented by the minus sign. If the observers

[1] Seashore and Williams, Op. cit., p. 597.

did not bisect the standard form, *i. e.*, judge it in halves, the illusion of length did not enter. In making his judgments Obs. 2 followed this method and for him all six of the illusions enter in the perception of the cone and cylinder and the other double forms. The total height of the cone and cylinder is 228 mm., but the observers selected as equal to it a line of 254 mm., or one 11% too long. (No allowance for the series of lines has been made for these double forms, for it is probably less than 1% for these longer lines). The sum of the results taken upon the forms singly is 18% of 228; that is, when the cone and cylinder are in combination, there is a reduction of the total illusion. This is probably due in part to the presence of the illusion of length in the double form.

The illusions involved in the pyramid and cube when in combination are, with the exception of the illusion of cylinder length, the same as those in the cone and cylinder. The result for Case 13, where the length was compared with a horizontal line, is 27 mm., or +12% of the standard distance. The sum of the corresponding determinations for the forms taken separately is +15% of 228, or 3% more than the result for the forms when combined.

In the combined triangle and plate the number of illusions is reduced to four: the illusion of the vertical (+); the Müller-Lyer illusion (—); the area illusion (+); and the illusion of length (—). The total result is an overestimation of 24 mm., or +10.5%, as against +13% when the measurements are made upon the single forms.

The mean variation for these double forms is greater than for the single forms because the conditions are more vague and indefinite.

A complete analysis of the illusions involved in the forms in Series V is found in Table XV. The results of this series justify the following general conclusions:

The apparent strength of the illusion of the vertical varies with the method employed.

The area illusion is demonstrated in the triangle and the combined triangle and plate; and the area and volume illusions are demonstrated in the cone, pyramid, cone and cylinder, and pyramid and cube.

The illusion of length is present in both the standard form and the compared line when there is a motive to bisection as in the double standard forms.

The total effect of the illusions in the length of the double forms (Fig. 1, Forms 9, 10, and 11) is not as great as the sum of the corresponding illusions in their components.

Series VI

Series VI, the records of which bear the date of July, 1901, was planned to show the effect of distance upon some of the illusions studied in the previous series.

The observers were six young women, members of the class in elementary psychology in the summer session, and on the whole they may be classed as observers of the second type.[1] Four trials on each case were made by each observer.

The effect of distance upon the illusion of the vertical in the square, upon the illusions in the cylinder, and upon the illusion of the vertical in the line, was studied. A, B, and C, following, refer respectively to the square, the cylinder, and the line.

A. Case 2 of Series I and II was repeated with the observers standing at different distances. The problem was to find the square plate at a distance of 1 meter (Case 1); at 3 meters (Case 2); at 6 meters (Case 3); at 9 meters (Case 4); and at 12 meters (Case 5). The results are given in Table XVII, from which it is seen that the illu-

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[1] Observer 9 has astigmatic eyes. Right eye has 1.50 diopters of simple myopic astigmatism, axis 95 degrees; left eye, 1.50 diopters of simple myopic astigmatism, axis 98 degrees.

TABLE XVII.

Obs	Case 1 (1 meter) E	d	Case 2 (3 meters) E	d	Case 3 (6 meters) E	d	Case 4 (9 meters) E	d	Case 5 (12 meters) E	d
1	104	0	107	2	107	0	110	1		
3	107	2	110	3	111	1	115	1	113	1
5	110	1	109	1	110	3	109	1	108	1
7	108	2	108	1	110	2	108	1	111	3
9	103	1	103	2	105	1	104	0	108	2
11	102	4	100	1	104	2	104	2		
Ave	106	2	106	2	108	2	108	1	110	2
Il	—8		—8		—6		—6		—4	
% Il	—7		—7		—5		—5		—3.5	
% D	2.6		2.6		2		2.6		1.8	

Notation same as in Table I.

TABLE XVIII.

Obs	Case 1 (1 meter) E	d	Case 2 (3 meters) E	d	Case 3 (6 meters) E	d	Case 4 (9 meters) E	d	Case 5 (12 meters) E	d
1	95	1	94	0	94	0	95	1		
3	101	3	102	1	106	1	103	2	107	2
5	98	3	98	2	99	3	102	3	105	2
7	105	2	105	2	108	2	104	2	108	2
9	98	3	98	3	98	2	99	3	102	0
11	89	0	88	4	92	4	94	2	94	2
Ave	98	2	98	2	100	2	100	2.5	103	2
Il	—16		—16		—14		—14		—11	
% Il	—14		—14		—12		—12		— 9.6	
% D	3		3		4		3		3	

Notation same as in Table I.

sion of the vertical in the square of this size decreases with increase in distance, the illusion being twice as strong for the nearest as for the farthest distance.

B. Case 1 of Series I and II was repeated with the distance varied as for the plate above. For a detailed description of the test and the illusions involved in the cylinder,

the reader is referred to Series II. From the results, which are given in Table XVIII, it is seen that there is a tendency for the illusions in the cylinder to decrease with the increase in distance.

C. The purpose of this part of the series was to determine the effect of distance upon the illusion of the vertical for the line. The method of production was adopted and new apparatus devised in which the lines were represented by black watch springs. A piece of light manilla cardboard was tacked upon a frame 45 cm. by 90 cm. From the front side the ends of a section of a watch spring, 3 mm. in width, were pushed through two small slits 114 mm. apart. There was thus visible, from the front, a portion of the spring 114 mm. long, which was the standard line. The variable spring was pushed through from the back of a similar background and lay closely against the front side of it. The experimenter varied the length of the visible part of this spring as the observer directed until so much was exposed as was judged to be equal to the standard. The standard line was placed in front of the observer and a little below the level of the eyes. It was also in the horizontal position throughout the experiment. The background with the variable line rested upon an easel and was on a level with the eyes and at right angles to the line of regard. In Cases 1, 3, 5, and 7, the standard and variable lines were both horizontal, but the distances for the four cases were 3, 6, 9, and 12 meters respectively. In Cases 2, 4, 6, and 8, the distances were as above, but the variable line was vertical. The results are given in Table XIX.

It is a matter of common experience that an object at a distance appears to be smaller than one of the same size close at hand: hence in Cases 1, 3, 5, and 7, in which the illusion of the vertical is not involved, one would expect the sign of the result to be plus; that is, the farther line should be made longer in order to appear equal to the nearer one. It happens that the reverse of this occurs;

TABLE XIX.

	Case 1 (*3 meters*)		*Case 2* (*3 m'rs*)		*Case 3* (*6 m'rs*)		*Case 4* (*6 m'rs*)		*Case 5* (*9 m'rs*)		*Case 6* (*9 m'rs*)		*Case 7* (*12 m'rs*)		*Case 8* (*12 m'rs*)	
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	118	3	101	2	105	1	91	5	105	9	84	1	101	2	87	5
3	119	3	115	9	130	11	126	5	131	5	122	10	134	.5	132	10
5	106	12	107	5	105	7	103	5	98	8	101	5	98	7	105	10
7	117	9	110	1	110	11	99	3	105	6	99	1	95	8	93	4
9	104	8	102	8	92	6	106	5	96	10	101	6	99	5	107	5
11	110	5	103	6	114	9	99	10	113	5	107	2	116	8	113	6
Ave	112	7	106	5	109	8	104	6	108	7	102	4	107	6	106	7
Il	— 2		— 8		— 5		—10		— 6		—12		— 7		— 8	
%Il	— 2		— 7		— 4		— 8		— 5		—10.5		— 6		— 7	
%D	5		3		8		7		8		7		10.5		10	

Notation same as in Table I.

the signs of the results are minus signs. The explanation of this is that the observers consciously allowed for the variation in apparent size with distance.[1] They reasoned as follows: a line at a distance is really longer than it appears to be; in other words, a distant line which appears to be equal to a near line is really longer than the near line, therefore, if the distant line is to be actually equal to the near line, it must appear to be somewhat shorter, and the farther away the distant line is, the shorter it must be made. This reasoning was applied consistently, for there is a gradual increase in the amount of allowance which was made. The result for Case 1 is —2%; for Case 3, —4%; for Case 5, —5%; and for Case 7, —6%. These percentages are statements of this conscious allowance. In Cases 2, 4, 6, and 8, in addition to this conscious allowance for variation in apparent size with distance, the illusion of the vertical is involved with a tendency to cause the production of a line shorter than the standard (—). Thus there

[1] This does not apply to Observer 3, whose records indicate that she made no allowance.

are in these cases two motives for making the compared or distant line too short. The results for the four cases are respectively —7%, —8%, —10.5%, and —7%. To secure a statement of the illusion of the vertical for the different distances, the amount of allowance which was made must be eliminated from these results: 2% must be deducted from Case 2, 4% from Case 4, 5% from Case 6, and 6% from Case 8. The illusion of the vertical in the line then is 5% for Case 2, 4% for Case 4, 5.5% for Case 6, and 1% for Case 8. The results for Case 8 are probably less reliable than the others, as the compared line could not be seen with distinctness at a distance of 12 meters.

The following statements may be made from the results of Series VI:

The illusion of the vertical in the square varies with distance; it decreases with the increase in distance.

The illusions in the cylinder also decrease with the increase in distance.

There is a tendency to make a conscious allowance for variation with distance in the apparent length of the line.

No appreciable variation of the illusion of the vertical in the line, with distance, is indicated.

Series VII

In this series an analysis of the methods most frequently used was attempted. It was noticed that the illusion of the vertical for the line appeared to be larger than usual when the observer turned around ninety degrees to select from the series of lines one equal to the standard line. This is well illustrated in the first five cases in Series V. As the validity of all the conclusions drawn rested to some extent upon the reliability of the method employed, it was seen to be necessary to make a very careful study of the methods of production and selection used in the tests. The

illusion of the vertical for the line was taken as a medium for this study.

The six persons who served as observers were members of the laboratory class in experimental psychology in December, 1901. They had paid but little attention to illusions for about a year; they knew of the illusion but were not conscious of making allowance and they may therefore be classed as observers of the first type. The comment of one observer, that he had forgotten which way the illusion of the vertical ought to work in the various cases and so had considered it unsafe to try to allow for it, was a characteristic remark. When several different cases follow each other in rather rapid succession, the observer who has no practice in this experiment becomes, in a way, confused and usually abandons any attempt to react consistently against an illusion.

There were eight cases in the series and each observer made sixteen trials in each case. Half the number of trials, in the double fatigue order, were made at one sitting. In the first four cases the method of selection was employed.

Case 1. To select a horizontal line equal to the vertical standard line.

Case 2. To select a vertical line equal to the horizontal standard line.

Case 3. To select a horizontal line equal to the horizontal standard line.

Case 4. To select a vertical line equal to the vertical standard line.

The method employed in these four cases was the same as that used in Case 5 of Series V (p. 93). The observers turned upon a stool from one background to the other and selected from a series of lines one equal to the standard line. The lines were on a level with the eyes and one meter away.

In the remaining four cases the method of production was used.

Case 5. To produce a horizontal line equal to the vertical standard line.

Case 6. To produce a horizontal line equal to the standard horizontal line. The backgrounds fitted with watch springs as described for Series VI, were used. The standard and compared lines occupied the same position, relatively to each other, as in the first four cases; that is, the observer turned from one to the other.

Case 7. To produce a horizontal line equal to the vertical standard line.

Case 8. To produce a vertical line equal to the horizontal standard line.

Watch springs were again used for lines, but both the standard and variable lines were upon one background, which was 90 cm. square. Thus the effect of turning and the error due to the series of lines were eliminated. The two lines were at right angles to each other but the diagonal distance between the two adjacent ends was 114 mm.

The results for this series are given in Table XX, the study of which brings out several important facts.

The first of these to be considered is the effect of the series of lines upon the apparent length of the line. In

TABLE XX.

	Case 1		*Case 2*		*Case 3*		*Case 4*		*Case 5*		*Case 6*		*Case 7*		*Case 8*	
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	127	8	111	5	120	4	114	4	122	6	119	3	119	3	107	3
3	124	5	106	3	115	3	112	3	127	4	116	3	121	3	110	2
2	125	7	112	5	118	4	115	4	124	4	118	4	120	5	110	5
5	124	4	117	2	121	4	120	4	119	3	116	3	117	2	117	2
7	122	3	110	2	116	2	113	1	119	3	115	2	118	3	103	2
9	123	6	111	4	118	5	115	3	124	4	115	3	120	2	109	2
Ave	124	6	111	4	118	4	115	3	123	4	117	3	119	3	109	3
Il	+10		— 3		+ 4		+ 1		+ 9		+ 3		+ 5		— 5	
%Il	+ 9		— 2.6		+ 3.5		+ 0.9		+ 8		+ 2.6		+ 4.4		— 4.4	
%D	1		1.6		1.6		1.7		2		1.6		1		2.6	

Notation same as in Table I.

connection with Series III, it was stated that when a line stands among others in a series it looks too short by 1% and proper allowance was made for this error in every series in which it occurred. This 1% was obtained from a comparison of Cases 1 and 5, and 3 and 6 of the present series. The standard line was vertical and the compared line horizontal in both Cases 1 and 5, but in the former instance the compared line was one of a series and in the latter it stood alone. All the other conditions were as nearly the same as it was possible to make them. The difference between the results for the two cases is an indication of the effect of the series of lines. The result for Case 1 is +9% and for Case 5 it is +8%; there is a difference of 1%. Cases 3 and 6 are parallel to these two except that the illusion of the vertical is not involved in either. In both instances the standard and compared lines are horizontal, but where the compared line is one of a series (Case 3), the result is 0.9% greater than when it is alone (Case 6). A line placed in such a series as this (see p. 60) then, looks too short by 1%, and a line longer by a corresponding amount is selected by the observers.

This error due to the series of lines may be termed an illusion of position; it enters as a result of the position of the line in a series. Only one line was fixated at a time but the other lines were in the indirect field and so influenced the line attended to. It is probable that this error in the series of lines varies with different individuals, with the position (whether vertical or horizontal) of the line, and with the distance apart and arrangement of the lines upon the background, but for want of time the effect of these various conditions could not be measured. The allowance of 1% has been made in all cases into which this error enters upon the assumption that the error due to the series of lines was of approximately the same force for the several conditions noted.

The effect of turning from one background to the other was also determined. Cases 5 and 7 were alike in all respects except that in the former the observer looked from one line to the other, while in the latter both lines were in the field of vision at the same time. The illusion in Case 5 is 3.6% greater than in Case 7, and this difference must in some way be due to the turning. In Case 3 the illusion of the vertical is not involved, but, after the elimination of the error due to the series of lines, there is a residual of 2.5%, which is another statement of the effect of turning. Still another measurement of this is obtained from Case 6, where it amounts to 2.6%. These three statements of the effect of turning from the standard to the compared line are gained from independent sources and they are all in the same direction. The error is not peculiar to one method, for the same tendency is brought out both by the method of selection (Case 3) and the method of production (Case 6). It cannot be explained upon the hypothesis that the turning increases the illusion of the vertical by giving greater freedom, because the illusion of the vertical is not involved in either Case 3 or Case 6, in which this error is about the same as for those cases in which the illusion enters. The general law that the second of two equal stimuli appears greater in intensity, as in the case of sounds and weights, does not apply, for the observer looked back and forth from one line to the other, and furthermore, the error is not in the right direction to be accounted for in this manner.[1] The effect of this illusion due to turning from one line to the other, is to increase the apparent length of the standard line; this is shown by the selection or production of a line longer than the standard as equal to it. It was thought best not to make any allowance throughout the several

[1] The observer was allowed to turn back and forth and make several comparisons between the standard and the compared forms before giving his judgment. Therefore one cannot say that the standard was the first, and the compared form the second stimulus.

series for this error due to turning, as this was a constant element in all the tests and so has no effect upon the relative value of the results nor the conclusions drawn from them, other than to reduce slightly some of the percentages given.

It will perhaps be wondered why the result for Case 2, which is —3.6% after the 1% has been subtracted for the error due to the series of lines, is not the same as for Case 1, since apparently it is only the reciprocal of it. Two reasons may be given for the lack of a closer correspondence in the two cases. One is that more attention is called to the illusion of the vertical when the vertical line is varied, and there is therefore a stronger tendency to allow for the illusion. A second reason is that the apparent length of the standard line is increased by turning, and this results in the selection of a longer compared line, thus reducing the apparent force of the illusion of the vertical in Case 2.

Case 8 was introduced as a check upon Case 7, the same illusion being involved in both cases. The method of production, in which neither the error due to the series of lines nor that due to turning was involved, was used. The results for the two cases agree; and moreover they agree also with Cases 4 and 3 of Series V, in which the same method of production was used but with different apparatus and other observers.

In the method of selection as most frequently used, it is found then that the error due to the series of lines is 1% (Case 3 minus Case 6); and that the effect of turning is 3.6% (Case 5 minus Case 7). By the method of production, in which these errors were not involved, the illusion of the vertical for the line is 4.4% (Cases 7 or 8). In Case 1, the illusion of the vertical, the error due to the series of lines and that due to turning are all involved. The result for this case is 9%, or an exact equivalent of the sum of these three tendencies (4.4%, 1%, and 3.6%), the statements of which were obtained from sources entirely independent of Case 1.

The following conclusions may be drawn from this series:

The apparent length of the line is decreased when the line is one of a series like the one employed.

In turning ninety degrees around from the standard to the compared line, the apparent length of the standard is increased.

The illusion of the vertical is the same for either the method of production or the method of selection after the elimination of the error due to the series of lines and that due to turning. The variation shown in Series V is therefore only apparent.

Series VIII

This series was arranged to determine the effect of practice upon the illusion of the vertical for the line.[1] In this form the illusion of the vertical seems to be the only illusion involved. The records are dated January, 1902. A series of one thousand tests was made upon each of three observers. There were ten periods of practice for each observer and one hundred trials were made at each period. These ten periods of practice occurred on successive days, except Saturdays and Sundays. Each period was about an hour long and with one or two exceptions the hour of the day was the same for each observer.

[1] Professor Judd has made a study of the influence of practice upon the Müller-Lyer illusion. Several hundred measurements were made upon each of two observers with the result that the force of the illusion decreased rapidly with practice. The nature of this improvement with practice was regarded as "a change in the perceptual process, which change has taken place through repeated efforts to deal directly with the objects perceived." This perceptual process was regarded as "uninfluenced by expectation." Psychol. Rev., 1902, IX, p. 27. The announcement of Professor Judd's conclusion in regard to the Müller-Lyer illusion was the immediate occasion for making these experiments upon the illusion of the vertical, although the study of the effect of practice was a part of the original plan.

Observer 1, a young lady, seemed to be entirely unaware of the existence of any illusion. She was a careful observer, wholly without practice, and her judgments were naive. At no time was there a suggestion made from which she could obtain any hint as to how she was progressing. At the beginning of the series she was told simply that the object of the experiment was to determine whether or not practice had any influence upon the accuracy of her judgments.

Observer 2, a man of extremely phlegmatic temperament, was an advanced student in psychology. He had studied illusions and had had some practice as an observer in some of the previous tests. He aimed to avoid recalling what he had studied in regard to illusions, and according to his own statement he made no allowance whatever. Each judgment was independent of all the preceding ones and appeared correct to him. He was not told anything about his records until all the trials were completed.

Observer 4 had the advantage of knowledge of the subject of illusions and also a considerable amount of practice as observer. He was not at all sure that the illusion would come out for him, but he endeavored faithfully to make judgments which appeared to him correct. He was told nothing about the results of any of the other observers nor of his own until all the trials were completed and his own introspective account had been written out. This account is given herewith.

"January 16, 1902. The comparison has been made by fixating the middle of each of the lines, thus bisecting them and comparing the four halves rather than the wholes.[1] I adopted this method because I had formed the habit in

[1] On page 96 it was stated that when there is a motive for bisection the illusion of length enters. This does not have any effect upon these results as the standard and compared lines are bisected with equal effort and consequently the effect of the illusion of length is eliminated.

previous experiments and it seems to me to be the easiest and most accurate method of comparing. It did not obviate eye movements. I have felt no inclination to use units of measurement. I have imagined the horizontal line turned to the vertical position and superposed upon the standard line. In this act I have felt a strong inclination to make a correction of something over 6% which I estimate my normal illusion to be under these particular conditions. This 6% is only an estimate because I have never measured my illusion under these conditions before. The new factor here is the distance between the two lines. The method of adjusting the variable line is also new. The correction tends to enter irresistibly in the perceptive process. I am unable to tell whether I have made any correction or not. But, from my point of view, the conditions have been uniform throughout each period and throughout the whole series. If the results show any systematic variation in the progress of the experiment, I think it may be due to practice. I have asked the experimenter to mark certain records with which I felt especially well satisfied. There were marked differences in the certainty that I felt.''

The method of production was employed by means of a new apparatus which proved exceedingly satisfactory. The lines to be studied were represented by watch springs as in the foregoing series. (Standard length, 114 mm.; width of spring, 2 mm). The observer adjusted the length of the compared line (the adjustable spring) by moving a lever. The lever was pivoted at a point 1 meter back of the background. From this point an arm extended to the back side of the background where it was connected with a sliding guide, to which one end of the spring was attached; the other arm of the lever was 2 meters long and came within convenient reach of the observer. This enabled the observer to make his own adjustments quickly and accurately, and eliminated the personal element of the experimenter.

In the statement of the results for each observer, the one

TABLE XXI. *Observer 1*

	1st day		*2d day*		*3d day*		*4th day*		*5th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	139.7	4.3	135.5	2.6	145.2	1.8	138.6	2.4	142.4	2.8
	132.7	3.1	137.8	3.4	143.6	2.4	142.3	4.1	142.3	2.3
	134.2	4.4	136.7	1.1	144.3	2.0	141.3	2.5	141.8	3.0
	138.6	2.9	134.1	3.5	142.3	2.4	137.8	4.6	137.2	2.6
	136.3	4.0	134.9	4.5	140.3	2.3	135.4	3.2	138.0	1.2
	134.9	3.1	134.7	2.5	136.5	2.5	137.2	3.8	141.3	2.1
	138.2	4.2	134.2	3.0	138.1	2.9	139.6	2.4	138.9	2.1
	138.1	4.3	134.6	2.2	138.8	3.2	138.9	3.1	137.1	1.9
	140.3	3.5	135.5	2.5	141.3	2.1	137.7	2.7	137.1	1.3
	140.5	1.3	135.2	1.9	141.8	2.8	135.4	2.8	136.3	2.1
Ave	137.4	3.5	135.4	2.7	141.2	2.4	138.4	2.2	139.2	2.2
%Il	20.2		18.4		23.7		21		21.9	

	6th day		*7th day*		*8th day*		*9th day*		*10th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	143.3	1.7	140.6	2.2	142.9	2.4	137.6	1.6	141.8	2.0
	142.1	2.1	138.6	2.6	143.9	2.5	138.1	2.3	141.5	2.5
	140.3	3.5	132.8	1.0	138.1	2.1	137.9	1.3	139.8	2.0
	141.9	1.9	136.3	2·5	136.0	1.2	138.7	1.9	137.4	2.0
	137.9	3.0	135.8	1.2	135.2	1.4	137.0	1.8	135.8	1.6
	141.8	1.6	138.0	2.4	134.8	2.6	137.0	2.2	134.4	2.0
	139.3	2.5	137.2	2.6	137.5	1.3	134.5	2.7	139.1	3.3
	136.7	1.7	134.3	0.9	133.8	2.4	138.1	3.1	137.2	3.0
	140.0	1.8	136.3	1.7	133.6	2.2	136.2	1.8	137.3	1.9
	138.0	1.2	132.7	1.7	136.0	1.6	137.6	2.0	135.9	2.3
Ave	140.2	2.1	136.3	1.9	137.2	2.0	137.4	2.1	138.0	2.3
%Il	22.8		19 3		20.2		20.2		21	

Final average, 1000 trials, 138 mm. or an illusion of 21%. Mean variation, 2%.

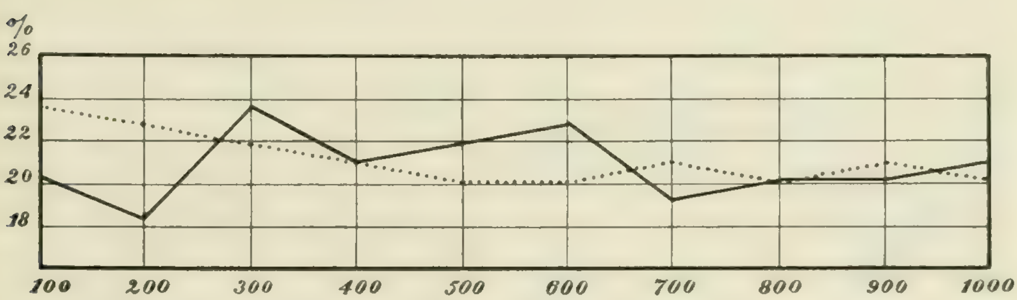

FIGURE 2

TABLE XXII. *Observer 2*

	1st day E	1st day d	2d day E	2d day d	3d day E	3d day d	4th day E	4th day d	5th day E	5th day d
	139.4	6.4	120.7	7.2	136.5	5.4	137.1	3.5	135.6	4.6
	142.0	3.6	126.1	4.7	140.7	3.1	138.4	2.0	134.6	3.0
	139.6	5.7	128.4	5.4	143.7	2.5	138.1	2.3	136.0	8.0
	146.0	3.2	126.1	2.5	144.3	3.9	142.6	1.2	145.7	3.3
	135.4	4.6	133.1	2.1	155.3	3.5	136.5	4.7	138.3	3.3
	138.0	6.2	134.0	4.0	145.0	8.4	144.6	6.6	142.8	4.4
	134.7	6.9	140.3	3.1	144.8	4.0	143.1	2.9	145.5	2.7
	138.0	5.2	138.3	4.5	141.4	3.0	139.9	5.1	142.2	3.0
	136.5	4.3	137.5	4.1	140.9	5.5	141.2	5.4	139.1	4.3
	135.6	6.0	137.4	4.4	143.9	3.9	143.7	3.9	135.9	1.3
Ave	138.5	5.2	132.2	4.2	146.7	4.3	140.5	3.8	139.6	3.8
%Il	21.9		15.8		29		23.7		22.8	

	6th day E	6th day d	7th day E	7th day d	8th day E	8th day d	9th day E	9th day d	10th day E	10th day d
	139.8	2.2	137.2	3.4	141.7	2.7	135.7	1.7	136.0	1.2
	136.4	4.4	137.1	2.9	140.9	3.5	136.0	1.1	136.3	0.8
	140.7	2.9	136.9	1.7	138.3	3.3	134.4	1.6	134.5	1.1
	140.2	3.2	141.0	4.8	132.3	1.1	134.9	1.9	137.2	2.4
	142.0	4.4	139.2	3.6	132.8	1.4	135.5	0.9	134.9	1.9
	143.6	1.4	134.5	3.5	133.1	1.5	136.2	1.7	136.2	0.8
	140.3	2.3	136.0	3.0	131.0	3.6	139.8	1.8	137.8	2.0
	136.0	3.2	135.4	4.0	133.5	0.9	137.6	1.6	134.8	1.4
	139.1	3.1	138.3	2.3	134.7	1.7	135.7	1.3	134.6	1.0
	132.9	2.7	142.2	3.1	132.9	2.5	134.6	1.6	135.4	1.0
Ave	142.7	3.0	137.8	2.9	135.1	2.2	136.0	1.5	135.8	1.4
%Il	25.4		21.9		18.4		19.3		19.3	

Final average, 1000 trials, 138 mm. or an illusion of 21%. Mean variation, 2.7%.

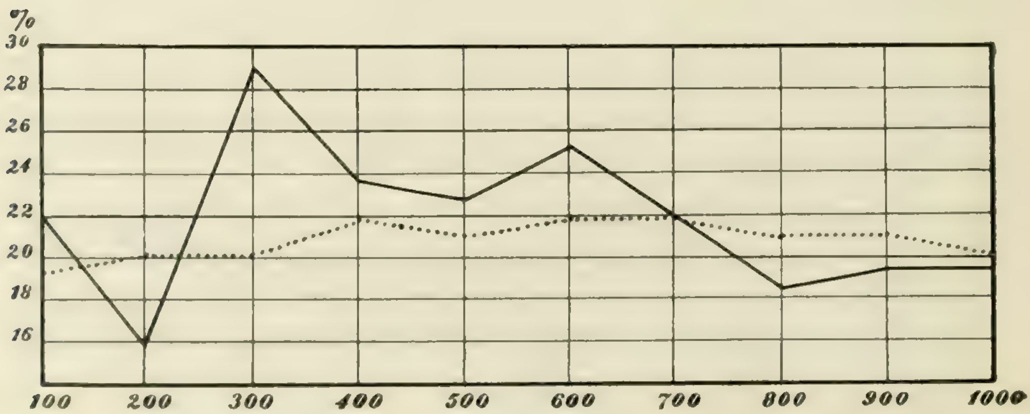

FIGURE 3

TABLE XXIII. *Observer 4*

	1st day		*2d day*		*3d day*		*4th day*		*5th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	123.9	2.2	121.2	1.0	121.0	1.6	116.4	2.8	120.2	2.2
	120.6	2.6	125.1	1.9	121.5	2.1	113.3	1.7	117.9	1.7
	121.1	1.5	119.2	1.4	123.9	1.3	114.7	0.9	117.8	1.2
	119.3	1.5	119.0	1.2	122.0	1.0	114.7	1.3	114.6	1.6
	118.1	1.3	120.1	1.3	122.5	1.6	113.8	1.6	115.4	1.4
	118.0	1.0	120.6	1.6	124.7	0.7	113.8	1.1	115.5	1.4
	117.5	1.9	120.7	1.1	124.3	0.9	114.1	0.7	121.3	2.2
	119.3	1.8	120.2	2.2	122.7	1.1	117.0	1.2	117.2	1.0
	120.5	2.1	119.2	1.2	122.7	1.7	118.7	1.7	118.6	1.6
	119.8	1.8	117.3	1.9	123.2	1.8	117.0	1.8	116.7	1.5
Ave	119.8	1.8	120.3	1.5	122.9	1.4	115.4	1.5	117.5	1.6
%Il	5.2		5.2		8.0		0.9		3.5	

	6th day		*7th day*		*8th day*		*9th day*		*10th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	119.4	2.0	120.7	1.3	122.0	1.6	123.7	1.1	122.1	1.1
	119.4	1.0	119.3	1.4	122.4	1.0	122.7	1.9	121.8	0.8
	121.0	1.2	119.0	1.3	123.7	0.5	123.7	1.5	123.6	0.8
	121.3	1.9	119.0	2.0	126.1	1.3	123.3	1.5	124.9	1.3
	120.2	1.6	120.4	1.6	124.7	1.9	125.4	2.1	124.4	2.2
	122.5	1.1	120.7	2.3	122.3	0.9	125.5	0.8	126.4	1.6
	121.4	1.8	120.4	3.6	124.5	1.7	124.8	1.4	124.0	1.0
	121.9	2.0	121.2	1.6	123.7	0.9	124.5	1.5	124.5	1.5
	122.1	1.3	120.4	1.0	123.0	1.4	125.9	1.7	124.7	1.5
	119.7	1.1	119.0	0.8	125.0	1.9	126.0	1.4	124.0	1.4
Ave	121.0	1.5	120.0	1.7	123.7	1.3	124.6	1.5	124.0	1.3
%Il	6.1		5.2		8.7		9.6		8.7	

Final average, 1000 trials, 121 mm. or an illusion of 6.1%. Mean variation 1.4%.

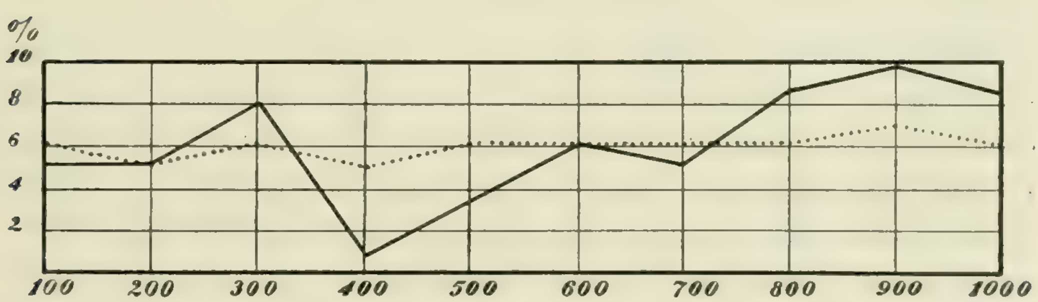

FIGURE 4

hundred trials in each practice period were divided into ten consecutive groups of ten trials each. The averages of these groups are given in Tables XXI, XXII, and XXIII, which represents respectively the judgments of Observers 1, 2, and 4. The results are represented graphically by two curves for each observer in which the numbers on the ordinates represent the percentage of illusion and the numbers on the abscissas represent the hundreds of trials. The continuous line shows the variation for the ten days and the dotted line, which is a composite of the daily curves, shows daily variation. Decrease in the force of the illusion with practice would be indicated by a fall in the curves.

The curves for Observer 1 are found in Fig. 2. The continuous line is somewhat irregular but shows no improvement as the result of practice. The dotted line indicates a slight daily improvement. The total illusion is 21%, with a mean variation of 2%.

Fig. 3 represents the curves for Observer 2. The continuous line is very irregular but it does not show any improvement worth considering. The dotted line shows no constant daily variation. The illusion, as obtained from the thousand trials, is 21%, with a mean variation of 2.7%. The strength of the illusion was very surprising to the observer himself. When shown his records and curves after the completion of the series, he was astonished at the force of the illusion, for he had been under the impression that he "was getting it exactly right."

The strength of the illusion for these two observers is unusual. It is probable that it is due to individual peculiarities, for it is known that the force of an illusion varies with different individuals, and even with the same person at different times, as the irregularities in the curves show. From previous tests it was known that the illusion for Observer 2 was strong (Series III, Observer 4), which is the reason he was chosen. The strength of the illusion for

Observer 1 was not known beforehand but it is extraordinary.

The curves for Observer 4 are given in Fig. 4. The dotted line approaches the straight line and indicates the absence of a constant daily variation. The continuous line, which represents the variation for the ten days, is rather irregular. The first thing noticed is that for the series as a whole there is no improvement as the result of practice, for the illusion at the completion of the test is fully as strong as at the beginning. On the fourth day a marked decrease in the force of the illusion is observed, for which no explanation can be offered other than that of an unconscious allowance. The observer expressly stated that he was not allowing for the illusion. At the close of the sixth period the observer watched while a few trials were made upon a little boy, and remarked that the boy should have made his estimates at least 8% shorter. By accident he saw the boy's records and was much surprised, for if the boy's estimates had been 8% smaller the illusion would not have come out at all. From this, Observer 4 concluded that he himself must have been allowing for the illusion and a rise in the curve for the subsequent tests may be due to a reaction against this. What has just been said does not apply to the first six periods and therefore fails to account for the fall in the curve on the fourth day and its rise on the fifth and sixth days. For Observer 4 the total effect of the illusion of the vertical as obtained from the average of one thousand trials is 6%, with a mean variation of 1.4%. The records with which Observer 4 was especially satisfied and which he pointed out as his best judgments are given in Table XXIV. The average illusion for these special estimates is greater than the average illusion for all the trials.

The following conclusions may be drawn from this series:

The illusion fluctuates in strength from day to day, especially for the observers who are aware of its existence.

TABLE XXIV. *Observer 4*

	1st day E	*2d* E	*3d* E	*4th* E	*5th* E	*6th* E	*7th* E	*8th* E	*9th* E	*10th* E
	117	122	124	114	123	121	123	125	124	125
	115	120	122	116	117	120	124	123	125	120
	120	118	125	117	121	122	119	126	122	123
				122	118	123	118	128	127	122
				121	117	120	116	126	124	124
				116	117	119	123	123	127	124
				116	117	121	115	125	126	127
					118	122	125	127	124	128
					116		120	124	126	127
					115		120	125	127	126
					118		119	124	128	122
					120			126	126	126
					118			126		127
								128		125
										124
Ave	117	120	124	117	118	121	120	125	126	125
%Il	2.6	5.2	8.7	2.6	3.5	6.1	5.2	9.6	10.5	9.6

Final average, 89 trials, 121.8 mm. or an illusion of 7%. Mean variation 2%.

The practice gained in 1000 trials does not decrease the force of the illusion of the vertical for the line: this is equally true of observers who know of the illusion and of those who do not know of it.

For one observer, who has had extensive experience in the observation of this illusion for years, the illusion still has a normal force.

Series IX

The purpose of this series was to supplement Series V by measuring the illusion of the vertical in some of the standard forms for which it was not measured in Series V, and incidentally to confirm further the area and volume illusions. The tests were made in February, 1902.

One of the observers (No. 2) was Observer 4 of Series VIII, and the other five were members of the class in elementary psychology. These five observers knew something about illusions and in some cases may have made an unconscious allowance. With reference to all but the area and volume illusions the observers belong to the first type. They knew nothing about the area and volume illusions.

The method of production was used as in Series VIII. To turn from the standard form to the compared line required an excursion of the head of about 30 degrees. The standard forms were placed to the right of the observer, 1 meter away, and at right angles to the line of regard.

The different cases are described in Table XXV. In Table XXVI the signs of the various illusions involved and the percentage of illusion are given. The individual records are found in Table XXVII. Four trials were made on each case by each observer. The term 'altitude' is used with its signification in geometry as a line drawn from the apex perpendicular to the base; the altitude of a form may thus be either vertical or horizontal.

Ten of the cases from Series V were necessarily repeated and a comparative list of these is found in Table XXVIII. The methods of the two series differed in that selection was employed in Series V and production in this series. In both series the observer turned from the standard form to the compared line but in Series IX the error due to the turning is less than in Series V, because the distance between the two forms is less, and accordingly a smaller illusion is to be expected in this series. A glance at the results for the single forms in the two series shows that they agree in general, but that the illusions are a little stronger in Series V, as was expected. The records on the double forms show a distinct disagreement and at present no adequate explanation for this discrepancy can be given, except that there is great uncertainty in the perception of these double forms.

TABLE XXV.

Case	*Dimension of Standard Form Measured*	*Direction*	*No. in Fig 1*
1	Length of line	Vertical	16
2	Length of line	Horizontal	16
3	Altitude of triangle	Vertical	5
4	Altitude of triangle	Horizontal	5
5	Height of plate	Vertical	1
6	Width of plate	Horizontal	1
7	Length of triangle and plate	Vertical	9
8	Length of triangle and plate	Horizontal	9
9	Altitude of cone	Horizontal	7
10	Length of cylinder	Horizontal	3
11	Length of cone and cylinder	Horizontal	11
12	Altitude of pyramid	Horizontal	6
13	Altitude of pyramid	Vertical	6
14	Length of pyramid and cube	Horizontal	10
15	Altitude of cone	Vertical	7
16	Length of cylinder	Vertical	3
17	Length of cone and cylinder	Vertical	11
18	Length of pyramid and cube	Vertical	10
19	Height of cube	Vertical	2
20	Width of cube	Horizontal	2

In each case the compared form was a horizontal line.

Statements of the illusion of the vertical were obtained by comparing the same dimension of a form alternately in the vertical and horizontal positions with a horizontal line and taking the difference between the results of the two comparisons. For instance, when the altitude of the vertical cone is compared with a horizontal line, the illusion of the vertical is involved; but when the altitude of the horizontal cone is compared with the horizontal line, it is not involved. The difference between the two comparisons is plainly a statement of the illusion of the vertical. The other illusions change but little with the change in position of the form.

TABLE XXVI.

Case	*V*	*M-L*	*A*	*Vl*	*L*	*C-L*	*% Il*
1	+	0	0	0	0	0	+11
2	0	0	0	0	0	0	+ 5
3	+	—	+	0	0	0	+ 9
4	0	—	+	0	0	0	+ 5
5	+	0	+	0	0	0	+10
6	0	0	+	0	0	0	+ 7
7	+	—	+	0	—	0	0
8	0	—	+	0	—	0	— 2
9	0	—	+	+	0	+	+11
10	0	0	+	+	0	+	+13
11	0	—	+	+	—	+	+ 2
12	0	—	+	+	0	0	+10
13	+	—	+	+	0	0	+11
14	0	—	+	+	—	0	+ 1
15	+	—	+	+	0	+	+12
16	+	0	+	+	0	+	+19
17	+	—	+	+	—	+	+ 2
18	+	—	+	+	—	0	+ 3
19	+	0	+	+	0	0	+14
20	0	0	+	+	0	0	+11

V, illusion of the vertical.
M-L, Müller-Lyer illusion.
A, area illusion.
Vl, volume illusion.
L, illusion of length.
C-L, illusion of cylinder length.
% Il, percentage of illusion.

The following statements of the illusion of the vertical were obtained:

Line	6%	Case 1 minus Case 2
Cylinder	6%	Case 16 minus Case 10
Triangle	4%	Case 3 minus Case 4
Cube	3%	Case 19 minus Case 20
Triangle and plate	2%	Case 7 minus Case 8
Pyramid and cube	2%	Case 18 minus Case 14
Cone	1%	Case 15 minus Case 9
Pyramid	1%	Case 13 minus Case 12
Cone and cylinder	0	Case 17 minus Case 11

The conclusion from Series V, that the illusion of the vertical varies with the form, is abundantly supported by

TABLE XXVII.

	Obs 2		1		4		3		6		5		Ave				
Case	E	d	E	d	E	d	E	d	E	d	E	d	E	d	Il	%Il	%D
1	124	0	137	3	128	5	118	5	130	3	126	14	127	5	+13	11	3
2	116	2	127	4	117	3	115	1	127	2	118	6	120	3	+ 6	5	4
3	125	1	132	2	130	4	116	3	120	2	118	8	124	3	+10	9	5
4	120	1	126	3	130	3	110	4	118	2	118	5	120	3	+ 6	5	4
5	123	1	138	5	135	3	113	1	127	6	121	4	125	3	+11	10	6
6	120	2	128	4	140	5	109	6	122	1	113	4	122	4	+ 8	7	7
7	242	6	253	17	251	10	193	9	207	9	226	4	229	9	+ 1	0	9
8	238	6	249	7	240	6	189	10	202	4	217	6	223	7	— 5	2	10
9	122	3	138	3	132	4	114	2	123	6	129	4	126	4	+12	11	6
10	123	4	128	5	140	4	127	1	125	3	133	4	129	4	+15	13	4
11	242	1	256	13	247	11	209	14	213	8	229	10	233	10	+ 5	2	7
12	121	2	135	3	141	3	113	5	127	4	115	6	125	4	+11	10	10
13	124	3	132	4	147	2	114	2	123	8	118	4	126	4	+12	11	8
14	247	5	250	8	268	3	204	10	198	3	221	9	231	6	+ 3	1	11
15	125	4	137	3	140	2	113	4	128	7	126	9	128	5	+14	12	6
16	130	3	140	6	154	3	121	2	138	4	134	4	136	4	+22	19	7
17	248	6	242	2	266	8	195	6	199	7	242	11	232	8	+ 4	2	10
18	154	5	246	7	271	1	192	2	192	11	251	11	234	3	+ 6	3	12
19	128	1	134	4	143	2	123	4	137	1	115	2	130	2	+16	14	7
20	120	3	128	3	149	4	118	4	132	3	110	6	126	4	+12	11	9

The notation is the same as in Table I.

the present series. Although it is difficult to group the forms in the order of their complexity, the general statement may be made that the illusion is smaller for the more complex forms, such as the cone and cylinder (Form 11), than for the simple forms, such as the line.

The presence of the area and volume illusions in this series is clearly indicated by the results. When the width of the plate is compared with the line (Case 6), the illusion of area is 7%. The differences between the results for the plate and cube give statements for the illusion of volume for the cube: 4% for both height and width (Case 19 minus Case 5, and Case 20 minus Case 6). The illusion of area for the triangle amounts to 5% plus the Müller-Lyer effect (Case 4). The volume illusion for the cone and the pyramid amounts to 6% and 5% respectively,

TABLE XXVIII.

Dimension of Standard Form Measured	*Series V* Case	*Series V* % Il	*Series IX* Case	*Series IX* % Il
Length of line	5	11	1	11
Altitude of cone	6	16	15	12
Altitude of pyramid	8	13	13	11
Altitude of triangle	10	11	3	9
Length of cylinder	15	20	16	19
Height of cube	16	17	19	14
Height of plate	17	15	5	10
Length of cone and cylinder	12	11	17	2
Length of pyramid and cube	13	12	18	3
Length of triangle and plate	14	11	7	0

In this table the signs are all plus.

plus the Müller-Lyer illusion (Case 9 minus Case 4 for the cone, and Case 12 minus Case 4 for the pyramid). In the triangle and plate combined, the area illusion and the illusion of length are outweighed by the Müller-Lyer illusion by 2% (Case 8). The volume illusion for the cone and cylinder in combination is 4% (Case 11 minus Case 8) and for the pyramid and cube it is 3% (Case 14 minus Case 8).

The following points are brought out in this series:

For the single forms there is on the whole a very satisfactory agreement between this series (method of production) and Series V (method of selection).

The illusion of the vertical for the line, cone, pyramid, triangle, cylinder, cone and cylinder, pyramid and cube, and triangle and plate, is again found to vary with the form.

The area and volume illusions are further demonstrated.

The evidences of this series, together with those of Series V, indicate that the illusion of cylinder length is present to a slight degree in the cone.

Series X

The purpose of this series was to determine the variation of the illusion of the vertical with the size of the square and with the length of the line. Up to this point the chronological order has been followed in the discussion of the various series of experiments. This series, however, preceded all the others in point of time, the records being dated November, 1899. Two sets of experiments are grouped together to form this series.

The first set was made to determine the variation of the illusion of the vertical with the size of the square. The method of production was used. A large sheet of cardboard, tinted a light pink, was placed before the observer at right angles to the line of regard. In front of this was a strip of white cardboard, the width of which was the standard distance. A strip of the pink cardboard of the same width was placed over this white strip, leaving only so much of it exposed as the observer judged to be a square. The experimenter moved this pink strip as the observer, who sat 1 meter away, directed. The standards were 38, 57, 114, 228, and 456 mm.

The observers were two university professors and five members of a class in experimental psychology, all belonging to the first type. They all knew of the illusion that was being studied, but upon request they tried not to make conscious correction for it. Forty determinations were made by each observer upon the vertical position of the standard with the horizontal dimension varying (A), and forty determinations upon the horizontal position of the standard with the vertical dimension varying (B). The results for these two positions of the standard are given in percentages in Table XXIX, A and B respectively.

The illusion of the vertical is the only one that enters. With the standard vertical, it causes the width of the form to be made too large; with the standard horizontal, it

TABLE XXIX. (*A*).

Standard	*38*		*57*		*114*		*228*		*456*	
Obs	*% Il*	*d*	*% Il*	*d*	*% Il*	*d*	*% Il*	*d*	*% Il*	*d*
2	+ 8	3	+ 4	3	+ 4	2	+ 6	1	+ 2	2
1	+ 4	4	+ 1	4	0	3	+ 1	3	+ 4	3
4	+ 1	2	— 2	5	+ 2	4	+ 7	3	+ 4	3
6	+ 5	2	+ 4	2	+ 4	2	+ 2	2	+ 2	2
8	+ 8	3	+ 5	1	+ 7	2	+ 4	3	+ 1	2
10	+13	3	+ 7	4	+11	3	+ 7	3	+ 4	2
3	+ 6	3	0	1	+ 1	2	+ 1	1	+ 1	1
Ave	+ 6	3	+ 3	3	+ 4	3	+ 4	2	+ 3	2

TABLE XXIX. (*B*).

Standard	*38*		*57*		*114*		*228*		*456*	
Obs	*% Il*	*d*	*% Il*	*d*	*% Il*	*d*	*% Il*	*d*	*% Il*	*d*
2	— 8	2	— 7	2	— 4	2	— 6	6	— 2	2
1	— 1	2	— 4	3	0	2	0	2	0	4
4	— 3	3	— 7	2	+ 2	4	+ 4	4	— 2	3
6	— 3	3	— 2	3	— 1	1	0	1	— 1	1
8	— 6	0	— 5	0	— 4	1	— 5	1	— 8	1
10	+ 3	5	+ 2	2	0	2	— 2	2	— 3	1
3	+ 6	3	+ 2	1	+ 3	2	0	3	— 4	2
Ave	— 2	3	— 3	2	— 1	2	— 1	3	— 3	2

causes the height of the form to be made too small. That is, there should be a plus sign and a minus sign respectively for the vertical and horizontal positions of the standard.

The results reveal no definite constant tendency for the illusion of the vertical to vary with the size of the square. The observers reacted against the illusion wherever there is a minus sign in Table XXIX (A), and a plus sign in Table XXIX (B), and probably they reacted to a less extent in other instances.

The second set of experiments in this series was made at about the same time and its purpose was to determine the variation of the illusion of the vertical with the length of the line. The method of production as employed in

TABLE XXX.

Standard	38		76		114		152		190	
Obs	%Il	d	%Il	d	%Il	d	%Il	d	%Il	d
2	−3	4	+5	1	+5	4	+11	5	+11	8
4	−1	1	+7	4	+9	10	+16	11	+23	15
6	−1	1	−1	2	−5	5	−8	7	−9	5
8	+2	2	+5	3	+4	1	+5	4	+3	6
12	+2	1	+7	3	+8	2	+15	4	+16	9
10	0	1	+4	4	+12	9	+13	9	+18	7
Ave	0	2	+5	4	+5	5	+9	7	+10	8

Case 1 of Series V was used. The standard lines were 38, 76, 114, 152, and 190 mm. in length and were in the vertical position. Twenty judgments were made upon each line by each of five observers and two judgments by each of ten other observers, all of whom belong to the first type. The records of the ten observers are grouped together and designated by the number 10. The records are summarized in Table XXX, where the percentage of illusion and the mean variation in millimeters are given. The results indicate that the illusion varies with the length of the line; for the five standard lines the illusion is respectively 0, 5%, 5%, 9%, and 10%.

It may be concluded that, for the observers in this series who tried the experiments upon the square, the illusion of the vertical does not vary constantly with the size of the square; but for those who tried the experiments upon the line, the illusion of the vertical shows a distinct tendency to increase in force with the increase in the length of the line.

Series XI

In this series the results of some experiments upon common objects are reported. The illusions which have been studied in the preceding series are not such as are found

only in a psychological laboratory; they are present in such forms as the book, box, hat, cup, barrel, coffee-pot, cupboard, churn, jug, basket, kettle, house, tree,—in fact in almost any object with which one has experience. For instance, it is well known that the height of the crown of an ordinary silk hat is greatly overestimated. This is usually attributed to the illusion of the vertical alone, but in the light of the present studies this accounts for only a part of the total illusion. The illusions in the silk hat are the same in kind as those in the vertical cylinder; the height of the hat is overestimated on account of the illusion of the vertical and the illusion of cylinder length, and the size of the hat as a whole is overestimated on account of the area and volume illusions. Experiments were planned to measure the illusions in some of these common forms, but for want of time they were not carried out. However, two excursions were made during which several large objects in the vicinity of the University campus were studied.

There were three observers in the first excursion and nine in the second; they will be referred to as the 3 Obs. and the 9 Obs., respectively. The 3 Obs., two instructors and an advanced student, were familiar with all the illusions described and therefore belong wholly to the first type.[1] The 9 Obs. were young men, members of the class in elementary psychology; with regard to the illusion of the vertical they belong to the first type, but with regard to the other illusions they belong to the second type. The two sets of observers did not all try the same tests.

In making the experiments four different methods were employed:

By the first method the required magnitude was marked off on a surveyor's steel tape. If the height of a building

[1] None of these observers knew anything in regard to whether the illusion would appear in the large objects, and the objects were not measured to determine their true sizes until after the first excursion.

was being studied, the observer was stationed at a distance from the building equal to the height. One end of the tape was fastened at the corner of the building a short distance above the ground, and an assistant held the other end so that the tape was stretched outward at right angles to the observer; the experimenter moved a pointer over the tape as the observer directed and thus marked off the distance from the corner and the number of feet was then read off from the tape. This method corresponds roughly to the method of production in the regular tests and for large objects it is sufficiently accurate.

In the second method the observer stationed himself at such a distance from the object as he considered equal to its height. This method was not very accurate and was used only to supplement the other methods.

The third method was the estimation of the ratio of the dimensions of the object. If for instance, the side of a building was being studied, the observer was asked: "If the height consists of ten units, how many such units are there in the length?"

The fourth method was to indicate upon the building a horizontal distance equal to the vertical; that is, the observer marked off a square. The experimenter stood close to the front of the building and moved a vertical pointer to the right or left as the observer directed until a distance was marked off which was judged to be equal to the height of the building.

The first object studied was the Home Education building. This is a large brick building with plain front. The stone foundation extends about four feet above the ground level and on this are four courses of red brick. From this to the eaves the brick is painted a dull gray. Counting from the top of the line of red brick to the eaves, the building is 32 feet high. This was taken as the standard distance. The length of the building is 60 feet.

Height, by the first method: The illusion of the vertical,

the area illusion and the volume illusion all tend to cause an overestimation. The 3 Obs. overestimated the height by 31%, (%D, 14); and the 9 Obs. overestimated by 34%, (%D, 8).

Height, by the second method: The same illusions are involved as with the first method but there are sources of error in the method. The 9 Obs. stood too far away by 69%, (%D, 16).

Height, by the fourth method: The illusion of the vertical is the only one entering in this instance. The 9 Obs. made the square too wide by 22%, (%D, 9).

Length, by the first method: The area and volume illusions tend to cause an overestimation. The result for the 3 Obs. is zero, (%D, 5); the result for the 9 Obs. is +18%, (%D, 8). The 3 Obs. probably made allowance for the illusions, as they knew of them, and the 9 Obs. did not.

Ratio of height to length, by the third method: According to the illusion of the vertical the height is overestimated and the units would be large, therefore a smaller number of them would be contained in the length; that is, the sign should be minus. The result for the 9 Obs. is —16%, (%D, 6).

The west side of the Physics Hall was also studied. This is a plain red brick building with a stone foundation. The height above the water-table is 43.5 feet and the length is 89 feet. The measurements upon it should be compared with the corresponding ones upon the Home Education building.

Height, by the first method: The effect of the illusion of the vertical, the area illusion and the volume illusion amounts to 18% for the 9 Obs.

Height, by the second method: In addition to large sources of error due to the method, the same illusions are involved as with the first method. The 9 Obs. stood too far away by 53%, (%D, 5).

Height, by the fourth method: The illusion of the ver-

tical caused the 9 Obs. to make the square 16% too wide, (%D, 11).

Ratio of height to length, third method: The illusion of the vertical enters with a minus sign. The result for the 9 Obs. is —19%. Five students in the department of Civil Engineering, belonging to the second type of observers, were asked to try this test. The result for them is —23%.

The excursion next proceeded to the smoke stack of the University heating plant. The chimney proper is octagonal and is built of buff pressed brick. It rests upon a square stone foundation which extends 20 feet above the ground level. The brick work above this is 103 feet, making the total height as used for the standard, 123 feet. A good unobstructed view of it was obtained from across the street, west.

Height, by the first method: The illusion of the vertical, the area and volume illusions and possibly the illusion of cylinder length enter. The 3 Obs. overestimated by 3%, (%D, 8); and the 9 Obs. by 15%, (%D, 9).

Height, by the second method: The same illusions enter as in the first method. The 3 Obs. stood back too far by 16%, (%D, 12); and the 9 Obs. by 28%, (%D, 12).

An isolated telephone pole, 31 feet high, was also measured. According to the first method the illusion of the vertical for the height of the pole for the 9 Obs. was 26%, (%D, 13).

The illusions in two large painted signs were measured by the third method. Both the signs, which were painted on the side of a two-story building, were taller than they were wide, the ratios being 19:10 and 12:10. In estimating the ratio of the width to the height, the height should be overestimated according to both the illusion of length and the illusion of the vertical. The results for the 9 Obs. were 21%, (%D, 10) and 17%, (%D, 3) for the two signs, respectively.

A very beautiful and symmetrical hard maple tree, standing somewhat isolated upon the campus, was also studied.

It had a distinct conical shape and was in full leaf. From the ground to the apex the tree measured 60 feet, and had a broadly spreading base. Just what illusions are involved in the perception of a tree has not been determined. With this particular tree the Müller-Lyer effect certainly was present (—), also the illusion of the vertical (+), the illusion of cylinder length (+), and the area and volume illusions (+). When the 9 Obs. compared the height of the tree with the tape according to the first method, they underestimated its real height by 13%, (%D, 5). These results were so striking that further experiments were made with seventeen observers, all but three of whom belonged to the second type. The results agree with those obtained above. When the height of the tree is estimated by the second method it is judged to be taller than by the first method. For the 17 Obs. the height of the tree is underestimated by 13%, (%D, 8); according to the second method it is underestimated by 3%, (%D, 9). This difference between the two methods is in accord with the differences found for the buildings and the smokestack. At various times different persons were asked to guess the height of the tree but none of them ever estimated its true height, the judgments as a rule being made too small by several feet. What motive for illusion can there be in the perception of the tree that is strong enough, in coöperation with the Müller-Lyer illusion, to counterbalance the combined effect of the illusion of the vertical, the illusion of cylinder length, the area illusion and the volume illusion? Further experiments will be made to determine this and how much may be due to the method of comparison. It seems to be a matter of common experience with those who have seen trees felled, that the tree looks much taller when standing upright than when lying upon the ground. It may be that an opinion to that effect may have led the observers to make semi-conscious or unconscious correction for it.

The stand-pipe at the C., R. I. & P. R. R. station is a

tall metal cylinder 52 feet high and 12.5 feet in diameter. It rests directly upon a concrete foundation 1 foot in height above the ground. No tall objects are near it.

Height, by the first method: The illusion of the vertical, the illusion of cylinder length, the area illusion and the volume illusion should cause an overestimation; but the result for the 3 Obs. is —9%, (%D, 4).

Diameter, by the first method: The area and volume illusions should cause an overestimation. The 3 Obs. overestimated by 12%, (%D, 0).

Ratio of diameter to height, by the third method: According to the illusion of the vertical and the illusion of cylinder length the result should have a plus sign. For the 3 Obs. the result is —9%, (%D, 15).

These results are confusing; the test must be repeated upon observers who are not aware of the illusions.

Measurements were also made upon two cylindrical oil tanks. The first tank was 20 feet in both height and diameter and had a flat roof. It corresponded to the equal cylinder in the regular experiments. The 3 Obs. were the only ones who tried the tests upon the tanks.

Height, by the first method: The illusion of the vertical, the illusion of cylinder length, the area illusion and the volume illusion all tend to cause an overestimation. The 3 Obs. overestimated by 15%, (% D, 15).

Diameter, by the first method: The illusions of area and volume enter with plus signs. The result is +10%, (%D, 10).

Ratio of diameter to height, by the third method: The illusion of the vertical and the illusion of cylinder length tend to to cause an overestimation. The illusion is 20%, (%D, 10).

The second tank was 16 feet high and 10.5 feet in diameter.

Height, by the first method: The combined effect of the illusion of the vertical, of cylinder length, of area and of volume is +13%, (%D, 19).

Diameter, by the first method: The area and volume illusions should cause an overestimation of the diameter. The diameter was overestimated by 10%, (%D, 10).

Ratio of diameter to height, by the third method: The effect of the illusion of the vertical and the illusion of cylinder length is +19%, (%D, 10).

There is great variation in these tests upon common objects. This is to be accounted for by the difference in the amount of knowledge possessed by the observer, by the individual peculiarities, the great uncertainty in the perception of the large forms, the complexity of the conditions, and the relative crudeness of the methods employed. The experiments are merely suggestive; they are too fragmentary to warrant any general conclusions, but in regard to the perception of a side of a building, two facts seem to be demonstrated:

The illusion of the vertical, the area illusion, and the volume illusion are all present.

The illusion of the vertical is greater for these large objects than for the small objects.

Critical Remarks

In the description just given of the eleven series of experiments the primary aim has been to present the full data to the reader and to place in the hands of future investigators records the significance of which has by no means been exhausted by the formal conclusions presented in this article. The writer has simply furnished the complete data and stated her conclusions as tersely as possible without elaborate discussion; the reader is referred to the tables of results for mean variations and all other such factors as enter into the interpretation of the records.

The discussions and conclusions have been based almost entirely upon the averages of the results in each series;

very little attention has been given to the records of the individual observers. A great deal might have been gained by a study of the individual observers; as for instance, a determination of the variation of the illusion with temperament and physical condition, the relative strength of the different illusions for the same person, their consistency under different conditions, variations with sex and mental ability, etc., but all this was prohibited by the closely defined limits of this report. The individual records are given in full and they may be reinterpreted at any time with these problems in view. The records also show the prevalence or constancy of the illusions for the different observers.

In many instances account has been taken of a small percentage of illusion where the corresponding mean variation has been rather large. This has been done only in those cases where there has been a clear motive for the appearance of the illusion and the variation has been normal for the particular method employed.

An apology must be given for a very rigid manipulation of the figures. In the rather intricate discussions of the results it was very desirable, for the sake of clearness, to make definite and, for the most part, unqualified statements. The fact was fully kept in mind that the results were only of relative value. The writer also regrets a certain inconsistency in the use of fractions. The apology is not that fractions were omitted in some instances, but that fractions were used at all. In the effort to interpret the results correctly this inconsistency was overlooked until the advantage to be gained from its correction would not justify the necessary expenditure of time.

Records of observers belonging to different types have in some instances been grouped together. Much confusion would have resulted from an attempt to keep the records of the different types separate. The observers as a class are representative of university undergraduates, chiefly

juniors and seniors. The individual observers have, however, been described with reference to their respective types.

With one exception, no attempt was made to determine the condition of the eyes of the observers, but no experiments were made upon any one whose eyes were obviously in a weak or strained condition. It is possible that some of the observers were troubled with errors of refraction in the eye and that the results were influenced by these defects.

The term illusion has been used in two ways: first, a motive, as when the conflict or struggle of illusions was spoken of; and second, an error, as the amount of illusion. The use of the word motive is self-evident.

General Summary

The following illusions have been considered more or less fully in the foregoing discussions: the illusion of the vertical, the area illusion, the volume illusion, the illusion of cylinder length, the Müller-Lyer illusion, and two illusions due to contiguity. Of these the Müller-Lyer illusion and the illusion of the vertical are well known. The illusion of length was reported in the article preceding this, which is really a part of the present research. The area illusion, the volume illusion, and the illusion of cylinder length have not, to the writer's knowledge, been reported before. The aim has been to make a detailed study of the illusion of the vertical, and to determine what other illusions enter into the perception of the size and form of the principal types of common objects.

A full account of the illusion of the vertical would include a discussion of the following general features: the prevalence and average strength of the illusion; variation with the form of lines and two and three dimensional ob-

jects; variation with size; variation with the distance from the observer; variations due to the angle at which the objects are seen; variations due to complications with other illusions; variation with the different standard methods of measuring illusions; variation with the age of the observers, and also with sex, intelligence, attention, environment, experience or familiarity, knowledge, practice, etc.; a complete discussion with crucial tests of the current theories to explain the illusion; and miscellaneous features, such as the duration of the stimulus, indirect vision, etc. The facts that have been determined in the present research will be summarized briefly.

The range of the illusion of the vertical is large; it varies for different observers from less than 1% to over 20%. The average illusion for the vertical line among adult observers who knew of it and were careful is about 6%. No case in which the illusion of the vertical did not appear was found which could not be accounted for by a reaction against the illusion or carelessness on the part of the observer. Very small percentages of illusion are usually to be explained in the same manner.

The illusion of the vertical varies with the form, being stronger for the less complex forms. The simplest form, the vertical line, shows the greatest illusion. The illusion is not so strong in two dimensional objects as in the line, and in three dimensional objects it is still weaker. It is probable, however, that this variation is, at least in part, only apparent; for in the complex forms there are many conflicting motives to illusion present and in their struggle for supremacy much of the manifest illusion of the vertical may be lost. It has also been pointed out in the preceding paper (page 32), that the effect of one illusion partly satisfies the motive of another. In the circle and related forms the illusion is, as a rule, practically absent on account of the knowledge of the geometrical relations of the forms. Under certain con-

ditions of attention, however, the illusion does appear even in the circle.

The illusion of the vertical varies with the size of the object. It increases in force with the increase in the length of the line; the same probably is true for two dimensional forms, although the experiments indicate no positive tendencies. The illusion is greater for the large buildings than for the small objects.

With reference to the variation of the illusion of the vertical with distance, the records show a decrease in the force of the illusion with the increase in distance. This is in accordance with the statement above that the illusion increases with increase in size; in other words, it is stronger for larger and nearer objects.

The angle at which the square is seen does not affect the illusion of the vertical in small objects.

After the errors incident to the methods have been eliminated, the illusion of the vertical is of about the same force for the different methods used.

Several subjective factors affect the illusion of the vertical. The illusion in the square is stronger for children (third type), than for adults (second type); that is, the illusion varies with age. This is due not so much to an undeveloped state of mind as to the fact that the children give unprejudiced judgments. The illusion was found not to vary with intelligence. With regard to the variation with sex there is practically no difference for the boys and girls in the force of the illusion, but among adults the women are less constant than the men. The illusion also varies with the direction of the attention. There are three ways in which the result may be influenced by the direction of the attention: the attention may be directed to the illusion itself; according to its nature, the motive of the illusion may be strengthened by attending to it or not attending to it; and the attention to one dimension of an object changes the apparent proportion of the other dimensions.

There is a greater incentive to make a correction for an illusion when the attention is strongly directed to it. The illusion of the vertical also varies with the knowledge of the illusion. It persists for some persons who have studied it for years, although they may have learned to make proper allowance for it. The illusion decreases in force when it is pointed out; it is stronger and more constant for the naive observers. The illusion was found not to decrease in force with the practice gained from making one thousand judgments. There is a very strong tendency to react against a known illusion. This illusion and other illusions also will lose much of their constancy when they become popularly known, just as the fixed customs of the savage dissolve under civilization.

A complete analysis of the area illusion would correspond somewhat closely to the one given for the illusion of the vertical. Only a few conditions under which this illusion varies have been determined. The experiments indicate that it is present in all objects having area, and its effect is to increase the apparent size of the object.[1] It is of about the same force for all the forms the areas of which are approximately equal. With the exception of two, none of the observers knew of the illusion and accordingly no conscious or unconscious correction was made for it.

In addition to the area illusion, the volume illusion appears in all forms in which the volume is either real or suggested. No constant variation with the different forms was found. As in the case of the area illusion, the observers knew nothing of the volume illusion and introduced no correction for it.

The initial purpose of this research was to study the illusion which has been termed the illusion of cylinder

[1] The existence of the area illusion may account for the fact that a line between two points looks shorter than the open distance. All the measurements upon areas in terms of lines, where the area illusion has not been eliminated, are vitiated by the existence of the area illusion.

length. The presence of this illusion of overestimation in the length of the cylinder has been fully demonstrated, but no adequate explanation for it has as yet been found. It is distinct from the illusion of the vertical and it is stronger than this illusion for adults. It is not due to one particular method, for it is brought out by the different methods used. It is not limited to the real cylinder, for it appears in the drawing of the cylinder. Neither is it due to a relative underestimation of the diameter, for it is present with its characteristic force in the tests where the ratio of the length and diameter is not considered. But these limitations do not determine the exact explanation of the illusion. The motive for eye-movement in the direction of the length of the cylinder is peculiar and seems to constitute an adequate motive for the illusion; but it has not been demonstrated by the experiments. There is an illusion in the length of the drawn cylinder (in which this motive for eye-movement is absent) equal to that in the real cylinder, but the two results are probably due to radically different causes: in the drawn cylinder it is manifestly a case of confluence due to the presence of the ellipse at the end. Association theories have been considered but it does not seem that they possess as high a degree of probability as the physiological theory. In Series I it was pointed out that this illusion varies with intelligence, being strongest for the dullest pupils; and in Series II it was shown that it did not vary with age. These two statements are in agreement, for an increase in age really means an increase in intelligence; the brightest children were probably as intelligent as the adults.

No direct determinations of the illusion of length were made, although it appeared several times in combination with other illusions. The incidental evidence which was gained supports the former report.

The Müller-Lyer illusion has figured prominently in the discussion of the results. It entered as a complicating factor

TABLE XXXI.

No. in Fig. 1	*Standard Form*	*Dimension of Standard Form Measured*	*Direction*	*A*	*Vl*	*C-L*	*M-L*	*V*	*L*
1	Plate (square)	Height	Vertical	+	0	0	0	+	0
1	Plate (square)	Width	Horizontal	+	0	0	0	0	0
2	Cube	Height	Vertical	+	+	0	0	+	0
2	Cube	Width	Horizontal	+	+	0	0	0	0
3	Cylinder	Length	Vertical	+	+	+	0	+	0
3	Cylinder	Length	Horizontal	+	+	+	0	0	0
3	Cylinder	Diameter	Vertical	+	+	0	0	+	0
3	Cylinder	Diameter	Horizontal	+	+	0	0	0	0
4	Sphere	Diameter	Vertical	+	+	0	—?	+	0
4	Sphere	Diameter	Horizontal	+	+	0	—?	0	0
5	Triangle	Altitude	Vertical	+	0	0	—	+	0
5	Triangle	Altitude	Horizontal	+	0	0	—	0	0
6	Pyramid	Altitude	Vertical	+	+	0	—	+	0
6	Pyramid	Altitude	Horizontal	+	+	0	—	0	0
7	Cone	Altitude	Vertical	+	+	+	—	+	0
7	Cone	Altitude	Horizontal	+	+	+	—	0	0
8	Disk	Diameter	Vertical	+	0	0	—?	+	0
8	Disk	Diameter	Horizontal	+	0	0	—?	0	0
9	Triangle and plate	Length	Vertical	+	0	0	—	+	+
9	Triangle and plate	Length	Horizontal	+	0	0	—	0	+
10	Pyramid and cube	Length	Vertical	+	+	0	—	+	+
10	Pyramid and cube	Length	Horizontal	+	+	0	—	0	+
11	Cone and cylinder	Length	Vertical	+	+	+	—	+	+
11	Cone and cylinder	Length	Horizontal	+	+	+	—	0	+
12	Circle	Diameter	Vertical	—	0	0	—?	+	0
12	Circle	Diameter	Horizontal	+	0	0	—?	0	0
13	Drawn square	Height	Vertical	+	0	0	0	+	0
13	Drawn square	Width	Horizontal	+	0	0	0	0	0
14	Ellipse	Long axis	Vertical	+	0	0	—	+	0
14	Ellipse	Long axis	Horizontal	+	0	0	—	0	0
15	Drawn cylinder	Length	Vertical	+	+	+?	0	+	0
15	Drawn cylinder	Length	Horizontal	+	+	+?	0	0	0
15	Drawn cylinder	Diameter	Vertical	+	+	0	0	+	0
15	Drawn cylinder	Diameter	Horizontal	+	+	0	0	0	0
16	Line	Length	Vertical	0	0	0	0	+	0
16	Line	Length	Horizontal	0	0	0	0	0	0

The notation is the same as in Table XV.

into many of the forms studied, therefore it was necessary to interpret the results with reference to this illusion.

Two illusions due to contiguity in space were brought out by the results. The first has been referred to as an error due to the use of a series of lines; the second, an error of a similar nature, due to the use of a series of plates. The effect of both was eliminated from the results.

In the way of a final summary, an analysis of the illusions involved in the sixteen forms studied is given in Table XXXI. The direction of the illusions is represented by the plus and the minus signs. The illusions are stated in terms of the horizontal line, for the method of production. The force of the illusions would vary with the numerous different conditions, but the direction of the illusions would remain as indicated in the table.

It gives the writer pleasure to acknowledge her obligation to all those who kindly served as observers, and more especially to Dr. C. E. Seashore who most generously gave his time and thought to the promotion of the research, with regard both to the planning of the tests and also to the arrangement of them in final form for presentation.

THE NEW PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF IOWA

BY

GEORGE T. W. PATRICK

The new Psychological Laboratory of the University of Iowa was completed and occupied in September, 1901. It is situated on the second floor of the north wing of the Hall of Liberal Arts, a new Bedford-stone structure begun in 1897. This is a fire-proof building, constructed at a cost of about $203,000, 210 feet by 120 feet, and three stories high with a basement. It is heated and ventilated by the fan system combined with direct radiation, a Sturtevant thermostat in each room maintaining the temperature at any desired degree of heat.

Previous to the drawing of the plans of the building, 4500 square feet of clear floor space were set aside for the department of philosophy and psychology, including the laboratory. Through the kindness of the Board of Regents, *carte blanche* was given to the department in conference with the architect in respect to the plans, including all details of construction and arrangement. The building committee of the Board generously granted every reasonable request to the end of securing in the outcome as perfect a laboratory as could be designed within the allotted space.

The rooms with their names and sizes are as follows:

Lecture Room	27′— 8″ × 29′—10″
Apparatus Room	15′— 7″ × 23′— 2″
Work Shop	20′— 0″ × 23′— 5″
Measuring Room	15′— 5″ × 18′— 8″
Observing Room	16′— 2″ × 12′— 7″

Private Laboratory	12′— 3″	×	20′— 0″
Research Room	16′— 2″	×	20′— 0″
Research Room	19′— 6″	×	20′— 0″
Small Lecture Room*	16′— 6″	×	23′— 9″
Library and Seminary Room*	20′— 9″	×	26′— 0″
Office	10′— 2″	×	20′— 9″
Office	9′—10″	×	23′— 2″

The arrangement of the rooms is such as secures both quiet and convenience. None of the laboratory rooms open directly upon the main hall of the building. Every room is as free as could be desired from noise and disturbance both from within and without the building, while for the two research rooms, still further seclusion is obtained by their location upon the floor above the main laboratory, from which they are reached by a private stairway from the work shop. The rooms are all thirteen feet in height, excepting the observing room which is 10 feet 2 inches in height. The floors are of maple and the other finishing is all of oak. The work shop is wainscoted in oak to the height of six feet and ceiled with oak in such a manner that, when desired, shafting, pulleys, or other fixtures may be attached for the operation of machinery. All the rooms are supplied with gas and electrical lighting. The large lecture room and the work shop are supplied with hot and cold water. The windows of all the rooms except the two offices have extra darkening shades working in grooves at the sides. These darkening shades are hung on heavy rollers boxed into the casing at the top of the window.

A switch-board is built into the wall of the work shop adjoining the battery closet. It is 2 feet by 3 feet in size,

*Owing to the recent fires, destroying two of the University buildings, these two rooms are temporarily used by another department. Meanwhile the apparatus room, which adjoins the large lecture room. is used as the library and seminary room, and one of the offices is used as apparatus room.

made of white marble set in an oak frame and contains eighty single terminals, all fused. From the switch-board electrical connections are made by concealed wires running through iron tubing in the walls and under the floors to the lecture rooms and each of the laboratory rooms, providing four circuits to each room except the observing room, which has five circuits, and the battery closet, which has six circuits. The mains from the University power-plant are also brought into the system. The switch-board is used not only for supplying the current to all the rooms but also for bringing different suites of rooms into telephonic or other electrical connection. The terminal boards in the several rooms are found at convenient places in the walls.

The construction of the observing room deserves especial mention. To make a dark room impervious to external light is a matter presenting no serious difficulty. To make a room impervious to external sound or wholly free from the jarring from surrounding rooms or adjacent streets is a problem which has not yet been solved and of course never will be. We made the attempt to approach a little nearer to this end than has hitherto been done. The result is a room as free from external disturbances as is needed in any experiments in which it is necessary to control visual, auditory and tactual stimuli. So far as this has been accomplished, the credit is largely due to the architects, Messrs. Proudfoot and Bird, who worked out many of the details of construction. The position of the observing room is central, occupying a place not otherwise desirable from lack of light. The room rests on an independent foundation, having no solid connection with the rest of the building either below, above, or on the sides. The superstructure which supports the room rests upon a sand bed and a second sand bed at a higher lever still further assists in eliminating possible jarring or sound which might be communicated from the ground. The

walls of the room itself, inside the main partitions, which separate the whole space from the surrounding apartments, are made of two four inch walls of hollow tiles separated by an air space and each covered with a thick insulating material made of sea-weed. Inside of all, the walls are plastered and then lined throughout with black broadcloth. The inside room is divided into the main observing room, 12′—2″ × 12′—7″, and a vestibule, 4′—0″ × 12′—7″. The room is entered through five doors, the outer one being an ordinary oak door and the other four specially constructed tight fitting cedar doors, covered with black cloth on the sides and edges. The doors close with strong springs and are held open by automatic catches. The floor is made of Tennessee red cedar and covered with linoleum painted black. The room is heated and ventilated by means of hot air introduced not directly but from the attic through cedar shafts provided with overlapping cloth partitions which admit the air but help to exclude sound. For very fine experiments the ventilating shaft may be closed and the room ventilated during intermissions. The room may be lighted by gas or electricity, the former being introduced through rubber tubes. The furnishing consists of black tables and chairs and a telephone connected with the measuring room. In the exclusion of sound and vibration, as well as in other respects, the room has proved to be a complete success. The loudest stentorian shouting just outside the doors is absolutely unheard within.

The work shop is supplied with a substantial work bench inclosed below for the reception of lumber; also with lathe, tool case and mimeograph, drawing materials, motors, etc., and material cases with glass doors. The measuring room is supplied with a special instrument bench. The other rooms are furnished with suitable tables, chairs, apparatus cases, a students' locker, a chart case, drawing and apparatus tables.

The library and seminary room contains the departmental library.

The equipment of the laboratory, the growth of the past twelve years, has been designed to fit the plan of instruction, according to which the student in psychology may, the first year, attend a course of lectures in which a rich collection of illustrative material is used and experiments are performed before the class by the instructor. The second year, the student may himself perform a series of model experiments, and for the third and following years he may engage in the investigation of original problems. Accordingly the equipment falls into three classes: (1) apparatus, charts and other material for use in class demonstration for the first year in general psychology; (2) a complete set of apparatus for standard exercises which constitute the laboratory course for the second year; and (3) apparatus and the varied means employed in the research work, for special tests, and for advanced demonstration experiments.

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CONTENTS

PERIMETRY OF THE LOCALIZATION OF SOUND.

BY DANIEL STARCH, A.M.

The purpose of this research is, first, to determine the data and elementary processes of the localization of sound; and second, to obtain some accurate measurements of the ability to discriminate between directions of sounds.

The localization of sound, as a field for investigation, was opened by Weber[1] in 1848, and since then various problems have been approached with more or less success. The earlv experimental contributions were crude and introductory, and only the more recent researches were made along specific lines, such as peculiarities of median plane localization, the outer ear in its relation to the localization of sound, the localization of fused sounds, etc. The keenness of perception of direction has been tested in part but the results thus far gained disagree in their essential respects, demanding more detailed consideration.

In the present investigation, typical directions in two representative series of planes (horizontal and vertical) were chosen. Accordingly, two main series of experiments were undertaken. The aim of the first was to find the least perceptible difference between directions in horizontal planes, and the aim of the second was to find the least perceptible difference between directions in vertical planes.

The apparatus consisted of the Seashore sound perimeter which was described by the designer in THE PSYCHOLOGICAL REVIEW, X., pp. 64–68. Professor Seashore has kindly consented to reprint the description in this connection.

THE SOUND PERIMETER.

Recent studies in auditory space perception have shown that the power to localize sounds rests, to a great extent, upon secondary factors. What unaided introspection would lead us to consider direct acoustic sensory data, exact experiment often reveals to be only associations or the result of subconscious

[1] *Berichte der kgl. sächs. Ges. der. Wiss.*, II., Bd. (1848), S. 237.

influences of some sort. In future experiments more attention must be paid to the elimination or control of these associations and suggestions. Within the last few years, much good work has been done in the study of the localization of sound, but all with crude and often inadequate apparatus. None of the sound cages, or substitutes for the same, which have been used, could have been operated without giving suggestions that would tend to invalidate the results. Only those who, like the writer, have been engaged in these experiments, can fully appreciate this criticism. Results have been obtained at the expense of wasted time and patience in the effort to conduct the experiments on such plans that the shortcomings of the apparatus might be overcone.

In order to be adequate for most purposes, the apparatus for the producing and registering of the sound which is to be located should permit, among others, the following variations in the stimulus without giving any suggestion or counter-suggestion to the observer: (1) the direction of the stimuli from the middle of the aural axis, (2) the intensity of each of the stimuli, (3) the distance of one stimulus, (4) the number of stimuli to be given simultaneously or in succession, and (5) the order and frequency of stimuli from a given position.

The sound perimeter shown in Fig. 1, has been designed to meet these requirements. It consists of a system of telephone receivers so mounted and connected as to make the above-named variations possible. The main frame is made of iron tubing and braced in such a way as to afford the maximum rigidity with a minimum of material which might reflect sound. The receivers through which the stimuli are produced, are mounted on movable arms, which may be denoted *A*, *B*, *C*, and *D*, respectively. Arms *A* and *B*, each representing an arc of 135° of a circle whose radius is one meter, are so mounted on a common center at the top that they may swing in the same course, describing a part of the surface of a sphere one meter in radius. Each of these arms carries a pointer, which moves under the circular scale placed above the bearings. This scale is graduated in five degree-units and marked with large figures, which may be read from the experimenter's position behind the tablet on the main support of the frame. The two arms are mounted on a common axis, but they turn on independent bearings, so that there is no friction between them. The arms are turned by means of cords which run from the experimenter's tablet up to pulleys at the top of the frame and thence to wheels mounted on the upward projections of the arms. There are two of these cords for each arm; pulling one cord turns the arm to the left, and pulling the other turns it in the opposite direction.

The third arm, *C*, turns in the surface of the same sphere as the other two arms, but is mounted on the side and counterbalanced, so that it may be turned readily by means of the crank which is seen directly above the tablet. The pointer on the crank runs over a circular scale which is graduated in five-degree units, in the same manner as the scale for arms *A* and *B*. The axle which carries this arm may be drawn back through the frame so that the arm may pass the other two arms without striking at the top, and so as to be out of the way when not in use.

Arm *C* may be removed by pulling the axle out after detaching the crank. and arm *D*, a straight rod, will fit in its place of support. In Fig. 1, arm *D* is seen only in part, being stored away on the side of the main upright of the frame, This arm carries the receiver on one end, and is graduated in centimeters for guidance in the adjustment of the distance of the receiver from the center of the

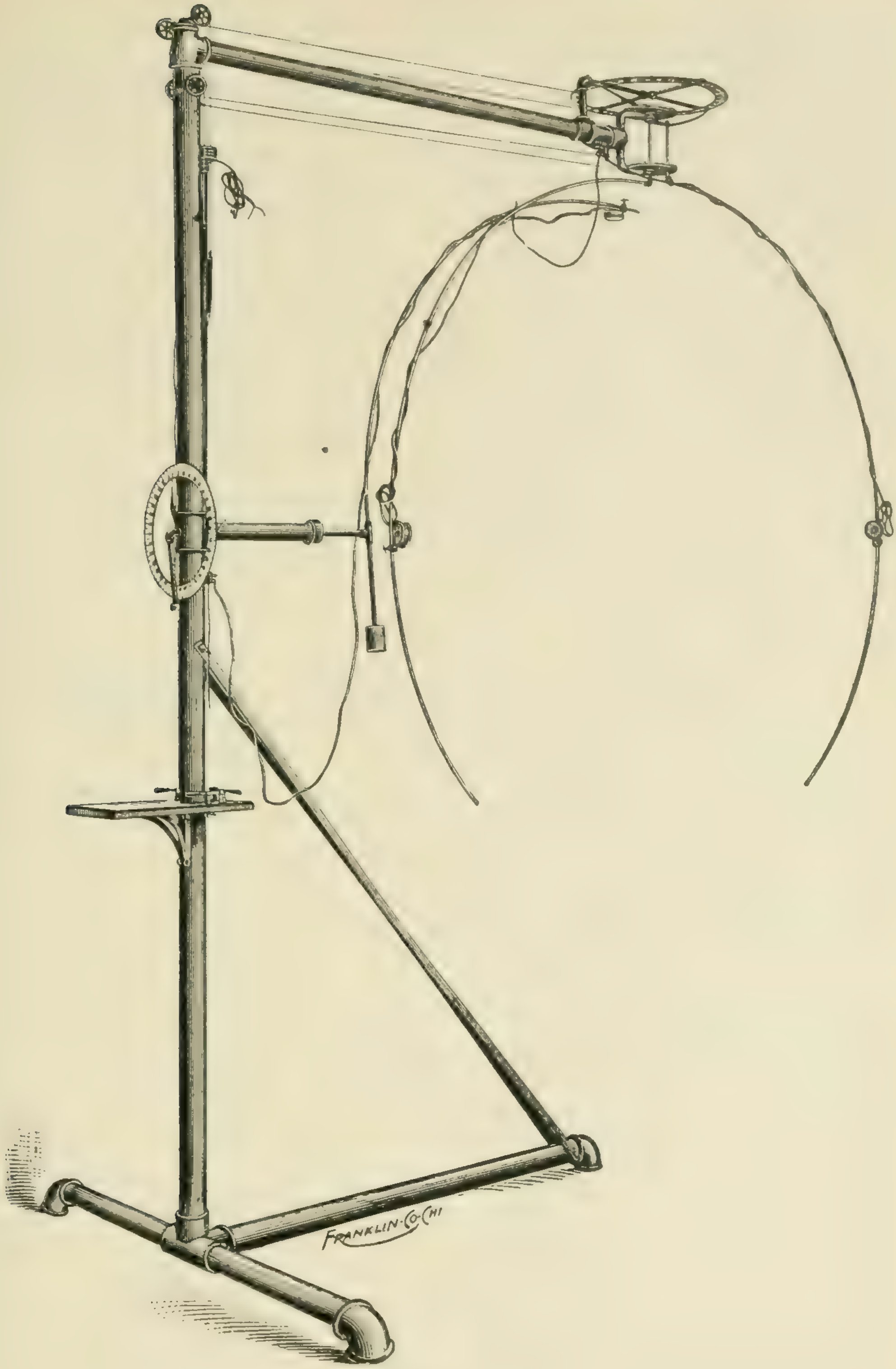


FIG. 1.

sphere. The arm is three meters long and slides freely in the horizontal direction.

For ordinary purposes only one receiver is needed on each arm, but it is evident that any number of receivers desired may be mounted on each arm for the purpose of special experiments. The receivers are clamped by a thumb

screw and may be placed in any position on the arms. In order to eliminate conduction along the arms, the receivers are insulated from their clamps by means of soft rubber.

Soft and flexible wires run from the receivers to terminals on the surface of the frame and permanent wires are laid from these terminals, inside of the frame, to the tablet. The same curcuit is used for arms *C* and *D*, as they are never used simultaneously. There is a knife switch for each circuit on the tablet and all the circuits are completed through the same battery and mercury key (not shown in the cut). Thus, when the key is pressed, a click will be heard in a receiver if the switch in its circuit is closed; and if two or three switches remain closed at the same time, the current is distributed equally to the corresponding receivers and the clicks will occur simultaneously in all.

Resistance may be put in the main circuit or in one or more of the branch circuits, as the needs may be, to vary the intensity of the click. If a dry-battery is used it may be fastened to the frame and then the apparatus will be complete without any further accessories.

To vary the quality of the stimulus, tones of different pitch may be substituted for the click. For that purpose it is necessary to have electric tuning-forks of the desired pitch in a distant room and to complete the perimeter circuit as a shunt through the fork. The tone will then be heard in the receiver whenever the key is pressed.

The center of the sphere described by the arms is 1.73 meters above the floor. A high, adjustable stool is placed under this center and adjusted for the observer so that the center of the observer's head occupies the center of the sphere. If a head rest is used, great precaution should be taken to prevent disturbing effects. It is best not to use any head rest, but to check the position of the observer frequently by putting arms *A* and *B* at opposite points and sighting across. The height is determined by reference to the axis of arm *C* or arm *D*.

The scheme for numbering the points on the scale is of considerable importance. That plan has been adopted which students tend to follow spontaneously when asked to describe the location of a point in space. In this there is no number higher than 90. The upper scale gives the reading for horizontal planes and the side scale for vertical planes. The nomenclature adopted may be described without any diagram. The horizontal scale has two zero-points, one in the median plane in front and the other in the median plane behind; *i. e.*, every point in the median plane of the head is at 0° with reference to the horizontal plane of space, and degrees are counted toward the right and toward the left from the median plane both in front and behind. In the vertical scale, the two zero-points are at the level of the ears; *i, e.*, every point in the horizontal plane through the ears is at 0°, and degrees are counted upward and downward from this level. This gives a simple and natural nomenclature for direction, *e. g.*, a point is 'in front, 15° left and 25° up.' The upper scale may be turned so that this system will correspond to any desired position of the observer.

This apparatus will favor the use of the method of right and wrong cases and the method of minimal changes, in which it is not necessary for the observer to estimate degrees. However, it is sometimes advantageous to allow the observer to indicate the direction with a pointer; the experimenter may then swing the perimeter arms to such a point and read off the result on the scales.

This brief statement, supplemented by the figure, may suffice to give a general idea of the apparatus. Its special merits are, that it enables the experimenter to stand in one place throughout complicated series of experiments and operate all the parts of the apparatus without giving any suggestion by movement or delay, that the movable parts of the apparatus are made to act without sound or jar, and that it makes it possible to vary, measure, and control the essential factors. C. E. SEASHORE.

The perimeter was so located in an almost cubical room (20 ft. × 16 ft. × 13 ft., and containing only the necessary apparatus) that the center of the sphere described by its arms was equidistant from the walls of the room. In order to approximate perfect uniformity in the reflection of the sounds from all directions still further, the sounds were always given on the same side, and instead of swinging the arms of the perimeter to the different standards, the observers turned to the required standard positions. Following the adopted nomenclature of the perimeter, the points used as standards can be most easily located with reference to the accompanying diagram (Fig. 2). This diagram represents the right hemisphere. The letters uf, df, ub, db, rf, lf, rb, and lb stand for up front, down front, up back, down back, right front, left front, right back, and left back, respectively. To describe the position of a point, we mention first its plane and then its location in that plane, as for example, 30° u, 15° rf; or 15° d, 45° rb, etc.

The stimulus was furnished by an electric fork of 100 vibrations, in a distant room, kept vibrating by a current completing its circuit through the receivers of the perimeter. The sound thus heard from the receivers was favorable for localization and of sufficient strength to be heard 12–15 meters away.

The method followed is an abbreviated form of the method of right and wrong cases or 'Konstanzmethode' as Müller[1] prefers to call it. This method was preferred to the method of minimal change mainly because it gives more accurate and precise judgments. The stimuli were the same but were given in different positions with intervening time intervals which kept the mind more alert for minute distinctions than where a continuous stimulus moved along slowly until the observer called halt. In a few preliminary trials the failure of the method of minimal

[1] *Die Gesichtspunkte und die Tatsachen der Psychophysichen Methodik.*

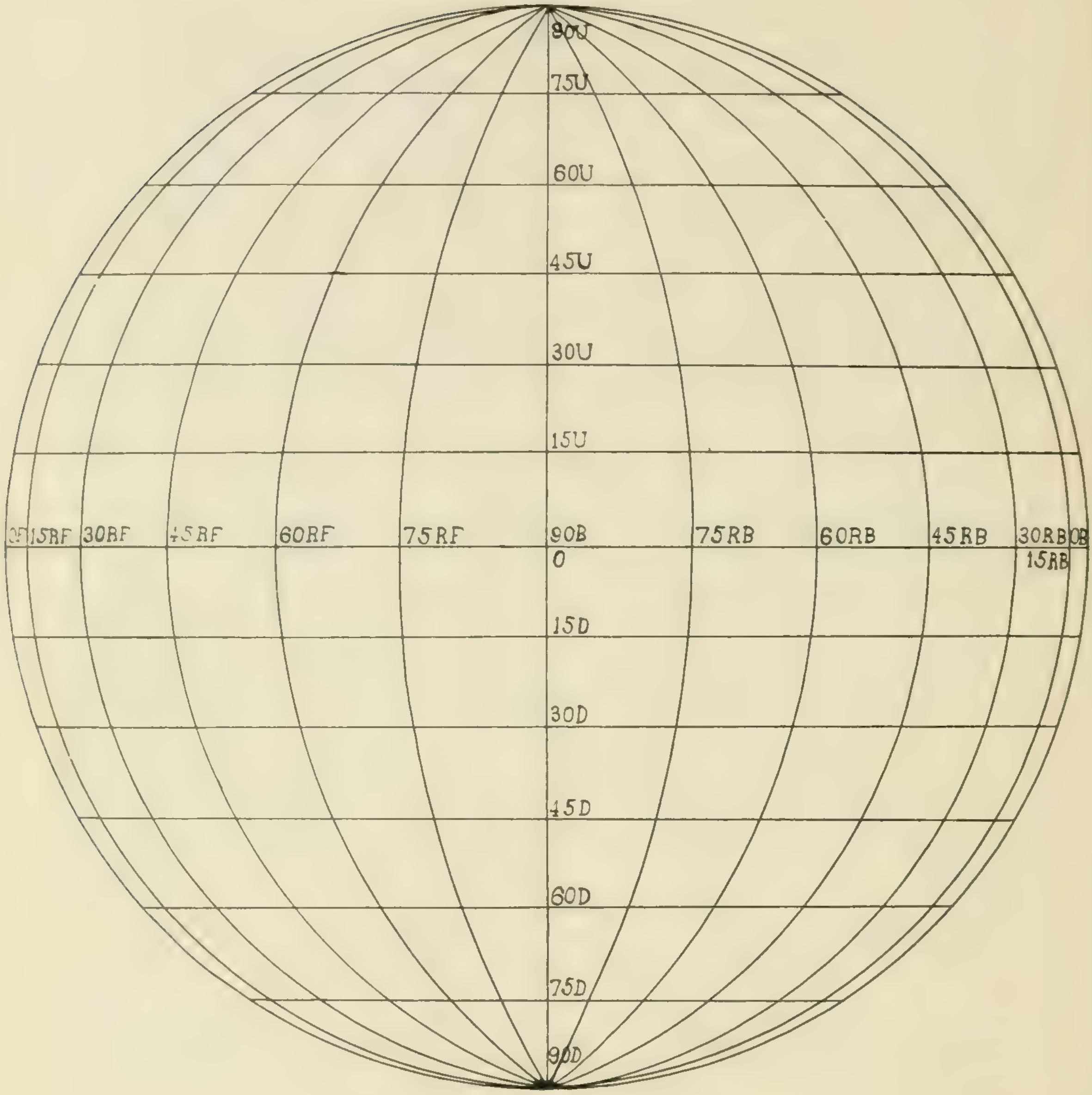


FIG. 2.

change to hold the attention of the observer and to suggest directly the contrast of different directions became evident.

The particular type of the Konstanzmethode that was adopted is the one which allows the observer to have choice between two alternatives only. The equality or doubtful judgment was not permitted. Elimination of that judgment greatly simplifies the computation and final evaluation of the results, and is an incitement to keep the observer aggressive. In the words of Professor Jastrow[1] the equality judgment ' encourages fatigue (weariness) and diminishes the regularity and simplicity of the

[1] 'Critique of Psychophysic Methods,' *Am. Journ. Psych.*, I., p. 283.

judging process.' On the same point Whipple[1] says that the equality judgment induces the tendency 'to pronounce two impressions alike when the difference between them is not clearly made out.'

To illustrate the actual procedure of making the tests, let us suppose that we wish to take the record of the standard 0°f, horizontal plane through the aural axis. The observer is seated on the stool having his eyes closed and his head at the center of the sphere. With the receiver at the standard, 0°f, the experimenter presses the key twice in rapid succession giving two short sounds, each of which has a duration of about one fifth of a second, and the interval is also one fifth of a second. With the other hand he quickly moves the receiver 5° either right or left and there again gives the same double stimulus. The time interval between the two stimuli is about one second. The observer then gives his judgment by saying that the second stimulus is on one side or the other of the first one. If after all the trials with the interval, 5°, have been made, more than 90 per cent. of the judgments are correct, the next smaller interval, 3°, must be tried; or, if less than 60 per cent. are correct, the next larger interval, 10°, must be tried. If that is not sufficient, the interval may be increased 5° each time until the percentage of correct judgments lies within the limits.

The method was thus abbreviated in order to save time in experimentation. This was thought justifiable by the fact that at the beginning of each row of standards that are in the same plane the smallest interval was always tried first and, if that was not sufficient, large intervals were taken successively. The interval that was found satisfactory at a given standard was the one first employed at the following standard. But if this interval was not adequate there, a larger or smaller step was used as the case demanded. Prejudice that would be involved in the arbitrary selection of an interval for a given standard was thus avoided.

In order to have a uniform basis for evaluating the tests on all the standards, the threshold of discrimination for directions was found by the 'table for determining the probable error

[1] 'Discrimination of Clangs and Tones,' *Am. Journ. Psych.*, XII., p. 412.

from the percentage of right cases and amount of difference.[1] On this basis the required interval for each standard was computed to make the per cent. of correct judgments 75.

Sixteen persons who may be divided into two groups took part as observers.[2] The four regular observers, *W*, *K* (women), *B*, and *S* (men), had considerable training in observing and in conducting psychological experiments. *K* and *B* had become familiar with these experiments through the preliminary tests. *W* is an instructor in the University and *K*, *B*, and *S* are graduate students in psychology. Each one gave fifty judgments at each standard. The additional observers who were employed in some special tests, *Wi*, *C* (women), *H*, *Ch*, *Kl*, *Sc*, *G*, *M*, *We*, *Sc*, *Bu*, and *Ho* (men), are also students and are in a general way familiar with psychological experiments.

The eyes were not blindfolded (except in one case) but merely closed and no head rest was used to guard against possible disturbing effects from this source. The exact position of the observers was checked by sighting across small labels which were fastened on the walls of the room. In order to distribute the effect of practice, fatigue, etc., the tests of each individual were made in the double fatigue order. The test periods were never longer than an hour and were always at the same time of the day for each person. The experiments were made during the academic year 1903–4.

Series I. Horizontal Planes.

The immediate aim of this series was to measure the power of discrimination between directions of sounds in horizontal planes. The points or standards, ninety two in number, are located in the following planes (see Fig. 2): 0°, 15°u, 15°d, 30°u, 30°d, 45°u, 60°u, and 75°u, of which the last two have seven standards each, 30° apart, while the others have thirteen standards each, 15° apart.

Each of the four regular observers, *W*, *K*, *B*, and *S*, gave fifty judgments at each standard, passing through the entire

[1] Fullerton and Cattell, *On the Perception of Small Differences*, p. 16. (Univ. Penn. Phil. Series, No. 2.)

[2] The writer wishes to express his thanks to the observers.

series of points in the double fatigue order, which gives in all over 18,000 separate judgments.

No extensive tests were made on the left side of the median plane for the reason that some tests made on both sides did not indicate any essential differences between the two sides.

The results are not given in statistics but in the form of curves, which seemed preferable. But to illustrate the statistical form of the records the following section is adduced.

HORIZONTAL PLANE 15°u.

Standards.	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r
Intervals.	D3°	D3°	D3°	D3°	D3°	D3°	D3°
	x	I	I	x	I	x	x
	x	I	I	I	I	I	x
	I	x	I	I	I	I	I
	I	I	x	I	I	I	I
	I	I	I	I	I	x	I
	I	I	I	I	I	I	I
	I	I	I	I	x	I	x
	I	x	I	I	x	x	x
	I	I	I	x	x	x	I
	I	I	x	I	x	I	x
	I	I	I	I	I	I	I
	I	I	I	I	x	I	I
	I	I	I	x	x	x	I
	I	I	I	I	I	I	I
	I	I	I	I	I	x	I
	I	I	I	x	x	I	x
	I	I	I	x	I	I	I
	I	I	I	I	I	x	x
	I	x	I	I	x	x	I
	I	I	x	I	I	I	I
	I	I	I	I	I	I	x
	x	I	x	I	x	x	I
	I	I	I	x	I	I	x
	I	I	I	I	I	I	I
	I	I	I	I	I	x	x
	88%	88%	84%	76%	64%	60%	60%
Interval for 75%	1.7°	1.7°	2.0°	2.9°	5.7°	7.9°	7.9°

The broken lines in the charts represent graphically the accuracy of localization in the horizontal planes for each one of the four observers. The continuous line embodies the average of the four. The composite of the averages in which the details disappear has value only in so far as it is a means of comparison of general features. In these figures the radii represent the standard directions, the distances between the arcs represent degrees and the center of the arcs represents the center of the

observer's head. Thus, if the curve passes through the intersection of the third semicircle with the radius 15°rf, it means that a sound must be 3° right or left of 15°rf to be perceived as coming from a point right or left of the standard. In those places where the curves go beyond the area of the charts the intersections are indicated by the angles which the curves make with the extended radii. It must be noted that a degree does not represent the same actual distance in all the planes. Its value is greatest in the horizontal plane through the aural axis, or equatorial plane, and is less for the others, namely:

In horizontal plane			0°		1° = 17.5 mm.
"	"	"	15°u	(or d)	1° = 16.9 "
"	"	"	30°u	"	1° = 15.2 "
"	"	"	45°u	"	1° = 12.4 "
"	"	"	60°u	"	1° = 8.8 "
"	"	"	75°u	"	1° = 4.5 "

Figs. 4–11 have been reduced on this scale so that 1 mm. of the radial distance is equal to 2.6 mm. of actual distance between two receivers, and these eight figures are on the same absolute scale. Fig. 3 is drawn on a much larger scale.

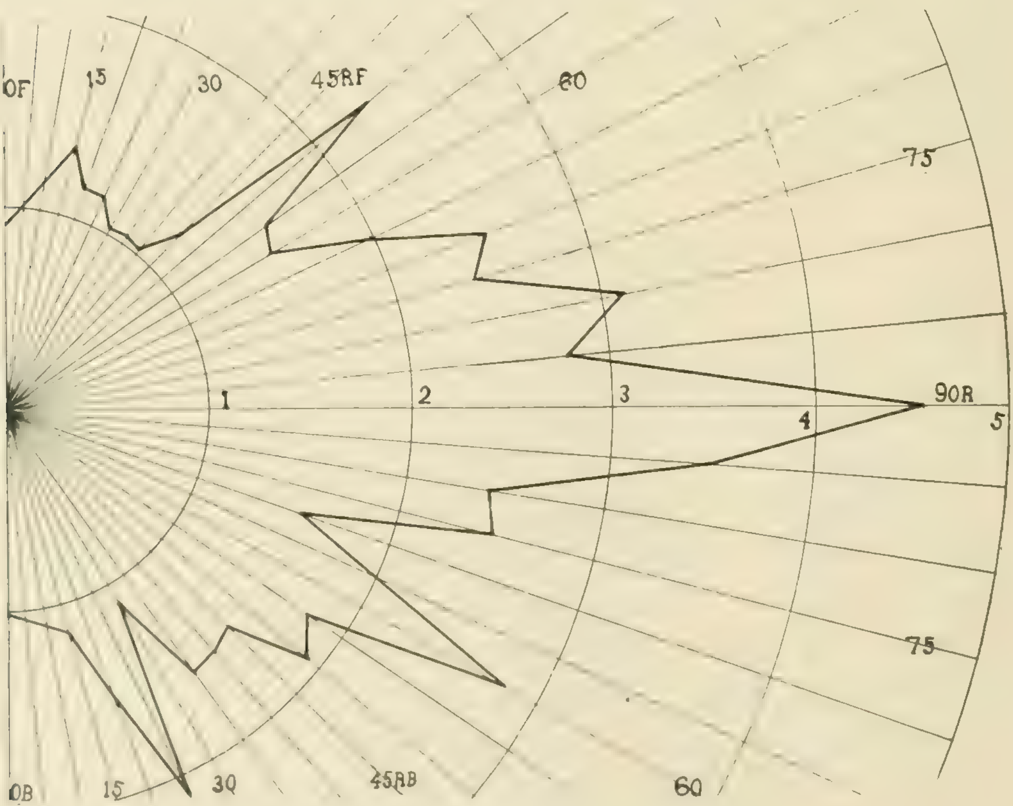


FIG. 3.

After the measurements in the horizontal planes had been completed it was thought desirable to make a more detailed investigation of a single curve. For this purpose the plane through the aural axis was chosen in which the thirty seven

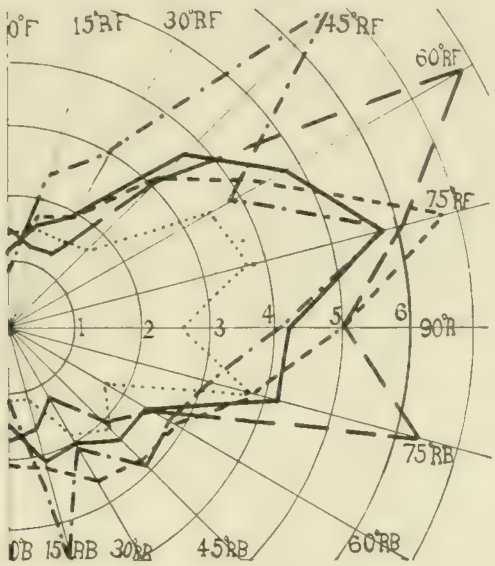


FIG. 4. Horizontal plane through the aural axis.

In Fig's 4–24, the four observers are represented by different forms of lines as follows: W ...., K --------, B – · – · – · , S – – – –, composite ———.

standards tested are only 5° apart. One of the four observers, *S*, was employed and gave 150 judgments at each point, in all 4,550 judgments. The curve based upon these measurements (Fig. 3) is more particularly referred to in the following.

Data in the Curves.

(*a*) In front, the localization is keenest. (*b*) In the back, it is nearly as keen. (*c*) At the side, it is least accurate. Then

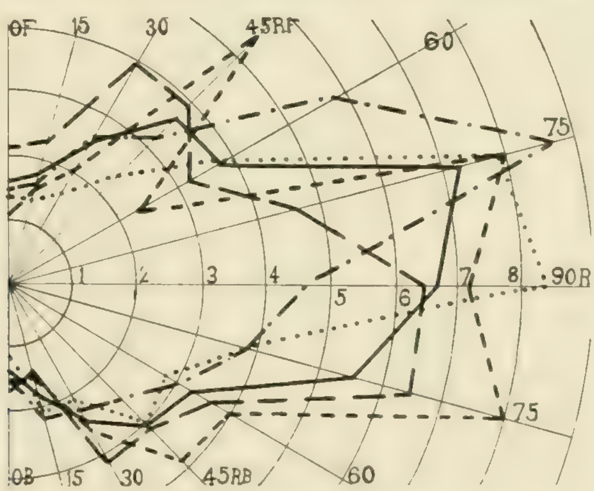


FIG. 5. Horizontal plane 15°u.

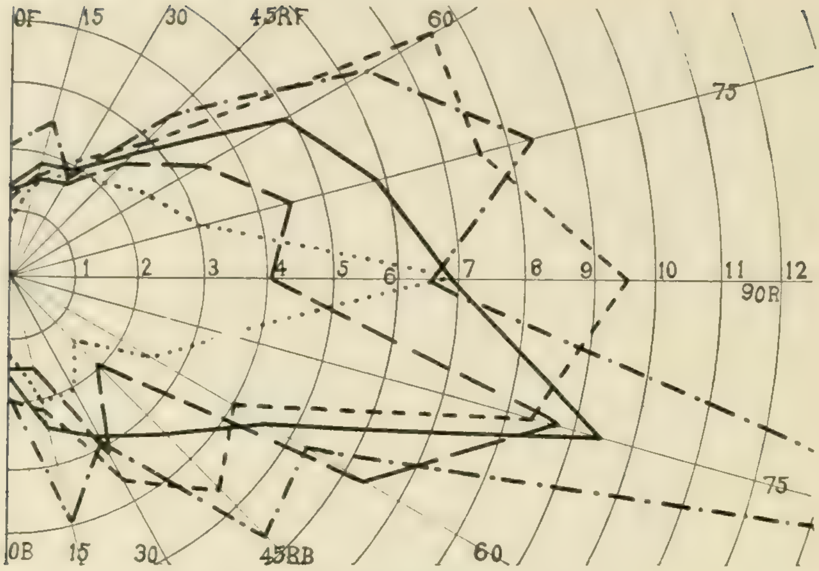


FIG. 6. Horizontal plane 15°d.

there are several prominences in the curves. Besides the large projection at 90°r there seem to be four quite conspicuous prominences (Fig. 3), at 15°rf, 25°rb, 50°rf, and 60°rb, which we shall designate for convenience P1, P1′, P2, and P2′, respectively, and the projection at 90°r we shall designate P3.

Glancing over the other curves we notice a quite general agreement with this one both in regard to the keenness of localization and in regard to the prominences. In Fig. 6, curve *B* and in Fig. 7, curve *K* are specially prominent illustrations of P1. In Fig. 4, curves *W* and *B* illustrate P1′. In Fig. 4, curves *B*, *S*, and *W* illustrate P2 and curves *W* and *S* illustrate P2′.

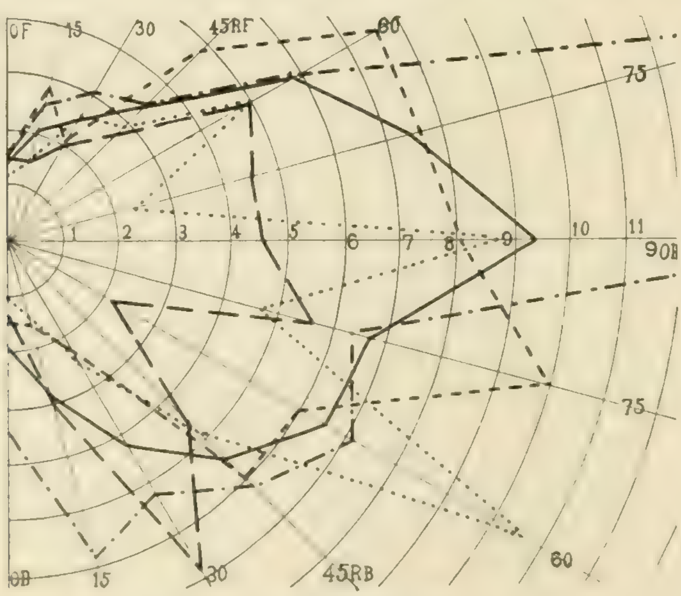


FIG. 7. Horizontal plane 30°u.

Discussion: Introspective and Theoretical.

Disturbing Elements. — It is necessary here to point to some items in the experiments which are disturbing elements, in the

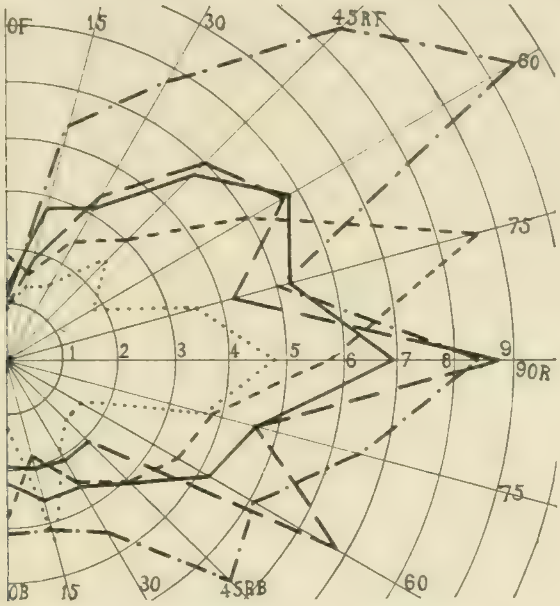


FIG. 8. Horizontal plane 30°d.

sense that they complicate the results and perhaps distort the real characteristics of the curves by obliterating or unduly accentuating the particular features. One element of this nature

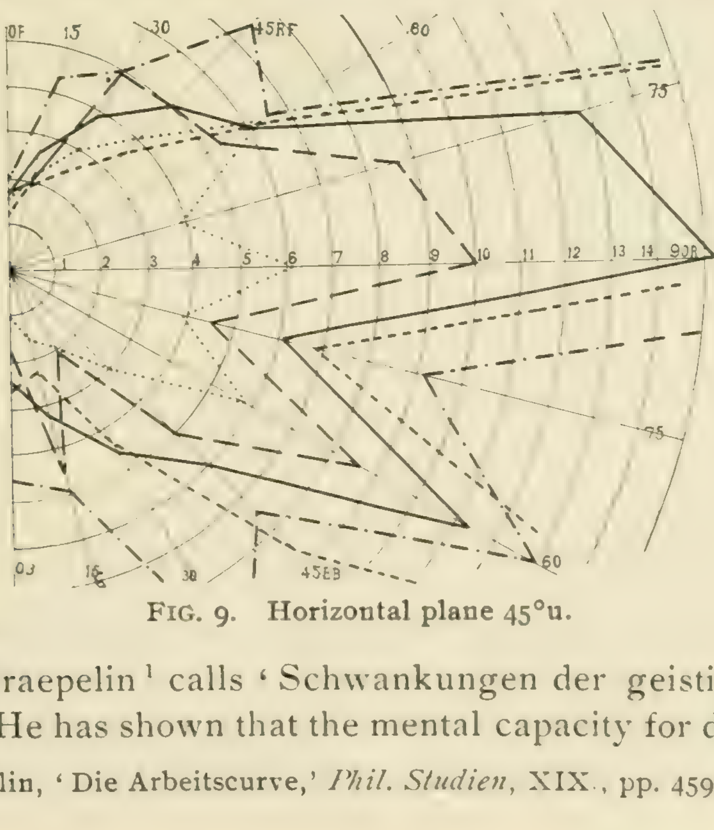


FIG. 9. Horizontal plane 45°u.

is what Kraepelin[1] calls 'Schwankungen der geistigen Leistungen.' He has shown that the mental capacity for doing work

[1] Kraepelin, 'Die Arbeitscurve,' *Phil. Studien*, XIX., pp. 459–507.

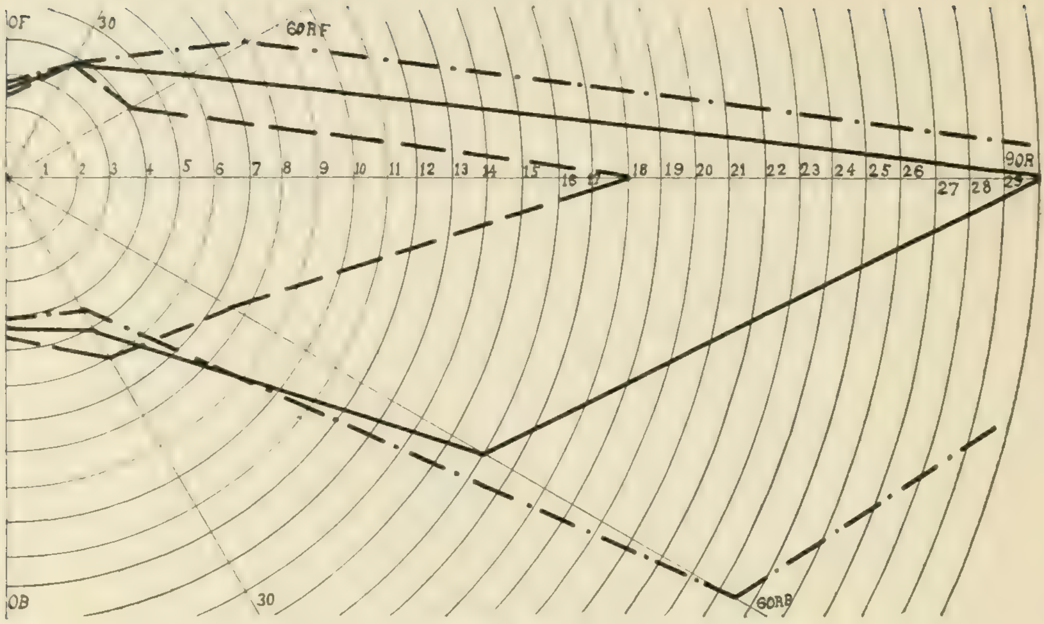


FIG. 10. Horizontal plane 60°u.

does not remain constant for any period of time. It fluctuates continually and the work accomplished varies accordingly. In addition to the variables which Kraepelin recognizes, there are constant tendencies to periodicity in continuous mental work, as has been demonstrated by Seashore and Kent. (See following article.)

All these fluctuations must be taken into consideration in

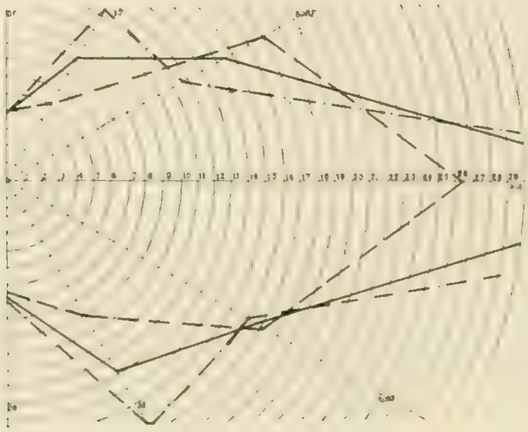


FIG. 11. Horizontal plane 75°u.

continuous mental work. The accuracy of discrimination varied with the wave of mental efficiency, other things being equal. That the mental conditions of the observers actually varied from standard to standard or from one group of standards to another

is probably also implied in the fact that, after all the trials at a standard had been made, the observers repeatedly remarked that they thought the localization to be rather poor at that point because of their lack of attention, or, that the localization seemed easier on account of their being able to pay attention better. A deflection in the curve that would otherwise occur may at one time be lessened and at another time be accentuated by the changes in mental capacity according to the counteraction or the coöperation of the two.

The individuality of each observer must necessarily appear in the results. This has the tendency to hide the common, universal characteristics. Although the double fatigue order has on the whole a neutralizing effect for practice and fatigue, nevertheless the fact that different parts of the records were made at different times, and consequently under slightly different conditions is responsible for some accidental features. In fact the records show that occasionally there are quite noticeable differences between the records of the two orders for the same observer.

It was found that in the process of localization the actual comparison was not usually between the standard and the sounds at the side but chiefly between the two sounds on the sides. The images of the sounds continue for some time and the observer unconsciously disregards the standard after a few stimuli have been given, and the attention is directed rather toward the two sounds at the sides. This would tend to make the distance between the sounds on the two sides the threshold of discrimination, instead of the distance between the standard and the sound on a side. However, this scarcely detracts from the validity of the results since the relative accuracy of discrimination in the various directions is the important aim.

Localization in the Median Plane Belt. — The most noticeable feature in the curves is the keenness of localization in front and in the back. This seems to imply unmistakably, that the ability to discriminate directions of sounds depends to a large extent upon the relative intensities received by the two ears. A sound so situated that it may readily reach both ears will make a change in the ratio of the intensities easily perceptible

and its direction, therefore, can be accurately perceived and easily distinguished from a sound coming from a slightly different direction. This is exactly the condition that obtains in the belt along the median plane. The points of and 0°b in each one of the charts are particular cases of this condition. In this conection a conclusion by Matsumato[1] may have some bearing: "No sound on the right or the left side was localized in the median plane. * * * No sound in the median plane was localized on the right or left side of the plane." The same observation was mentioned by Preyer.[2]

That the localization should be more accurate in front than in the back is what might be expected and is in accordance with the law of economy. The pinnæ are so attached that sounds coming from the front are received more easily. Change in intensity as well as any qualitative variations can thus be perceived more favorably. We are also more accustomed to hear sounds in front so that our attention leads us to expect and to visualize more easily in front. Professor Seashore[3] says: 'We hear more sounds from objects that we pay attention to, *i. e.*, face, than from objects that we do not attend to, *i. e.*, those behind us.'

Münsterberg,[4] and later Bloch,[5] made experiments with a similar aim testing one plane, the horizontal plane through the aural axis. In regard to front and back our results agree with the figures of Bloch. On the other hand, Münsterberg's observer localized most accurately in front but less and less accurately toward the back where discrimination was minimal, which he considers in support of his theory of reflex movements.

Localization at the Side. — Certain prominences in the curves are mentioned above. They are seemingly not individual peculiarities since they occur in the curves of all the observers; nor is it probable that they are accidental since in the

[1] Matsumato, 'Researches on Acoustic Space,' *Studies Yale Psych. Laboratory*, 1897, V., p. 5.

[2] Preyer, 'Wahrnehmungen der Schallrichtung mittelst der Bogengänge,' *Archiv. f. d. ges. Physiol.*, 1887, XL., 568.

[3] *Univ. of Iowa Studies in Psych.*, 1900, II., p. 54.

[4] *Beiträge zur experimentellen Psychologie*, Heft 2, 'Raumsinn des Ohres. p. 220.

[5] Bloch, *Das binaurale Hören*, pp. 31, 35.

special curve (Fig. 3) in which the double fatigue order was repeated several times, starting at different points and going over the same ground six times (twenty-five judgments at a time at each standard), they became more conspicuous in each successive order while other irregularities tended to vanish. The question arises, to what are these prominences due? Let us see what the introspections of the observers may suggest.

Introspective. — The following are some illustrations of frequently recurring remarks by the observers.

"At the standards 20°rf, 25°rf, 30°rf, 35°rf, and 40°rf the sounds on the right of the standard seemed slightly farther away. Then at standard 55°rf it seemed difficult to distinguish. At standards 65°rf and 70°rf the sounds toward the back seemed decidedly nearer. At 75°rf and 80°rf there was again no very definite means of localization. At 85°rb, 80°rb, 75°rb, and 65°rb the sounds toward the front seemed stronger and nearer. At 60°rb it again seemed difficult to distinguish the directions."

"It seemed especially difficult to localize at standard 25°rb. There was apparently no means of discrimination."

Standard 45°rf. "There is a difference in quality. Those toward the back have lower partials and are richer while those toward the front are thinner."

Standard 75°rf. "Those toward the back seem nearer and those toward the front seem farther away and thinner."

Standard 75°rb. "Those toward the back seem thinner."

Standard 60°rb. "The sounds toward the back are lower."

Standards 60°rb, 45°rb, and 30°rb. "In nearly all trials the sounds back of the standards seemed thinner as if heard 'around the corner of a building' in comparison with those in front of the standards."

These quotations from the records serve to make some, though not very definite, suggestions. (*a*) There seem to be certain directions where the localization is more difficult than in others, apparently because there is no means of discrimination. (*b*) There are relative differences in intensity for different directions. (*c*) Although the sounds were objectively at the same distances for all directions the observers felt quite sure that they did not seem to be at the same distances. (*d*) Then, variations in pitch, richness of sound, etc., are mentioned.

In order to determine more precisely the significance of these data and, if possible, to find new elements, a special set of tests was made upon four other observers *Wi*, *H*, *Ch*, and *Kl*, who were entirely ignorant of the results thus far obtained. Three typical standards were chosen namely, 45°rf, 90°r, and 45°rb, in each one of three horizontal planes, 30°d, 0°, and 30°u.

The apparatus was the same as in the regular experiments but the method was slightly different. The standard was given

Table I.

Standard 45°rf in Each of the Three Planes.

Observer.	Planes.	Forward. Fainter.	Forward. Further.	Forward. Less Clear.	Forward. Pitch. h.	Forward. Pitch. l.	Forward. Richer.	Forward. Misplacement.	Backward. Louder.	Backward. Nearer.	Backward. Clearer.	Backward. Pitch. h.	Backward. Pitch. l.	Backward. Thinner.	Backward. Misplacement.
	30°u	4(1)		2		2			1(4)	(1)			3		
Wi	0°	(2)	2			1			4	1	1	2	1		1u
	30°d	2(1)	1	(2)		3			2(1)		1	1	2		
	30°u	(4)			1								4		2d
H	0°				5				2		1		2		
	30°d	(3)			1			2u			1		4		
	30° u		1(2)		3			2u 1d		1(5)			2		1u
Ch	0°		1(2)		2				1				4		
	30°d	(1)	1		4			3u	3				4		1u
	30°u	(1)	1					3u	(3)				2		3d
Kl	0°	4						2u	5						1d
	30°d	(1)	(4)					2u	4	3					1d
		10(14)	7(8)	2(2)	16	6			22(8)	5(6)	4	3	28		

The numbers in parenthesis indicate the judgments that were contrary to the column under which they are given.

Table II.

Standard 45°rb in Each of the Three Planes.

Observer.	Planes.	Forward. Louder.	Forward. Nearer.	Forward. Clearer.	Forward. Pitch. h.	Forward. Pitch. l.	Forward. Richer.	Forward. Misplacement.	Backward. Fainter.	Backward. Further.	Backward. Less Clear.	Backward. Pitch. h.	Backward. Pitch. l.	Backward. Thinner.	Backward. Misplacement.
	30°u	4		3	1	1			4(1)				2		
Wi	0°	4	3		2			1u	2(1)	2(1			4		
	30°d	5(1)		1	1	1	1		3(1)						
	30°u	3			2								4		1d
H	0°	3		3	2		1				(2)		3		
	30°d	2		2	3		1						3		3d
	30°u	2						3u 1d					3		5d
Ch	0°	3	1		1	2			(1)	2		1(?)	2		
	30°d	3		1		1			(1)	1(1)		1			2d 1u
	30°u	4		1				5u	(4)		1(1)		2		1d
Kl	0°	5		1				3u	(2)(?)						3d
	30°d	3	5	1				2u		5					
		41(1)	9	13	12	5	3		9(11)	10(2)	1(3)	2	23		

and then a sound on one of the two sides so far from the standard that the observer could easily tell its direction, which was in each case determined by a few preliminary trials. Each observer gave ten judgments at each standard. Before the tests were begun, the observer was told to describe as accurately and carefully as he could the differences that he noticed between the sounds from the directions under comparison at any given trial. The results thus obtained are given in the tables.

TABLE III.

STANDARD 90°R IN EACH OF THE THREE PLANES.

Observer.	Planes.	Forward.										Backward.									
		Intensity.		Distance.		Clearness.		Pitch.		Richness.	Misplacement.	Intensity.		Distance.		Clearness.		Pitch.		Richness.	Misplacement.
		m.	l.	m.	l.	m.	l.	h.	l.	m.l.		m.	l.	m.	l.	m.	l.	h.	l.	m. l.	
	30°u	1	4			1							5								2u
Wi	0°	3	1		1			3		1	2u	4	1						4		1u
	30°d	5			1	1			2				5	1			1		5		
	30°u	3				1		2			1u	1				1			3		
H	0°	2				1		1				1							4		
	30°d	3				1		2											3		2d
	30°u								2		3u 2d	1							1		3d 1u
Ch	0°	2			2			1				2			2				3		
	30°d	2			1			2			1u	1					1		5		
	30°u	1			2	1					4d			1			1				4u 1d
Kl	0°	2	3	3								4									3d
	30°d	2			3										1				1		4d
		26	8	3	10	6		11	4	1		14	11	2	3	1	3		29		

(*a*) Intensity is the most prominent feature. In Table I. we notice that under the rubric *backward* twenty-two are said to be louder and eight fainter. In Table II. under *forward*, forty-one are louder and one fainter. In Table I. (standard 45°rf), *backward* means nearer to the aural axis and, in Table II. (standard 45°rb), *forward* means nearer to the aural axis. This indicates that for these particular positions sounds nearer the aural axis seem more intense. On the other hand, in Table I. under *forward*, *i. e.*, farther from the aural axis, ten are fainter and fourteen louder; and in Table II., under *backward*, *i. e.*, farther from the aural axis, nine are fainter and eleven louder. There is no definite inclination for the converse of the above statement, that sounds farther from the aural axis seem less

intense. But the implication probably is that the statement, the nearer a sound is to the aural axis the louder it seems, is true only for the immediate vicinity of the aural axis, and that if we go beyond certain limits the opposite may possibly be true as one of the quoted introspections indicates. In Table III. (standard 90°r), under *forward*, twenty-six are louder and eight fainter. The tendency seems to be to designate those *forward* as louder. Hence the point where a sound seems loudest, *i. e.*, the subjective aural axis, is probably farther forward than the objective aural axis.

(*b*) Distance is frequently mentioned. On the ground that change in intensity signifies change in distance, the figures under this head in the tables would have the same meaning as the figures under intensity. Sounds nearer the aural axis seem nearer to the head than those farther away from the axis.

(*c*) Clearness and richness are also spoken of, and here as for intensity and distance sounds nearer the aural axis are clearer and richer.

(*d*) In each table it may be noticed that the sounds *forward* are said to be higher in pitch and those *backward* are said to be lower. What this item means is not entirely clear. It may be that under it other elements are included which the observers could not very well designate by any other name, or the word forward may suggest higher and the word backward lower pitch.

(*e*) There are two kinds of misplacements, upward and downward, most of which become intelligible when we reduce them to terms of distance or intensity. For example, if, at the standard in the front upper quadrant, the sound *forward* is said to be up, or the one *backward* is said to be *downward*, it probably means that it seems farther or nearer respectively. But knowledge of the positions of the sounds tells that it is neither farther nor nearer in a radial direction, therefore it is shifted up or down. Most of the misplacements are probably accounted for upon this basis. However some may be due to other causes such as expectation and other differences in subjective and objective conditions.

Since it has been shown that distance (or intensity) is a potent factor in localization, another brief set of experiments

was planned with the view to specify it more definitely. Again the same apparatus was used. Five observers, M, We, Se, Bu, and Ho, who were entirely naïve in regard to the structure of the apparatus and the method of experimentation and its purpose were engaged. Before the observer was brought to the apparatus he was blindfolded to avoid suggestions that might be gained from the structure of the apparatus. The observer was seated in the regular position within the perimeter; a sound was given in front and then the receiver was moved to one of the regular standards (horizontal plane through the aural axis) in the right front quadrant and there another sound was given. The observer then, not knowing that the sounds were actually at the same distances, estimated in inches the comparative distances of the two sounds. It was not prohibited to judge them to be at the same distance. This was repeated five times for each standard by each observer.

In a similar manner tests were made in the rear quadrant where the comparison, however, was with the sound directly back instead of in front. The essential aim was to make a distance comparison of all directions with one direction.

Table IV.

	0°f In.	15°rf In.	0°f In.	30°rf In.	0°f In.	45°rf In.	0°f In.	60°rf In.	0°f In.	75°rf In.	0°f In.	90°r In.
Bu	37	42	38	39	35	43	36	40	36	38	36	40
Se	28	31	26	27	28	26	25	26	22	20	27	20
Ho	26	23	22	22	24	24	35	26	27	17	38	28
We	23	32	22	33	24	29	22	28	23	33	24	33
M	32	33	37	39	39	38	36	37	35	30	35	29

	0°b In.	75°rb In.	0°b In.	60°rb In.	0°b In.	45°rb In.	0°b In.	30°rb In.	0°b In.	15°rb In.
Bu	53	48	48	45	48	48	44	46	43	41
Se	27	24	26	25	28	26	23	25	26	29
Ho	32	26	42	30	36	32	29	31	30	32
We	24	32	22	37	24	31	22	30	23	32
M	38	35	39	36	37	41	34	38	36	38

	0°f In	15°rf In.	30°rf In.	45°rf In.	60°rf In	75°rf In.	90°r In.	75°rb In.	60°r b In.	45°rb In.	30°rb In.	15°rb In.	0°b In
Bu	40	45.4	41.1	49.1	44.4	42.2	44.4	36.3	37.5	40.0	41.8	38.1	40
Se	40	44.3	41.5	37.1	41.6	36.3	29.6	35.5	38.4	37.1	43.4	44.6	40
Ho	40	35.4	40.4	40.0	29.7	25.2	29.5	32.5	28.6	35.5	42.8	42.7	40
We	40	55.6	60.0	48.3	50.9	57.4	55.0	53.3	67.3	51.7	54.5	55.6	40
M	40	41.3	42.1	39.0	41.1	34.3	33.1	36.8	36.8	44.3	44.7	42.2	40
Av.	40	44.4	44.7	42.7	41.5	39.1	38.3	38.9	41.7	41.7	45.4	44.6	40

The results are shown in Table IV. The upper portion of the table gives the averages of the estimates of each observer. In the lower section the ratios of the pairs of comparison are reduced to the basis of 40 inches, the actual distance of the sounds. For example the first observer's estimate 0°f 37 and 15°rf 42; then, 37 : 42 : : 40 : 45.4, etc.

These figures are represented graphically by Fig. 12.

(*a*) Sounds are judged nearer in three places, in front, in the back, and at the side.

(*b*) Between these regions, about the middle of each quadrant, they seem farthest away.

Theoretical.—It remains to establish a connection, if possible, between the introspections, the results of the special tests, and the prominences in the curves.

Since in some directions sounds seem nearer and stronger than in others, and since there seem to be differences in quality for different directions; in short, since there are variations in the data upon which the localization depends, it seems reasonable to infer that there must be corresponding changes in the process of discrimination of directions. It has been stated that in front localization depends upon the relative intensities received by the two ears and that here a sound seems nearer than an equidistant sound farther toward the right. If a sounding receiver be moved from 0°f toward the right it apparently recedes at the same time, seeming to be farthest away about the middle of the quadrant.

This leads to the assumption that there is a change in the basis of localization from the condition in which sounds compared at any one time are seemingly at the same distance and of the same intensity to the condition in which the sound at the right of the standard appears farther away.

Now if the receiver be moved still farther to the right it will again seem to approach and become more intense, reaching its greatest intensity at or near the aural axis. A sound farther toward the right would now be stronger and nearer than one not so far toward the right, which would effect another change in the method of localization. Besides, there is here also the addition of qualitative elements. Sounds nearer to the aural

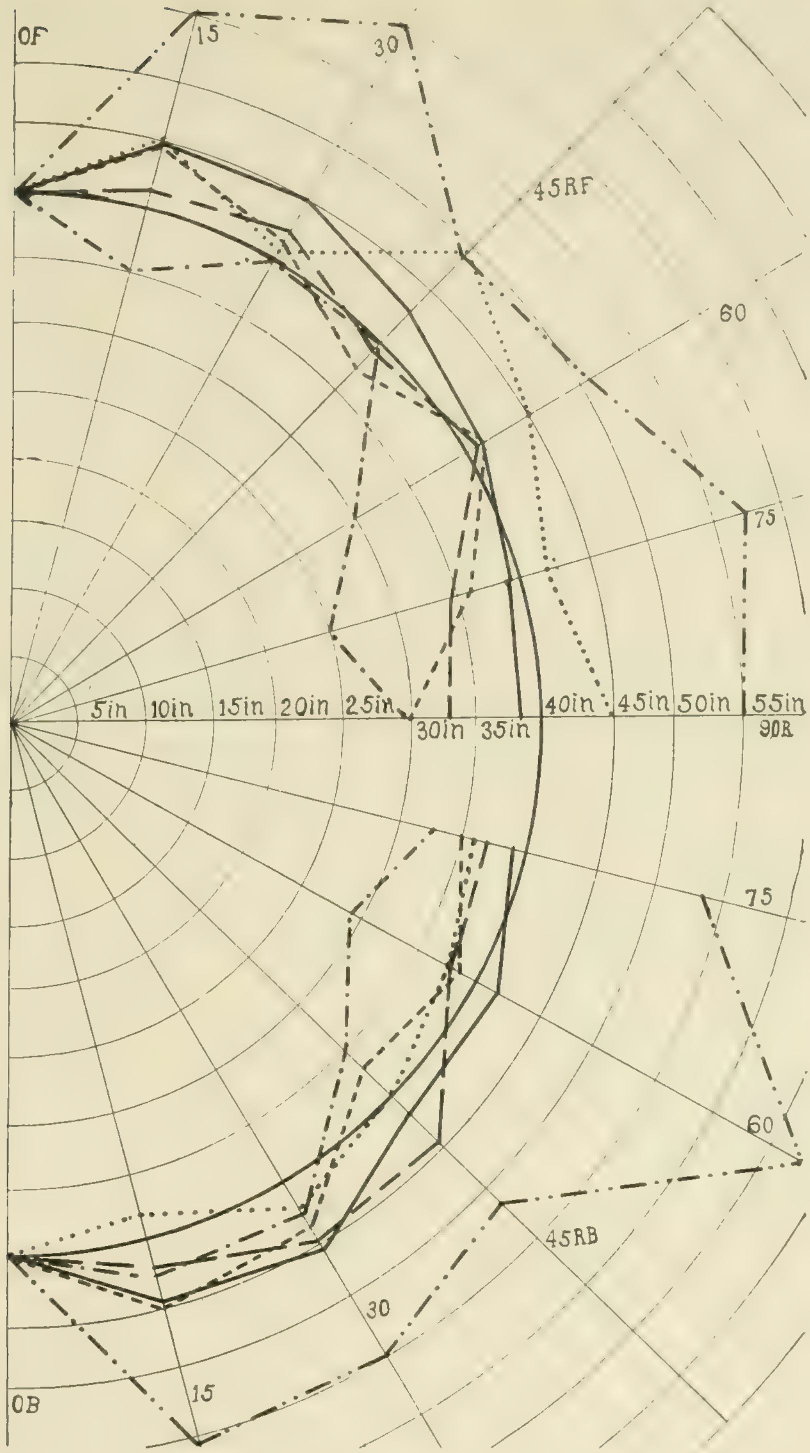


FIG. 12.

Bu · · · ·, Se - - - - -, Ho – · – · – ·, We – · · – · · –, M – – – –, composite ———.

axis seem stronger, nearer, clearer, and richer than those not so near to it. That is, the localization in this region ceases to

be binaural and becomes monaural.[1] The localization of sounds coming from the front or near the front depends, in individuals having both ears normal, upon the coördinating function of the two ears. But sounds here at the side are not distinguished from one another by this combined action alone. The left ear drops out of service and the right ear becomes the chief means of perceiving the sounds. Those from the right side do not, in the nature of the case, reach the left ear as directly as from points in or near the median plane. It cannot be otherwise than that a sound on the side is stronger to the ear on that side. Frequently the observers remarked in passing from one standard to another in the locality of this transition, as for example at 45°rf or 60°rf, that there the sounds were very strongly perceptible to the right ear. The more favorably an ear can receive a sound the more responsible that ear is for a correct localization. Qualitative and quantitative features are among the determining elements or local signs of direction.

The modifying effect of the pinna and side of the head become influential. The function of the former is twofold, positive and negative: positive in aiding the passage of sound waves to the meatus, negative in obstructing the course of sound waves. The more favorably a sound is situated to reach the meatus the more forcible, complete and full will it be. This fact was well illustrated by the pseudophone of S. P. Thompson.[2] His conclusion is that the 'intensity of a perceived sound depends upon the amount of space over which the waves are gathered by the external collecting apparatus of the ear; and by analogy with optical phenomena we may say it depends upon the number of rays of sound which reach the ear.'

As we pass the aural axis the sounds farther forward, instead of backward as before, seem louder and more distinct. Hence there is a third transition point at the subjective aural axis. In the rear quadrant sounds seem most distant about the middle of the quadrant and, corresponding to the two transition points in the front quadrant, there are two in the rear quadrant since the conditions of localization are practically the same at ob as at of.

[1] The terms binaural and monaural are not used in their ordinary absolute meaning.

[2] Thompson, 'The Pseudophone.' *Phil. Mag.*, VIII., 1879, pp. 385-390.

In all there are five transition points (T). The first T1, lies between 0°f and 45°rf, the second, T1′, is located symmetrically in the rear quadrant, the third, T2, and the fourth, T2′, are near or just beyond the middle of the quadrants, and the fifth, T3, is at the aural axis. T1 is essentially a change in binaural localization 'from the condition in which sounds compared are seemingly at the same distance and of the same intensity to the condition in which the sound at the right of the standard appears more distant.' T2 is essentially a transition from binaural to monaural localization. T3 is a change in monaural localization, *i. e.*, a reversal of the monaural process. More will be said below in regard to this transition point. T2′ and T1′ are reversals of T2 and T1 respectively.

The positions of the transition points correspond to the positions of the prominences in the curves. T1 corresponds to P1, T1′ to P1′, T2 to P2, T2′ to P2′, and T3 to P3. Now, the relation between the two is in the fact that change in the data and elementary processes involved in localization, *i.e.*, change in the basis of discrimination must cause more or less confusion on the part of the observer. In several of the introspections the observation is made that at some standards it was especially difficult to localize. It may be noticed that these directions correspond to the positions of the prominences. That these transitions resulted in confusion is further supported by the introspective remark of the observers that a definite distinction could be noticed between the standard and the sound compared with it but that the observer could not tell with certainty on which side of the standard the second sound was. Consequently more errors were made, and hence the prominences in the curves. But in a large number of trials the observer soon learned to interpret these distinctions correctly, in part at least. In other words he became adapted to the modified method. That such adaptation actually occurred is quite apparent from the distribution of the errors in the records. In 75 per cent. of all such transitions the records show that nearly all errors made at a standard were made in the first half of all the trials made with that standard. This clearly indicates that in the second half the observer had learned to interpret more correctly the

distinctions that were already recognized in the first half but not understood.

In regard to P3 it should be mentioned that beside the fact that it is a transition, there is another condition involved. There must be a point in the aural axis or thereabouts where the coördinating function of the two ears reaches its minimum, or where monaural localization reaches its maximum. If the two ears render any assistance to each other in the perception of direction there must be a point where this mutual effect is least, and consequently tends to make discrimination less accurate. Then there is also the presence of confusion points. A telephone moving from front toward back comes to a position in or near the aural axis where it has its greatest monaural intensity. The locus of this point at different distances from the head was above designated as the subjective aural axis. If the monaural intensity is maximal in the subjective aural axis there must be, for each point on one side of the axis, a point on the opposite side of the axis with similar characteristics. That is, a sound near the subjective aural axis permits of more than one interpretation in terms of perception, and hence a system of confusion points.

In recent years, the so-called secondary, qualitative factors have been recognized as important elements of localization. Intensity was held as a very essential factor but recent investigations[1] show that we are not justified in laying so much emphasis upon it. Intensity alone is not sufficient to account for all the features of localization. The fact that monaural localization has been demonstrated indicates a more complicated basis for the perceiving of direction of sounds. Careful observation and introspection point to the presence and importance of qualitative data in the process of localization.

Referring again to the curve of Bloch, we find that it does not indicate any prominences or deflections beyond the general fact of poorer discrimination at the side. The chief reason for this is that the standards which he tested are farther apart,

[1] Angell and Fite, 'Monaural Localization of Sound,' PSYCH. REV., 1901, VIII., pp. 225–246; pp. 449–458.

Angell, 'Significance of Partial Tones,' PSYCH. REV., 1903, X., pp. 1–14. Pierce, *Studies in Space Perception.*

namely 22.5°. In the charts, with standards 15° apart, the prominences do not appear so regularly as in Fig. 3, with standards, 5° apart.

SERIES II. VERTICAL PLANES.

The immediate aim of this series is to determine the power of discrimination between directions of sounds in vertical planes.

The apparatus and its arrangements are exactly the same as in the first series with one exception. It was possible in the first series to procure uniformity in the reflection of sounds by keeping the source of the sound in the same locality and allowing the observer to take different positions for the various standards. This could not be done in the vertical measurements. To overcome this difficulty a screen was devised consisting of

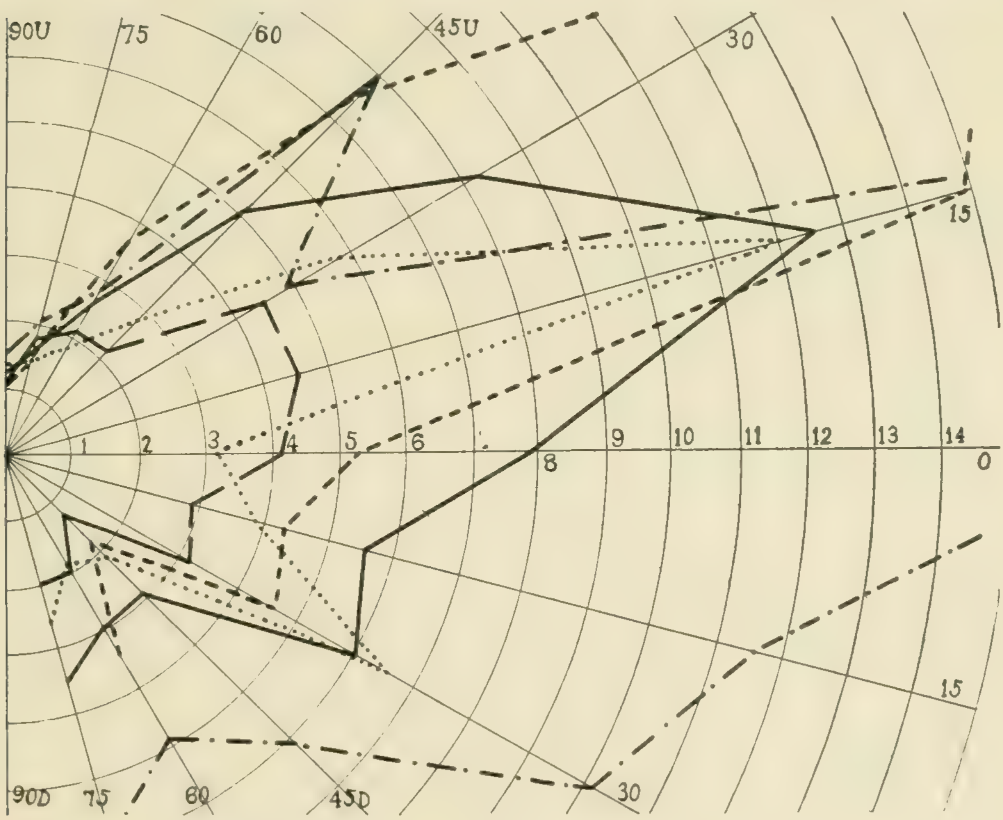


FIG. 13. Vertical plane 90°r.

a frame built of iron pipes over which a canvass was stretched. The screen thus constructed had the shape of a hollow cylinder 3.40 meters in diameter and 2.70 meters in length. It was so situated with reference to the perimeter that the center of the

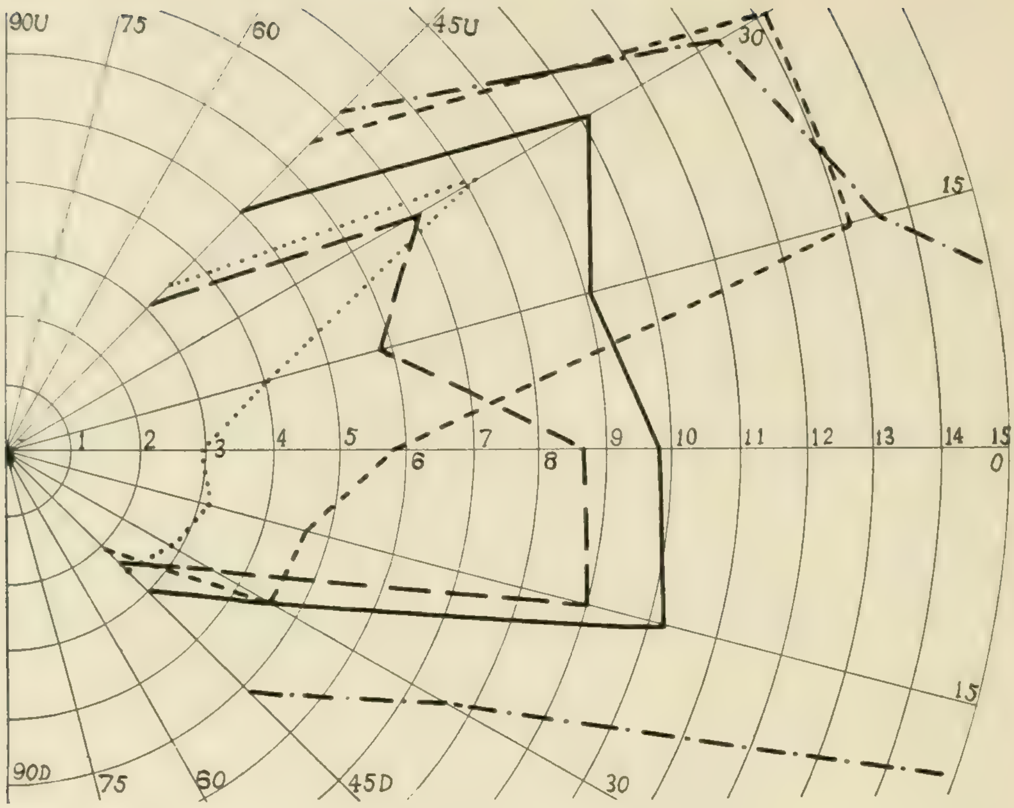


FIG. 14. Vertical plane 75°rf.

sphere described by the arms of the perimeter coincided with the center of the cylindrical screen.

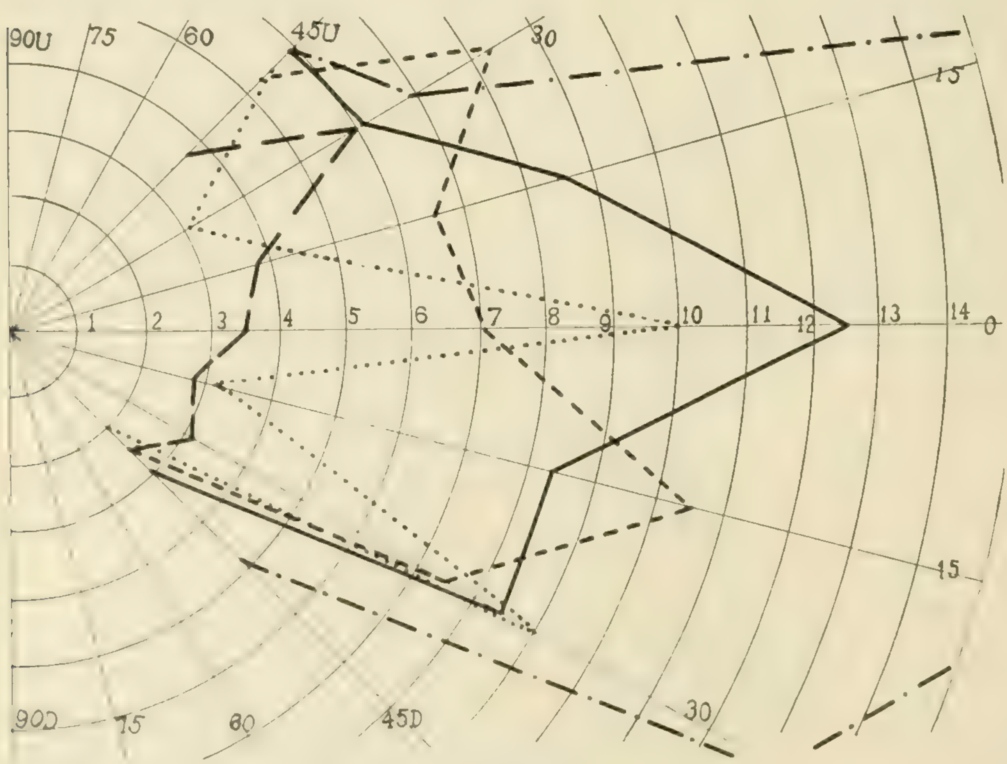


FIG. 15. Vertical plane 75°rb.

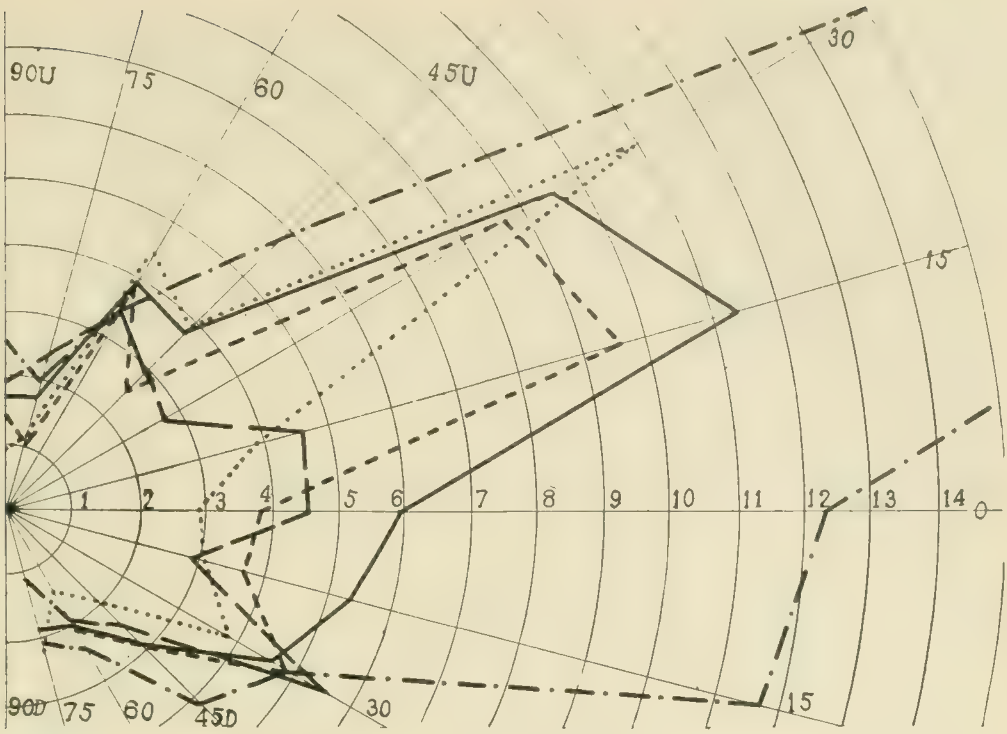


FIG. 16. Vertical plane 60°rf.

A brief but careful test was made of one of the vertical planes to discover whether the screen would be sufficient to give uniform reflection. The observer took a horizontal position on

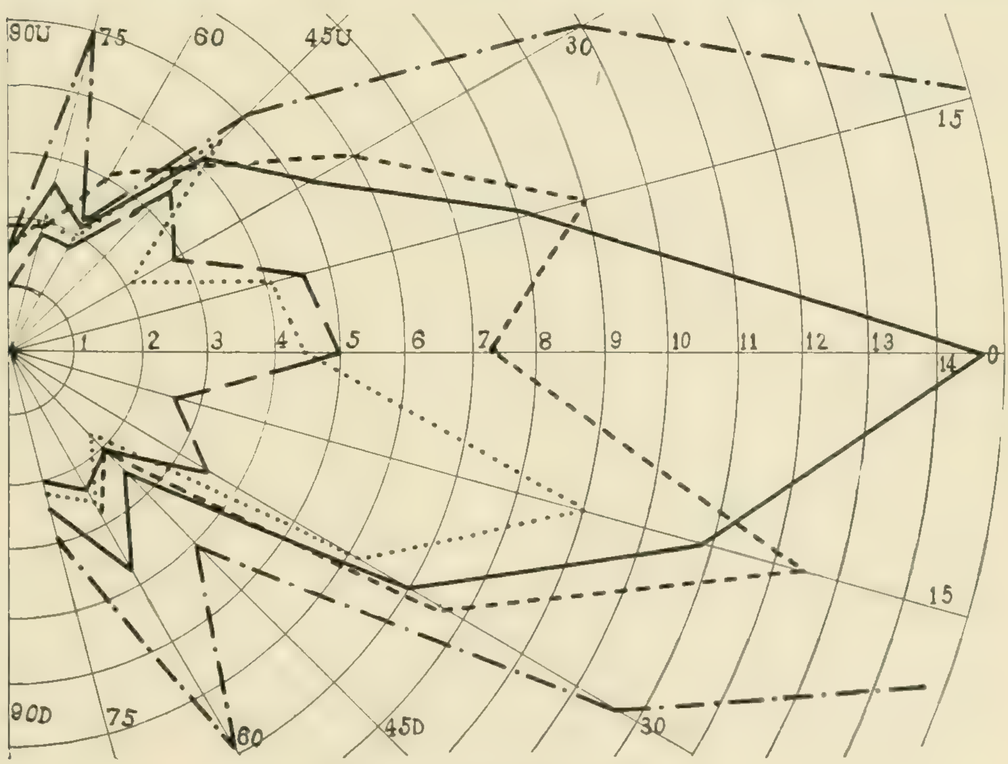


FIG. 17. Vertical plane 60°rb.

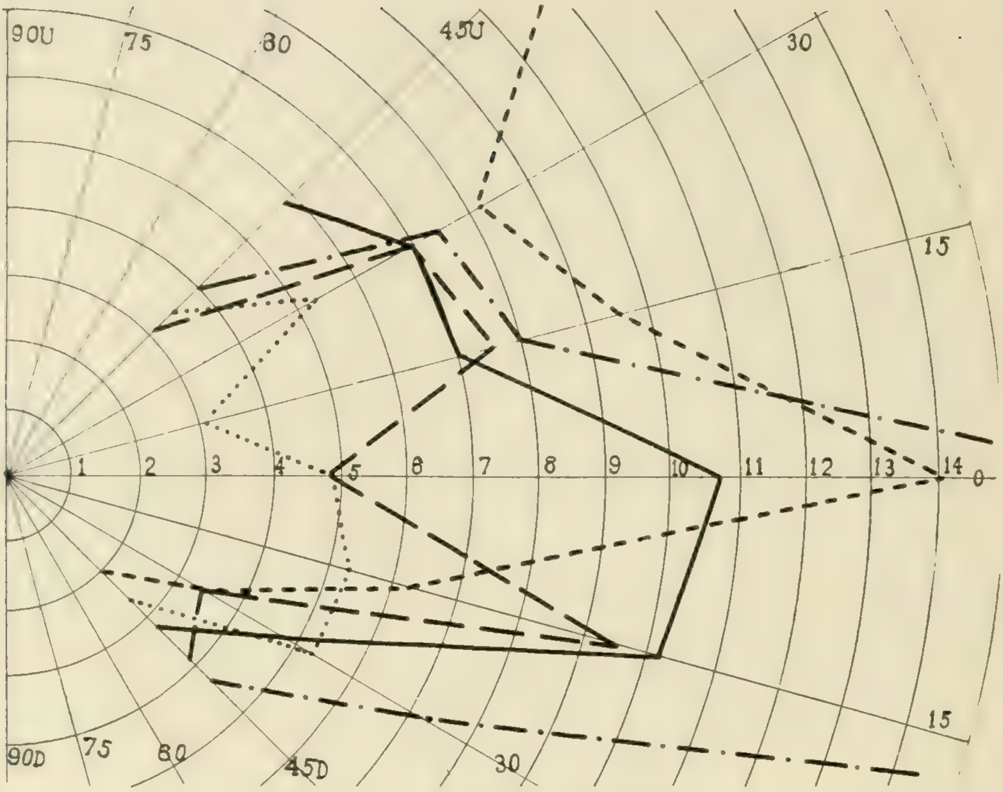


FIG. 18. Vertical plane 45°rf.

a bench constructed with as little material as possible. The stimuli were given in the same manner and locality as in Series I. The results indicated no essential deviation from those

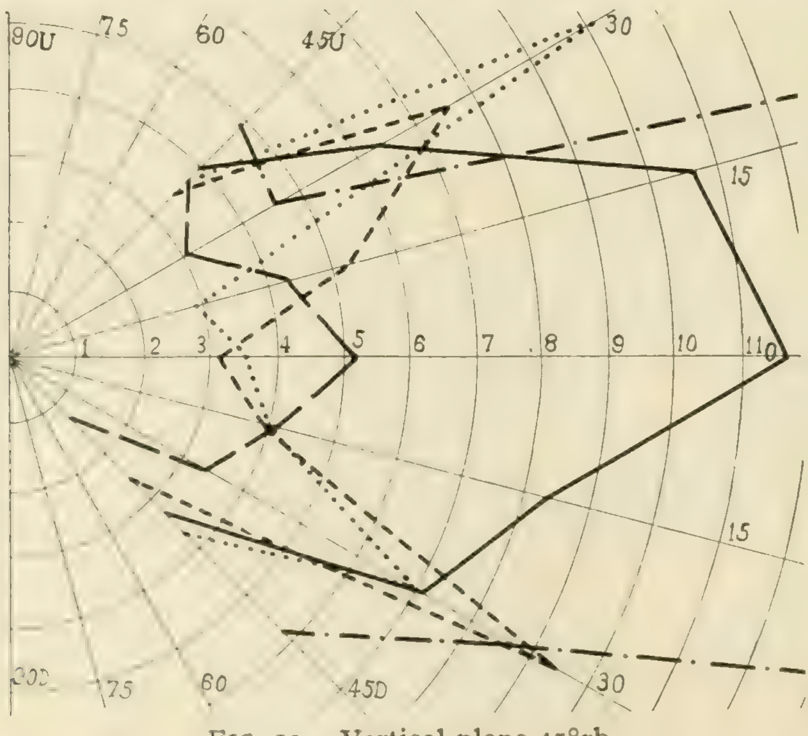


FIG. 19. Vertical plane 45°rb.

obtained when the screen was used and the observers had the upright position.

The standards, one hundred and twenty-five in number, are in the following vertical planes (see Fig. 2): 90°r, 75°rf, 75°rb, 60°rf, 60°rb, 45°rf, 45°rb, 30°rf, 30°rb, 15°rf, 15°rb, and the median plane. The planes 90°r, 60°rf, 60°rb, 30°rf, 30°rb, have twelve standards each, 15° apart. The point 90° down was not tested. The planes 75°rf, 75°rb, 45°rf, 45°rb, 15°rf, and 15°rb, have seven standards each, 15° apart and ranging

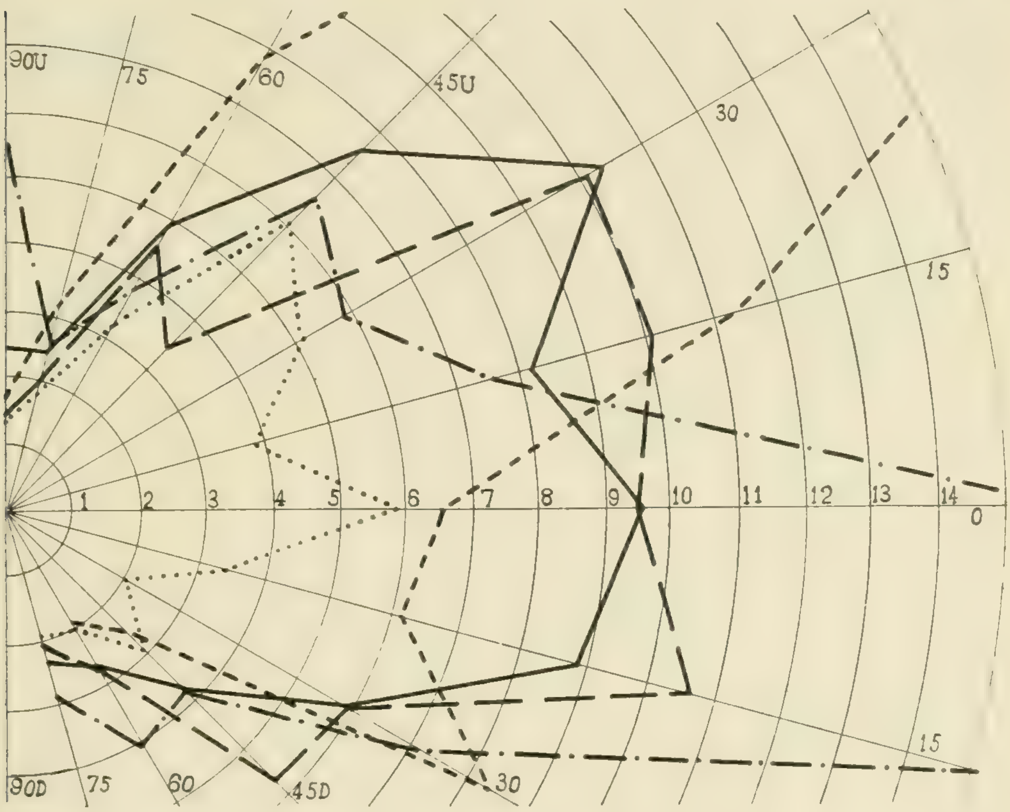


FIG. 20. Vertical plane 30°rf.

from 45°u to 45°d. In the median plane are twenty-three standards.

Each of the four regular observers, *W*, *K*, *B*, and *S*, gave fifty judgments at each standard passing through the entire series of points in the double fatigue order, which gives in all about 25,000 judgments.

The charts are constructed on the same principle as in the foregoing series but here a degree has the same distance-value throughout, namely, 17.5 mm.

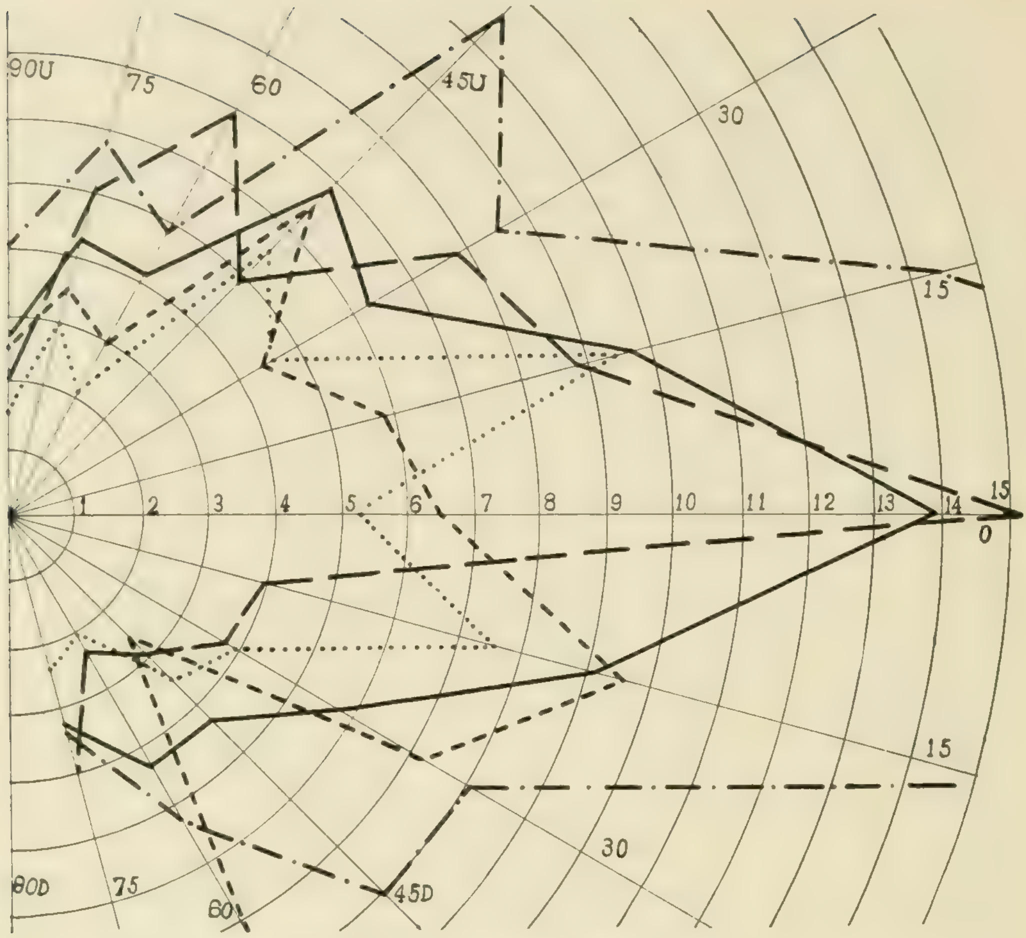


FIG. 21. Vertical plane 30°rb.

A. The Vertical Planes Excluding the Median Plane.

Data in the Curves.

(*a*) Localization is most delicate at the point directly overhead. (*b*) There is an approach toward considerable accuracy in the lower quadrant near the median plane. (*c*) In addition to the fact that localization is less accurate at the side there are several conspicuous prominences situated similarly to those in the curves of the first series. Particularly illustrative of this are Figs. 13, 16, and 17.

Discussion: Introspective and Theoretical.

Localization in the Median Plane Belt. — The keenness in discrimination at the two positions above and below accords with what has been said before in regard to the localization in the belt along the median plane. The two directions are simply particular cases of that general rule. The results also agree

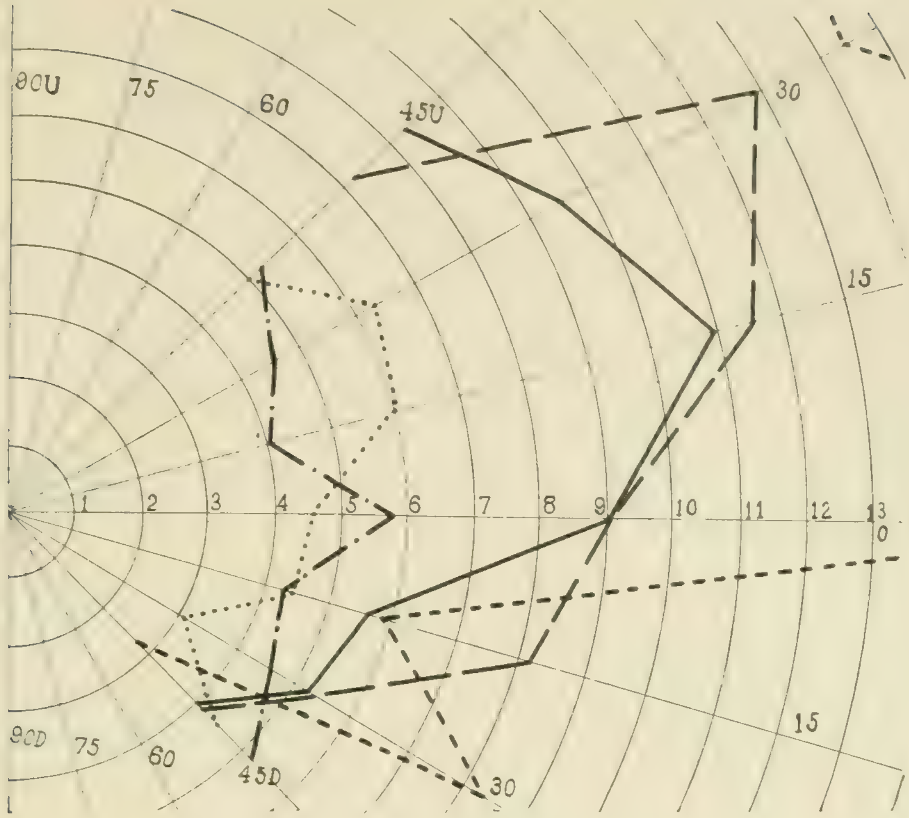


FIG. 22. Vertical plane 15°rf.

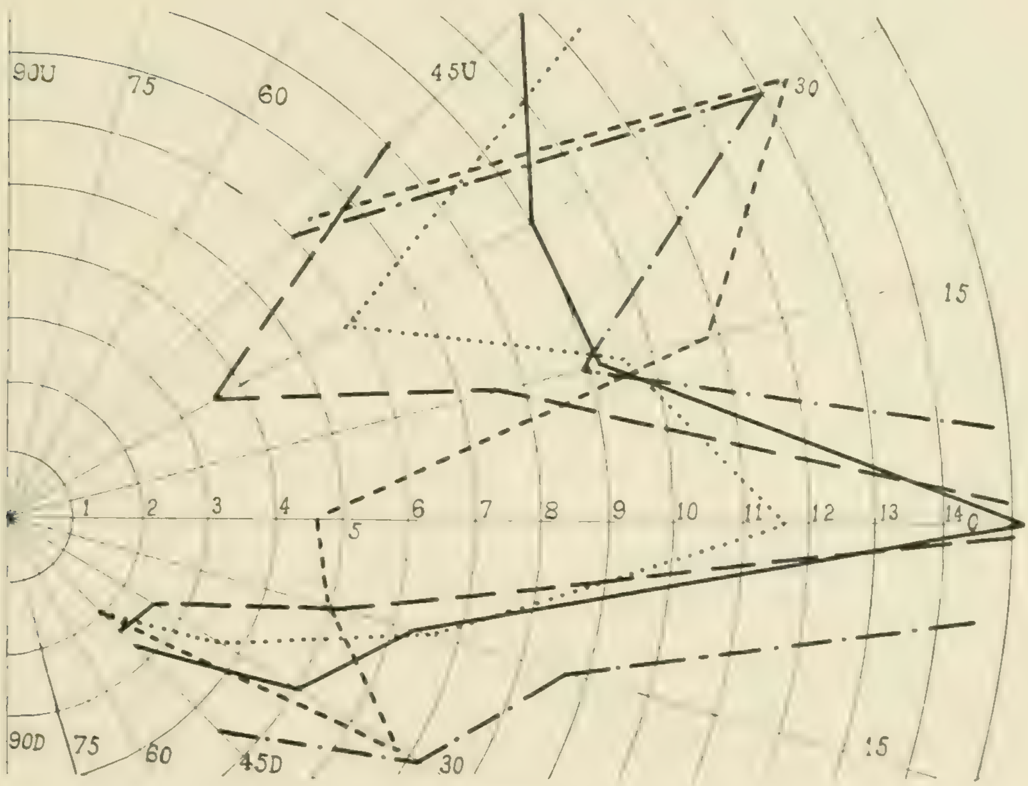


FIG. 23. Vertical plane 15°rb.

with the experiments of Bloch[1] who made some measurements in the vertical plane 90°r and found that his observer showed quite accurate discrimination in these two directions.

Localization at the side. *1. Introspective.* — The following illustrations are quoted from the records.

Standard 45°u. "Here the difference is very distinct. Those up seem as if there were some intervening object between the source of the sound and the ear, while those down come directly to the ear. It seems as if the upper edge of the pinna were a dividing line between the ups and the downs, the former being back of the pinna and the latter in the direct field of the pinna."

Standard 30°u. "The ups are higher in pitch."

Standard 0°. "The ups and the downs seem to be alike. It is confusing."

Standard 30°d. "The ups seem nearer and the downs seem down and farther away and strike the face differently."

These introspections indicate that localization is more difficult in some directions than in others, and that there are noticeable variations in intensity, distance, distinctness, pitch, etc.

For the purpose of determining more definitely the distinguishing features or local signs of direction, a special set of tests was made upon three other observers, *C*, *G*, and *Sc*. These observers had no special knowledge of the localization of sound and knew nothing of the results hitherto obtained. The three typical standards 45°u, 0°, and 45°d in each of the three vertical planes 45°rf, 90°r, and 45°rb were used as the localities of the experiment. The method of procedure was entirely the same as in the regular experiments except that the distance-interval was large enough to make the distinctions easily perceptible. This required interval was found in each case by a few preliminary trials. Ten trials were made with each standard by each observer. Before beginning the test the observer was told to describe as accurately and precisely as he could the differences that he noticed between the sounds from the directions under comparison. The results are given in tables V, VI, VII.

These figures scarcely need a word of comment.

(*a*) Intensity is the most effective datum. In Table V. under *up*, seventeen are said to be fainter and two louder; under *down*, nine are louder. In Table VI. under *down*, twelve are fainter and one louder; under *up*, seventeen are

[1] Bloch, *loc. cit.*, p. 39.

TABLE V.

THE STANDARD 45° UP IN EACH OF THE THREE PLANES.

Observers.	Planes.	Up.							Down.						
		Fainter.	Further.	Less Clear.	Pitch. h.	Pitch. l.	Thinner.	Displacement.	Louder.	Nearer.	Clearer.	Pitch. h.	Pitch. l.	Richer.	Displacement.
	45°rf	1(1)	2		2	2		3f	1	1			1		1b
C	90°r	3	2	2			1	1f	1	5			3	1	
	45°rb				1			3b	1	1			3	1(1)	1f
	45°rf	2		2				4f	1		3		1 (?)		5b
G	90°r	3(1)	1					2f	4	3					4b
	45°rb	3	2						1	3	4				
	45°rf	1			1	2				1		1	3		4f
Sc	90°r	4	1		3	1						1	1		5b
	45°rb		1(1)		3	2				1		2	3		
		17(2)	9(1)	4	10	7	1		9	15	7	4	15	2(1)	

The figures in parenthesis are the numbers of judgment of the opposite sort to those in the column under which they occur.

TABLE VI.

STANDARDS 45° DOWN IN EACH OF THE THREE PLANES.

Observers.	Planes.	Down.							Up.						
		Fainter.	Further.	Less Clear.	Pitch. h.	Pitch. l.	Thinner.	Displacement.	Louder.	Nearer.	Clearer.	Pitch. h.	Pitch. l.	Richer.	Displacement.
	45°rf					1			2	2	1	1			4b
C	90°r		1(3)	1		2			1	(1)		1			
	45°rb	1	2	1	1	3		1b	2	1			2		1f
	45°rf	3	2					1f 1b	4		1				1f
G	90°r	3	2					4b	3			1			3f
	45°rb	2(1)	4					2b	4	1					3f
	45°rf	2	1		3	2		3f			1		3		2f 2b
Sc	90°r	1	4			4		2f 1b		(2)		3	1		1f
	45°rb		3			5		1f	1	1		1	1		3f
		12(1)	19(3)	2	4	17			17	5(3)	3	7	7		

louder. In Table V. (standard 45°u) *up* means farther and *down* means nearer with respect to the aural axis and in Table VI. (standard 45°d) *down* means farther and *up* means nearer. Consequently the statement seems to be warranted here also, that, in the immediate vicinity of the aural axis, the nearer a sound is to the axis the stronger it seems, and *vice versa*.

Table VII.

Standards 0° in Each of the Three Planes.

Observers.	Planes.	Up. Intensity. m.	l.	Distance. m.	l.	Clearness. m.	l.	Pitch. h.	l.	Richness m.l.	Displacement.	Down. Intensity. m.	l.	Distance. m.	l.	Clearness. m.	l.	Pitch. h.	l.	Richness. m.l.	Displacement.
	45°rf				2	1		1			1f	3			2						2f
C	90°r			1			1	2	1									1	3		
	45°rb		1		2			2		1	1f		1	1					2		2b
	45°rf		3	1			1				2b		4				2				1f 1b
G	90°r	1		1				1			1b	2			2				1		
	45°rb	1	2									1	2								3f
	45°rf	1			2	2					1f	3			2	1		2	1		1f
Sc	90°r		3			1	1	1	2				3		2				5		
	45°rb	1						1	2		3b 1f							1	3		2f
		4	9	3	6	4	3	8	5	1		9	10	1	8	1	2	4	15		

(*b*) The second element in importance is distance which we may assume to be intensity in other terms and accordingly the same statement applies.

(*c*) Clearness and richness of sound are less conspicuous but nevertheless very decisive factors.

(*d*) The effect of pitch is somewhat obscure. To begin with, the results are not so definitely inclined toward one or the other side. The most general statement that is allowed is that a sound higher in position seems higher in pitch and one lower in position seems lower in pitch irrespective of the aural axis. It would seem that the difference is rather apparent than real and that the coincidence of higher and lower positions with the names, higher and lower pitch respectively, may be a matter of association or suggestion.

(*c*) Misplacements are quite frequent. They are of two kinds, forward and backward, which renders them intelligible. Let us suppose that a sound in the upper front quadrant below its standard seems backward and the one above this standard seems forward. It is reasonable to interpret these misplacements in terms of intensity and distance since the downward sound would appear louder and hence nearer, but knowledge of the positions of the sounds would unconsciously forbid the misplacement to be nearer in a radial direction toward the head.

In a similar manner the upward sound seems fainter and hence forward, *i. e.*, farther away. Some of these misplacements may occur for other reasons such as, anticipation, slight subjective or objective changes, etc.

In Table VII., where the standards are at 0°, *i. e.*, on level with the ears, the figures are indecisive and sometimes apparently contradictory. Under the column *up*, four are louder and nine fainter; under *down*, nine are stronger and ten weaker. Altogether thirteen are louder and nineteen fainter. It might be said that since the standard is nearer the aural axis than either the *ups* or *downs*, all should seem weaker. But it must be remembered that the subjective aural axis is not in the same position for all individuals. However, it is also possible that the observer may pay more attention to the second stimulus (*i. e.*, an *up* or a *down*) than to the first stimulus (*i. e.*, the standard) because he knows the position of the standard and the *up* or the *down* is the one to be determined.

If the second stimulus receives more attention it may induce the tendency to perceive it as louder. But this can scarcely impair the validity of these tables because they are quite decisive in view of this possible condition.

2. *Theoretical.* — Both the introspections and the curves present very much the same general features in the vertical as in the horizontal measurements. The factors of intensity, distance, quality, etc., and accordingly the transitions and the variations in the basis of discrimination play practically the same rôle in the vertical planes as in the horizontal planes. In these respects there seems to be no essential modification caused by the tilting of the horizontal plane through an angle of 90° to make it vertical.

On the other hand, Bloch found that his observer localized more accurately in the aural axis than at the other surrounding standards. But do we not meet similar conditions in passing from the point overhead to the axis as in passing from the front to the axis? Why should localization be more accurate relatively, in the aural axis, in the vertical measurements, than in horizontal measurements?

Comparing the upper and the lower quadrants, there is a

difference between the two in the absorption and reflection of sound by the clothes and body in the lower quadrant.

Comparing the composite curve of the vertical plane 90°r with the composite of the horizontal plane through the aural axis, we notice that the delicacy at the points above and below in the vertical plane is practically the same as at front and back in the horizontal plane. But at the other points localization is not so keen in the vertical as in the horizontal planes.

B. The Median Plane.

Median plane localization has long been known as least developed. Professor Seashore[1] found that, after all judg-

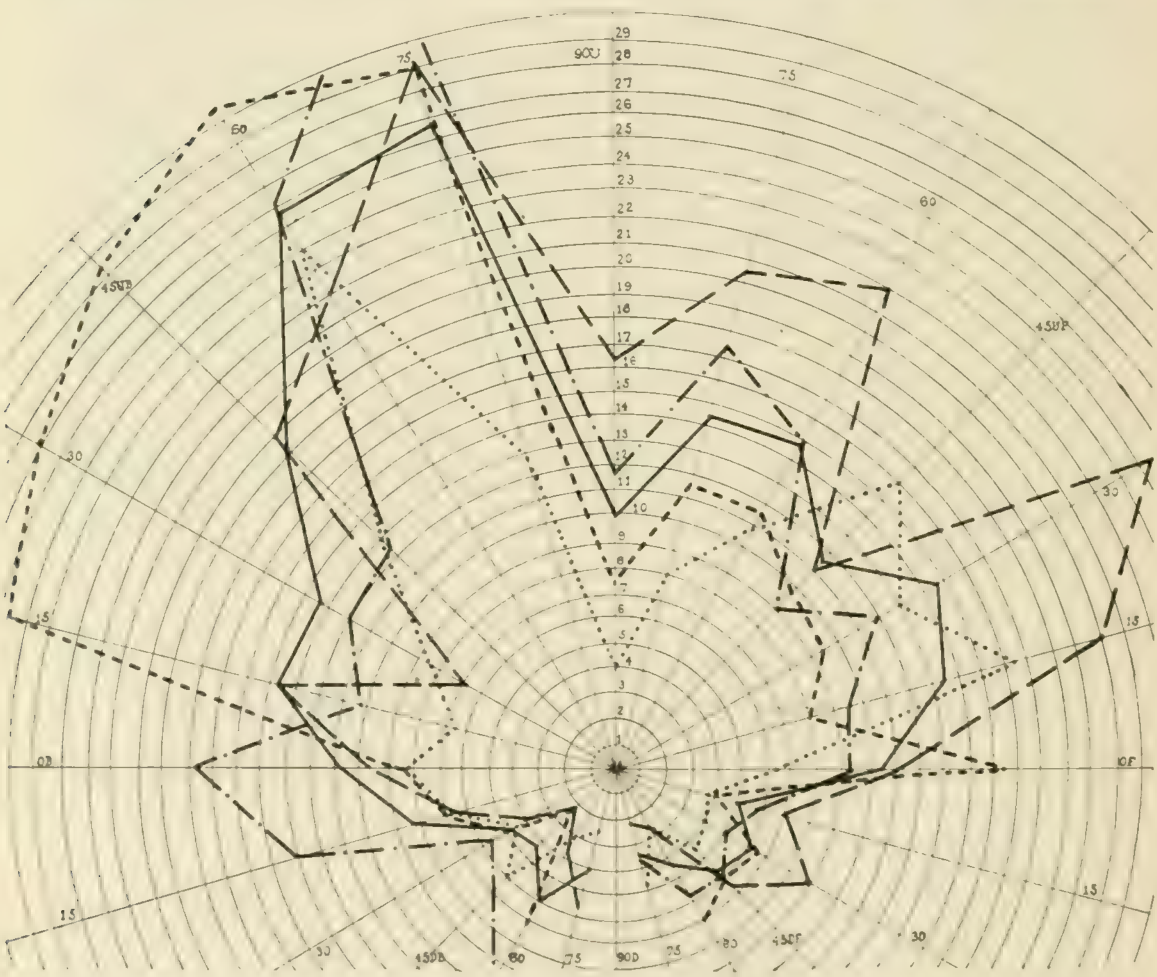


Fig. 24. Median plane.

ments correct by chance were deducted, there is a residual of eight per cent. and the implication is made that unfamiliar sounds

[1] *Univ. of Iowa Studies in Psych.*, 1900, II., pp. 46–54.

in the median plane are localized correctly only as often as chance requires.

But the conditions are somewhat different in these experiments where the sound is familiar to the observer and where the locality in which it is expected, is known so that there is almost surprising ability in median plane localization.

The Curve. — (*a*) Discrimination is more accurate in the anterior than in the posterior half, and in the lower than in the upper half. (*b*) The points of relatively accurate localization are 90°u, 45°uf, 15°df–75°df, and 45°db.

Introspective. — To illustrate the process of localization we quote again from the records.

Standard 90°u. "The standard seemed to be at 60°uf. The fronts sounded to the right and the backs to the left. This distinction seemed simply chance."

Standard 90°u. Another observer. "The fronts seem stronger."

Standard 75°uf. "The ups seemed left and the downs right."

Standard 75°uf. "The ups seem richer and closer than the downs."

Standard 60°uf. "The standard seemed to be only 35° up instead of 60°. The ups sounded just a little up and to the right. The downs seemed exactly the same as the standard, only nearer."

Standards 60°uf, 45°uf, and 30°uf. "The downs seemed richer and nearer than the ups."

Standards 45°uf and 30°uf. "The ups seemed to the right and farther away and the downs seemed to the left and nearer."

Standard 15°uf and all standards below that. "The ups seemed richer and nearer than the downs."

Standard 30°df. "The downs seemed farther away."

Standard 45°df. "All sounds seemed back of me. There was a slight difference between the ups and the downs."

Standard 60°df. "The standard seemed 22°db and a little nearer than it is in front. The downs seemed a little to the right."

Theoretical. — That the posterior half of the median plane is much less accurate especially in the upper quadrant can be attributed to the fact that the pinnæ are not so well adapted to receive sounds from the rear. That the lower half is more accurate than the upper half is very probably due to the modifying effect of absorption and reflection of the body of the observer.

There are some quite definite distinctions between directions in regard to richness, intensity, and distance. In the anterior half there are two localities from which sounds are perceived more correctly than from the other localities, *i. e.*, there are two

localities in which sounds seem richer and nearer. The composite curve has here two regions of more accurate perception of direction, 45°uf and 15°uf– 75°df. It seems probable that this condition is due to the pinnæ. Localization is more dependent upon the secondary factors.

These results are similar to those obtained by Bloch,[1] although the points do not coincide exactly, and his suggestion that the decreased delicacy between the two regions of more delicate discrimination is due to the tragus which hinders accurate perception, receives further support from these results. There is a rise and then a fall of the composite curve at 60°db which is probably due to the interference of the lobus and antitragus if we follow out the suggestion of Bloch.

Auditory Discrimination Ellipses.

Fig. 25 epitomizes the results of both horizontal and vertical measurements. The conception of sensory circles on the skin suggested this scheme of representing the space discrimination in the field of hearing. The figures may properly be called auditory discrimination ellipses. The vertical and the horizontal planes studied are represented by the straight lines, whose intersections represent the points used as standard directions in the experiments. This chart therefore corresponds to Fig. 2, and represents the right hemisphere in the field of hearing. The horizontal and the vertical axes of the ellipses represent the two measurements made at each point. As in sensory circles on the skin, we may imply that any axis of one of these ellipses represents the probable discrimination for points touched by that axis. Thus, in the ellipse at 90°r, plane 0, the horizontal axis represents 4.5°, the vertical axis 8°, and the axis with an inclination of 45° represents approximately 6.5°. This chart, then, exhibits all the numerical results of the two leading series of experiment in a graphic way.

General Observations.

1. Some additional observations were made which are either on accompanying phenomena or possible data for localization.

(*a*) Visual imagery of the position of sounds was very prom-

[1] *Bloch, loc. cit.*, p. 42.

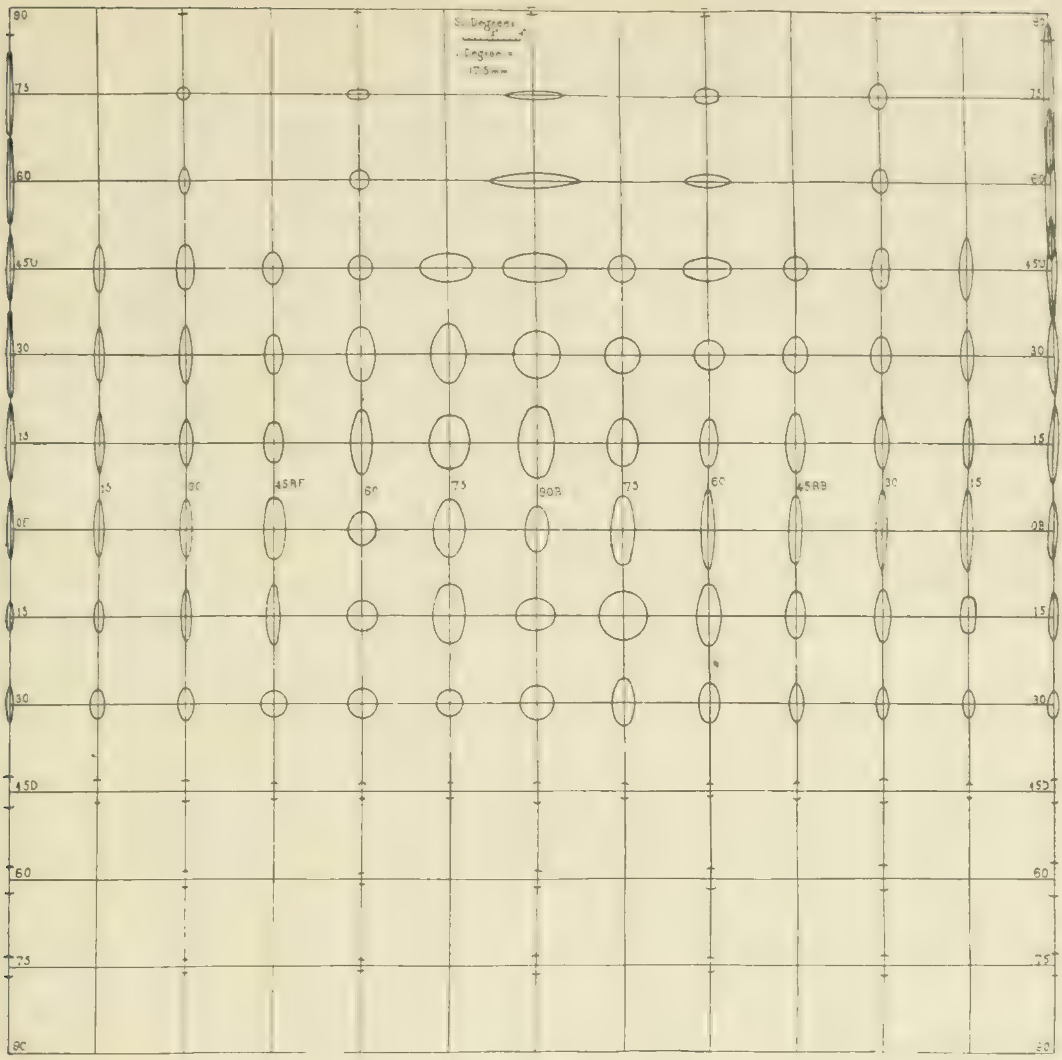


FIG. 25.

inent in the judgments of all observers. Very frequently they were conscious of scarcely anything beyond visualization. If the standard was visualized in another than its true position it was at times disturbing. This was most noticeable in the horizontal planes 60° and 75° above the aural axis, where a standard could be imagined as located in any position overhead.[1] Münsterberg[2] also found that if the observer fixated his attention upon a certain point misplacements were made that did not occur otherwise.

(*b*) With one observer tactual sensations were apparently a subsidiary means of distinguishing directions. 'Sounds from different directions strike the pinna differently' was the often

[1] Angell, *loc. cit.*, p. 14.

[2] *Loc. cit.*, p. 470.

repeated remark. He also seems to have been quite consistent in his perception of apparently different cutaneous sensations since at a certain standard he stated that the 'sounds do not strike the ear differently'; and, as the curve indicates, this is the least accurate position. It seems hardly probable that cutaneous sensations are as important as they seemed to be for this observer. It is a well known experience that if one directs his attention to any part of the body, the organic sensations of that part rise into consciousness. On the other hand, the directing of the attention to it would make more acute any differences of cutaneous stimulation that might be due to sound. But the evidence furnished by a special experiment on this point by Angell and Fite[1] seems quite conclusive to the effect that 'cutaneous sensations constituted a small part, if any, in the data of localization.' Their experiment was concerned not with the pinnæ themselves but with the surrounding parts, and it does not absolutely disprove the statement of Wundt,[2] that the pinnæ are delicately sensitive to pressure on account of the presence of tiny hairs. Further experiments are necessary to determine whether cutaneous sensations play a part in localization.

(*c*) Motor adjustment or the tendency to turn and face the sound was observed a few times, and turning the eye-balls in the direction of the sound was noticed more frequently.

2. A few very striking misplacements occurred which can be described by quoting from the records.

Vertical plane 15°rb. Standard 30°u. "The standard seemed to be 50°rf, 20°u, and eighteen inches away from the head. The ups were 15°rf, 60°u, and four feet away. The downs were 15°rf, 30°u, and one foot away." This misplacement occurred only three or four times and was especially conspicuous with one observer. In each case it was noticed in the same locality, *i. e.*, in the rear quadrant. A similar misplacement was found in the median plane.

Standard 45°ub. "The standard seemed 60°lf, 15°u, and two feet away. The ups seemed 45°lf, 45°u. The downs seemed straight back on level with the ears and a little nearer than they actually are."

Standard 30°ub. "The standard seemed to be at 60°lf, 15°u, and two feet away. The ups seemed 20°lf, 60°u, and six feet away. The downs seemed just back of the head and a little to the left."

3. After all the trials at a certain standard had been made,

[1] *Loc. cit.*, p. 236.

[2] *Physiologische Psychologie* (5th ed.), II., p. 487.

it was noticed that nearly all trials were errors. The experimenter then wanted to take a larger step, but this proved a surprise to the observer, because he had felt quite certain of his localizations and thought that he had made only a few errors. It was then suggested that he had probably reversed his process of localization for some unknown reason. The same interval was then tried again, but the observer always answered the opposite from what the direction seemed to be, for example, if a sound seemed above the standard he called it below. It was then found that he made only a few errors with the same interval. This occurred with only two observers and at the following standards: V. plane 60°rf, St. 15°u; V. plane 30°rf, St. 60°u; V. plane 60°rb, St. 15°u and St. 30°u; V. plane 45°rb, St. 0°; V. plane 30°rb, St. 60°u.

4. Comparing the first section of the double fatigue order with the second there is some improvement in the perception of direction, especially in the median plane. Pierce[1] has demonstrated that median plane localization is subject to much improvement. He concludes that 'if the exigencies of life required, an ability to locate sounds perfectly [*sic!*] within the median plane could be acquired.' The prediction of v. Kries[2] who said that, median plane localization may be possible 'wenn so zu sagen bereits erlernt ist, wie er [the sound] von vorn her und wie er von hinten her klingt' is fully verified.

5. After a large number of stimuli had been given on the right side in passing, for instance, from front to back it was very frequently observed that in testing the last point, 0°b, the standard seemed 10° or 15° toward the left instead of directly back and the sound on the left seemed much farther to the left than the one on the right seemed to the right. This was common to all four of the regular observers. Whether this is a characteristic of attention or a result of greater fatigue of the right ear is not certain. Professor Angell[3] mentions similar misplacements toward the unfatigued ear. Münsterberg and

[1] *Loc. cit.*, p. 103.

[2] Ueber das Erkennen der Schallrichtung,' *Zeitsch. f. Psych. und Physiol. d. Sinn.*, 1890, I., p. 237.

[3] *Loc. cit.*, p. 10.

Pierce[1] on the other hand find that only one out of five observers misplaced sounds after one ear was unusually fatigued.

6. The four regular observers may be divided into two types. In the one type, *W* and *S*, the curves are quite near to the center while in the other type, *K* and *B*, they sweep out farther. This suggests individual differences in shape and contour of the outer ear, differences in mental characteristics, as, in the retention of auditory images, in the presence or absence of visual imagery, etc. From another point of view there are types of observers according to the prevalence of misplacements, intensity, distance, quality, etc., in localization.

7. The fact that the sound was uniform through all the experiments and the resulting familiarity of the observers with it had marked influence upon the absolute accuracy of localization, but the validity of the comparative accuracy of the different directions is not thereby impaired.

Summary.

A twofold aim was stated at the outset. In regard to the discrimination between directions, the results show that:

1. In the horizontal planes, localization is most accurate in front, nearly as accurate in the back, and least accurate at the side. In the vertical planes excluding the median plane, localization is most accurate above and below, and least accurate at the side. In the median plane, localization is less accurate than in any other plane; it is more accurate in the anterior half than in the posterior half.

2. The delicacy of localization does not decrease gradually in passing from the median plane toward the side either in the horizontal planes or in the vertical planes, exclusive of the median plane. There are five curve prominences, *i. e.*, localities of less accurate localization, in passing from front to back, or from the point overhead to the point below.

In regard to data and elementary processes, the results show that:

3. In the greater part of the field, the localization depends chiefly upon the ratio of intensities received by the two ears.

[1] *Loc. cit.*, p. 471.

4. But within a considerable area around the aural axis, the localization is almost entirely monaural; yet intensity plays an important rôle.

5. It depends also upon other quantitative and qualitative characteristics, such as, apparent variations in intensity and distance, richness, clearness, timber, pitch, etc.

6. Variations in the characteristics of sounds occur systematically. In the immediate vicinity of the subjective aural axis, sounds nearer to the axis seem louder, nearer, richer, and clearer than sounds farther away. About the middle of each quadrant, sounds seem fainter and farther away. There are changes from binaural to monaural localization, and from monaural to binaural localization. There are also variations in the data of both binaural and monaural localization.

7. Corresponding to the variations in the data of localization there are changes in the process of localization. There are five transitions, which correspond to the five curve prominences.

8. Other features are, cutaneous sensations, motor sensations, visualization, and illusions of misplacements.

PERIODICITY AND PROGRESSIVE CHANGE IN CONTINUOUS MENTAL WORK.

BY C. E. SEASHORE Ph.D., AND GRACE HELEN KENT, A.M.

This study of the fluctuations in the efficiency of continuous mental work deals with three distinct processes: sensibility, discrimination, and memory. The experiments accordingly fall into three series. The common aim in all the series was to secure records of continuous work in representative processes under satisfactory experimental conditions. The most essential of such conditions were that the processes should be natural, definite, controllable, repeatable, recordable, and relatively free from varying associations, and that the elements of the processes should be as constant as possible, even throughout long-continued repetition.[1]

Fatigue[2] was the primary object of interest and search, but that illusive yet ever intrusive factor is almost hopelessly lost in the umbrage of related processes. Our leading effort has been to secure reliable and analyzable records of work done, then to discover the actual fluctuations in such work, and eventually to trace in part the rôles played by known factors as causes of these fluctuations.[3]

SERIES I. SENSIBILITY.

Problem, Apparatus, Method, and Observers.

The experiments in this series deal with the fluctuations in auditory sensibility which result from continuous work in listening to a liminal tone. The act which constituted the work con-

[1] The writers of this article are deeply indebted to the work of Dr. Florence Brown Sherbon whose experiments on the same subject, in this laboratory, preceded the present research. Both in the planning of our experiments and in the interpretation of the results, we have used freely the knowledge gained through the earlier work.

[2] My article on 'The Experimental Study of Mental Fatigue,' PSYCH. BULL., 1904, I., 97–101, constitutes the logical introduction to this report. C. E. S.

[3] The reader may profitably turn to Part IV. and read the 'General Conclusions' first; he will then be able to read the detailed account of the experiments more critically, having the full scope of the work in view.

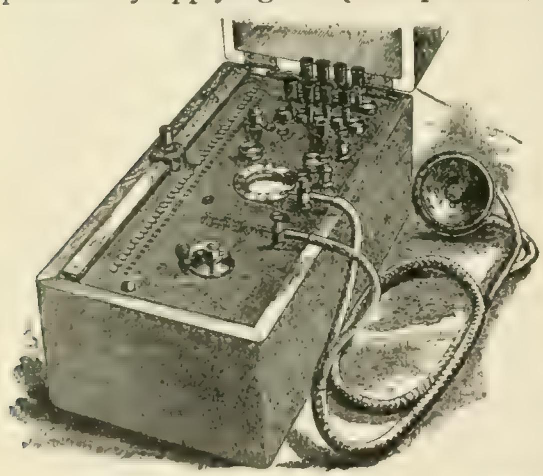
sisted in *determining the moments of appearance and disappearance of a tone which oscillated 'incessantly' about the threshold.* In the following discussion, one such double determination will be spoken of as a single act — the act. The work of the whole period of an experiment consisted in the continuous repetition of this simple act. The serial record of the two thresholds which were thus determined constituted the measurement.

The stimulus was a continuous tone of variable intensity, produced by a 100 v.d. electric tuning-fork, through the audiometer.

Since some idea of the construction of the audiometer is necessary for the understanding of this report, we herewith insert, for the convenience of the reader, some extracts from the original description.[1]

THE AUDIOMETER.

The essential and unique feature of this apparatus consists in the method of varying and measuring the relative intensity of the sound. This is accomplished by applying the principle that, for certain given relations between the primary and the secondary coils of an induction coil, the induced current varies directly with the number of turns of wire in the secondary coil. The complete apparatus consists of an induction coil, a battery, a galvanometer, a resistance coil, switches and a telephone receiver, all except the receiver being built into one compact and portable piece.



A dry battery is so connected that it may be thrown into the primary circuit of the induction coil by turning the left-hand switch. The galvanometer, seen through the crystal in the center, may be thrown into circuit by turning the right-hand switch. The fall of potential over the primary coil is reduced to the standard e.m.f., by varying the resistance by means of the plugs at the farther end of the chest and gauging it by the galvanometer. The resistance permits of as small variations as can be detected by the galvanometer; and the galvanometer detects smaller variations in the current than can be detected by the ear at the receiver. The lever at the near end of the chest is a key which is used for the rapid closing and opening of the primary circuit in producing the stimulus. No current is drawn except for the moment that the circuit is closed by this key. The primary coil is longer than the secondary. The latter is wound in forty sections, arranged in a series according to the number of

[1] Seashore, 'An Audiometer,' *Univ. of Iowa Stud. in Psych.*, 1898, II., 158–163.

turns of wire that each contains, as may be seen in the accompanying table. Each of these sections is so connected with the surface terminals along the scale that the spring contact on the sliding carriage throws into circuit the number of sections indicated by the numbers on the scale. Therefore, to vary the energy communicated to the receiver in this circuit, it is necessary only to move the carriage along the scale to the proper terminal. As it is most convenient to vary the stimulus in a geometric ratio according to the psycho-physic law, this principle has been taken as a guide in determining the scale of intensities of the sound. The numbers on the audiometer scale are given in the first column in the accompanying table; these indicate the corresponding number of sections involved in the secondary circuit. The second column gives the corresponding number of physical units in terms of the total number of turns of wire in circuit. The ratio of the increments in the sound is such that the forty steps in the series are, as nearly as can be determined, psychologically equal. The serial numbers on the scale are used in all readings. These measurements all refer to the strength of the current which energizes the receiver. The functional relation between the strength of current and the amplitude of vibration in the receiver is somewhat complex, but for the present purpose it may be regarded as fairly uniform.

Table of Values for the Audiometer Scale.

I.	*II.*	*I.*	*II.*	*I.*	*II.*	*I.*	*II.*	*I.*	*II.*
1	1	9	9	17	32	25	107	33	368
2	2	10	11	18	37	26	125	34	429
3	3	11	13	19	43	27	146	35	500
4	4	12	15	20	50	28	170	36	583
5	5	13	17	21	58	29	198	37	680
6	6	14	20	22	68	30	231	38	793
7	7	15	23	23	79	31	270	39	925
8	8	16	27	24	92	32	315	40	1079

I., scale on the audiometer.

II., corresponding values, *i. e.*, number of coils in the secondary circuit.

The range of the intensity of the sound is such that it is not probable that any person can hear the weakest sound and all who can hear ordinary conversation at all can hear the strongest sound. The average threshold for normal ears lies near the middle of the scale.

For certain tests by aurists and experiments in the psychological laboratory, it is desirable to have a tone instead of a click for stimulus. Provision has been made for the production of tones in the audiometer. The inside connections are so arranged that by attaching a double contact electric tuning-fork to the binding posts seen to the right, the fork may be made to interrupt the primary circuit of the audiometer and thus produce the tone of the fork in the receiver. This tone may be varied and measured in the same way as the regular stimulus.

An electric sounder in the measuring-room was connected with a light strap key held freely in the hand of the observer in the observing-room. The motor process which was necessarily involved in the act as defined above, and constituted the response of the observer, consisted in keeping the key closed when he heard the sound and open when he did not hear it.

The strength of the sound at the time of closing the key was recorded as the upper limit of the threshold and at the time of opening the key as the lower limit of the threshold. These two limits may be called, respectively, *To* and *Tu* (' threshold over and threshold under ').

The intensity of the sound was varied by moving the carriage of the audiometer over the scale of psychologically equal units of difference in intensity. The experimenter was guided by a metronome in moving the carriage at the rate of one step per second. Starting at a point below the threshold, the carrier was moved upward at this uniform rate until the *To* signal indicated that the sound was heard; the direction of movement was then immediately reversed and continued at the same rate until the *Tu* signal was heard indicating that the sound had become inaudible; the direction of movement was then immediately reversed again and continued as before, thus making a continuous oscillation about the actual threshold throughout the whole experiment. It is evident, therefore, that the quality of the record depended upon the alertness of the observer and that the height, width, and uniformity of the threshold constitute relative measures of the efficiency of the observer at any given time.

Each experiment was continued two hours, which is a long period for continuous and homogeneous work, and probably long enough to bring out the normal fluctuations for any ordinary single period of work. The observer was seated as comfortably as possible in the observing room.[1] The room was dark and quiet and there was no avenue of communication except the signals described. The observing room is 12′ 2″ × 12′ 7″ × 10′ 8″. No ventilation was carried on during the experiment period, but the room was thoroughly ventilated with fresh air by an electric fan just before each experiment and the observer was alone in the room.

The experimenter had an assistant to record the readings

[1] This room is described in *Univ. of Iowa Stud. in Psych.*, 1902, III., 140. Ordinarily it is relatively sound-proof, light-proof, and jar-proof, but at the time of these experiments, there was a temporary disarrangement by which this room made contact with the main building. Therefore it was not so quiet as would have been desirable; strong sounds from the outside could penetrate faintly.

from the audiometer and to divide the record into five-minute periods. In all except experiments III. and X., a telephone receiver with a head clasp was used and tied lightly to the head of the observer with a band in order to secure constant adjustment. Other particular precautions will be discussed later.

The conditions thus briefly described comply fairly with the requirements as laid down at the outset. The act was natural — 'Hold the key down while you hear the sound.' It was definite: the only question which should arise was, 'Do I or do I not hear that particular sound' — and that was the question continually in the mind; the audibility of the sound at every moment was the element measured; the act was simple, familiar, and clear cut. It was controllable; the stimulus was under control, and ordinary disturbances were excluded. It was repeatable; the setting did not change by repetition and the progressive change in the internal nature of the act was open to analysis on the ground of known conditions.

The real work was in the cognitive process. The motor process was practically automatic; it was not wearing but, on the other hand, afforded a sense of relief from the otherwise restricted attitude.

While this positive statement of successful attainments is true in the relative sense in which we describe and control psychological conditions, the very rigidity of the conditions revealed shortcomings not otherwise noticeable, and no one can be more cognizant of these than the experimenters. Even if not expressly eliminated, such factors will be duly weighed before reaching our final conclusion.

There are two fundamental factors in a continued threshold test of this kind. One is the change in the physiological irritability of the peripheral organ and the other is change in the central power of concentration of attention. The latter would be the same for the two ears; therefore any change in the sensibility of the unused ear that may take place during the experiment is probably central. This fact suggests a simple test which would seem to be concise and crucial, but we encountered very serious obstacles in the way of controlling the conditions. Immediately before and immediately after the two-hour period,

a test of twenty trials was made upon each ear, in the double fatigue order, by the same method that was followed in the main experiment. The object was to determine the threshold of each ear, under similar conditions, when rested and when fatigued.

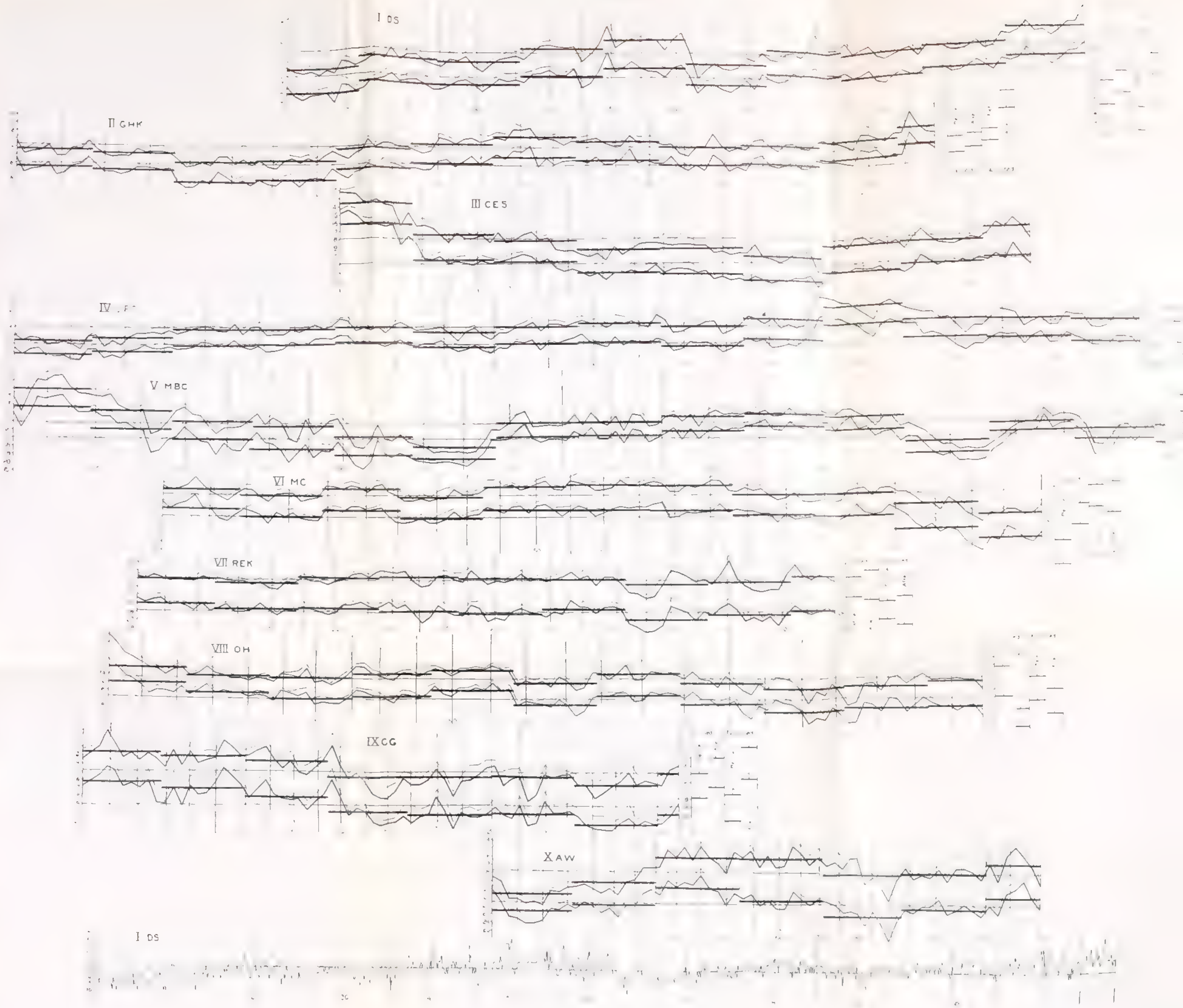
There are ten experiments in this series taken on as many observers, but all under similar conditions. The observers in experiments I., III., IV., VI., VII., VIII., and IX. are men and in experiments II., V., and X. women. These ten persons represent widely different degrees of practice, general efficiency in observing, and endurance. I. (D. S.), scholar in psychology, was thoroughly familiar with the situation and the conditions of the experiment. II. (G. H. K.), also scholar in psychology, and III. (C. E. S.) were the writers. X. (A. W.) was a first-year student in psychology, somewhat familiar with laboratory methods, but was not trained as an observer. The other observers were all students in the technical laboratory course and, with the exception of VIII. (O. H.), had had more than half a year of training as observers in the course. All knew the purpose and conditions of the experiment and took an active interest in it; but none of the observers, except the experimenters, had seen any other record of the kind.

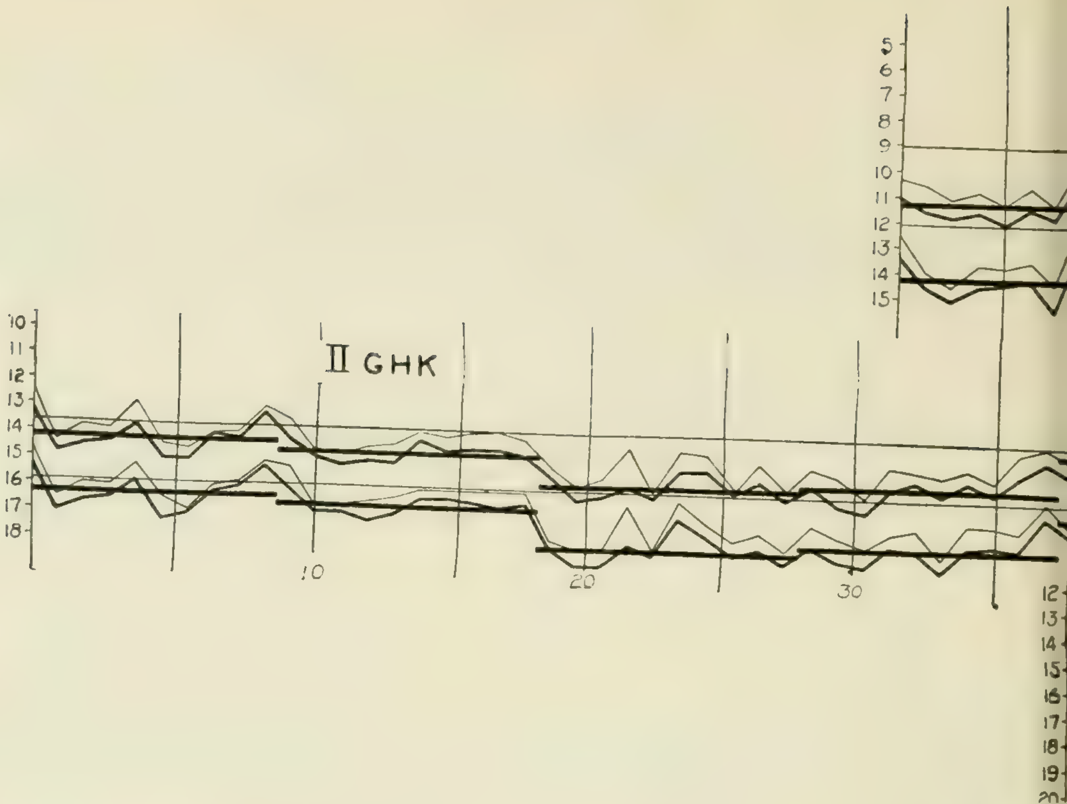
Each observer was allowed a preliminary practice of from two to five minutes, according to need, — enough to make the requirements and the nature of the experiment clear. This small amount of practice was quite sufficient because the act was extremely simple and all, except observer X., had previously served both as observers and experimenters in the measuring of hearing ability by this very method and apparatus.

Explanation of the Records.

The records consist of two series of numbers representing, respectively, the successive readings for the just perceptible sound, *To*, and the just non-perceptible sound, *Tu*. The numbers of each series were averaged by tens and by hundreds, and the mean variation found for each group of ten. Instead of printing tables, we present the results in the form of curves. This method is economical and throws the results into a better single perspective than could be obtained from the tables alone.

Plate I




II GHK
10
20
30

These curves constitute Fig's I. to X. in Plates I., II., and III. Fig. I. *a*, in Pl. I. is a representation in detail of the *To*-measurements of experiment I., Fig. I., which will be explained later.

Each of the curves represents a two-hour record of one observer. The vertical lines divide the records into five-minute sections. The light horizontal lines which extend the full length of the record show the average for the entire period, the lower being *To* and the upper *Tu*. The heavy horizontal bars mark the averages by hundreds. The heavy zigzags show the averages by tens, and the light zigzags, which are drawn on the heavy zigzags as base, show the mean variation for each of these tens. The figures at the left end give the scale of intensities in terms of the readings of the audiometer. For the purpose in hand, it is sufficient to remember that the smaller the number, the better the sensibility; so that the higher a point in the curve is, the keener sensibility it represents.

The audiometer was adjusted to different standards for different observers, in order to bring the threshold within the most convenient range of the scale; hence the absolute heights of the thresholds for different observers are not to be compared.

The observers indicated, by signals, the time of all special disturbance and had separate signals for subjective and objective disturbances. In the tabulation of the results the record of the act at such a signal was recognized only for its time-value.

The results of the before- and after-tests are represented graphically at the right end of each curve. The long average-lines of the main record are represented by dotted lines for the purpose of facilitating comparison. The horizontal lines show the average *To* and *Tu* before and after. Each is the average of twenty trials, the mean variation of which is represented by a vertical line upon the horizontal. The units in the scale of intensities are on the same scale as in the main curves.

Criteria of Change.

At least three criteria may be taken into consideration in an attempt to evaluate the efficiency of the work represented in these records. They are: (1) The height of the threshold; (2) the mean variation; and (3) the width of the threshold.

The first is self-evident: the higher the curve, the keener the sensibility. It is a quantitative measurement.

The mean variation may be large or small regardless of whether the sensibility is keen or dull. In this experiment it is probably not a measure of the sensibility of the sense organ, but of the power of concentration of attention. It is, however, a measure only so long as there is a continuous, maximum effort of concentration, which is the condition sought in these experiments. Even then it must be interpreted with great precaution and only in the light of introspective accounts.

The width of the threshold, which is the difference between *To* and *Tu*, depends upon the alertness of the observer. Slow reaction tends to give a low *To* and a high *Tu*, thus increasing the difference in both directions. A wide threshold means a long act; hence the number of acts in a given period varies inversely with the width of the threshold. The width of the threshold in records III. and X. is due in part to the method of reaction employed by the experimenter in these which were the first two experiments, but the method was uniform throughout the records. With this limitation, the characteristic width of each record is probably an expression of the personal equation of the observer. It may be assumed, other things being equal, that a narrow threshold indicates alertness, *i. e.*, steady keenness in discriminative attention. The principal counter-factor is the tendency to automatism. The automatism is at least favored by the approximate coincidence of the time of hearing with the high crest of the normal attention-wave. The feeling of 'let it go' came not only from the physical change in the stimulus but also from the termination of the subjective attention-wave. One of the writers experienced that very distinctly in the special experiment on that point (see p. 60, following).

Before we make any physiological or psychological interpretation of the records, we must inquire whether the fluctuations may not be due to changes in the stimulus. We have taken every precaution to keep it constant. The receiver on the audiometer is of good quality and well seasoned; we used the Edison-Lelande cells; the temperature was practically constant; as the interruption by the fork took place in a shunt cir-

cuit, the main circuit remained permanently closed; and the current was minimal so that there could not be serious danger from permanent self-induction. Therefore, although we have no absolute proof of the constancy of the stimulus, we must proceed on the assumption that it remained at the same standard. We may also invoke the evidences obtained in other series of experiments and especially those on sight. As will be seen, the conclusions drawn from this series on sensibility are all corroborated by the experiments in the two following series. They were also corroborated in visual experiments in which we had absolute control of the stimulus.

Periodic Change: A. Hour-waves.

The most salient feature in the records, especially with reference to the height of the threshold, is a periodicity. The records agree in showing at least two sets of rhythmical fluctuations; and, in addition to these, there enters the well known attention wave, which practically coincides with the individual acts and therefore does not appear as a wave in the record. For convenience, we may designate the three sets of waves, respectively, as

1. The hour-waves (20 to 200 minutes).
2. The minute-waves (½ to 20 minutes).
3. The second-waves (a few seconds).

The hour-wave can be seen most clearly by following the main zigzag lines showing the averages by tens. In some records two sets of hour-waves are discernible. For convenience we may call them the large and the small. The dividing line between the two groups is arbitrary and may be taken at about thirty or forty minutes.

In order to show approximately the number and length of the hour-waves a diagrammatic table, Table I., is given showing the upper and lower points, the crest and the basin, of each wave. The numbers denote the time, counting in minutes from the beginning of the test, and they are placed in an upper or a lower line according as they represent high or low points in the waves. The wave-length is proportional to the horizontal distance between the numbers, but differences in height are not

Table I.

Periods of the Hour-waves in Figs. I. to X.

I.	50 80 120	5 65 90
I.	20 50 80 110 120	5 30 65 90 115
II.	0 60 115	30 90
III.	0 115	75
III.	0 35 55 100 115	20 45 80 110
IV.	35 85	5 60 115
IV.	20 35 60 90 105	5 25 50 75 100 115
V.	5 85 110 120	55 100 115
V.	5 20 45 60 90 110 120	0 15 35 55 75 100 115
VI.	5 25 60 100	15 35 90 110
VII.	0 40 75 100 115	20 60 85 110
VIII.	0 55 75 120	30 65 100
IX.	5 25 50 70 90 120	15 40 60 80 105
X.	45 115	5 90
X.	0 20 45 65 100 115	10 25 50 90 110 120

shown. In these estimates, *To* is taken as the principal guide because it is a more reliable index to the moment of perception than *Tu*; the appearance of a sound can be determined more definitely than its disappearance.

The estimates in this table are, of course, somewhat arbitrary. In many cases there is latitude for differences in interpretation. The table represents the estimates upon which the writers have agreed. In five records, (I., III., IV., V., and X.) large and small hour-waves are discernible in more or less distinct sets, as indicated in the table. II. and VIII. might also

have been divided into short waves, but these waves are not very distinct.

The waves vary not only in length but also in form. While the general tendency is an approximation to the sine curve, this form suffers all sorts of distortion. On the assumption that there are two or more sets of waves, one can readily see the effect of interference and reinforcement. But many sporadic variations seem to be due to aperiodic influences.

One very expressive feature is that there is a tendency for the hour-wave to be shorter in the latter half of the record than in the first. This may be seen on a glance at Table I. Where there are two sets of hour-waves there is a tendency for the two to coincide near the end. Compare the two sets of waves, *e. g.*, in Record I., Table I.

One might suppose that every record should begin with a high crest, but there is no constant tendency in that direction.

Periodic Changes: B. Minute-waves.

We have spoken of the main curves as zigzags. These zigzags bring out the minute-waves. In order to show these short waves more clearly than they are shown in the curves of averages, a section of the *To* from each of the ten curves is represented in detail (Pl. II.). All these sections begin with the beginning of the second half hour of the record and include two hundred acts. This portion of the record is selected because it is perhaps freest from erratic variations, coming as it does after a period of adaptation and before the onset of discomfort.

Fig. I*a*, Pl. I., represents the whole *To*-record in this manner for Record I., to which it runs parallel. It shows how the minute-waves enter as partials in the hour-waves.

The tendency toward periodicity is unmistakable, but the waves are not homogeneous, nor are they limited to one system. Here, as in the longer waves, different sets of tendencies are operative producing reinforcements, balances, or interferences. One wave appears as a partial in another and is itself made up of ripples. A minute-wave may be a partial in an hour-wave: there is a gradual transition from one to the other.

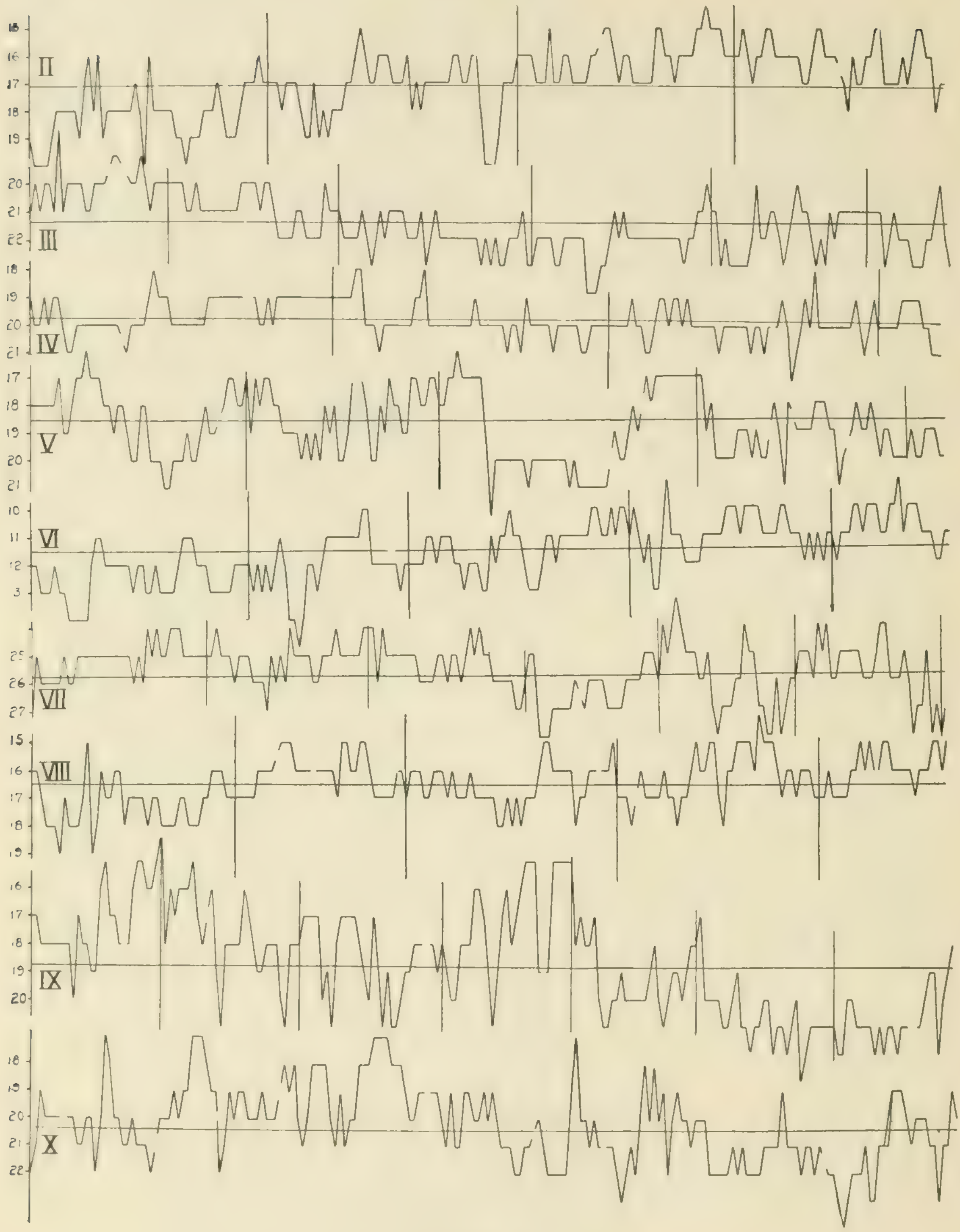


PLATE II.

Periodic Change: C. Second-waves.

The attention-wave of Urbantschitsch[1] plays an important rôle in this work. It is unnecessary to enumerate here the con-

[1] *Med. Centralbl.*, 1875, 628 ff. Good summary accounts are found in Titchener, *Exp. Psych.*, 'Instr. Man'l, Qualitative,' 194 ff., and in Wundt, *Physiol. Psych.*, 5th ed., III., 366 ff.

clusions of the valuable researches on that wave. We have assumed its existence and it remains to point out under what conditions it entered into the present process. We shall try to show that the attention-wave in this experiment is synonymous with, or rather, constitutes, what has been defined above as the second-wave.

The act which is the object of study in this series on sensibility, as defined on page 3, forms a peculiar basis for the second-wave. Owing to the method employed in recording, these waves do not show in the records, but the known conditions and requirements and the introspective accounts furnish us satisfactory evidence of their existence.

The act may be regarded as the basis for one wave or for two, according to the point of view taken. According to the former, not only is the duration of each act approximately the length of an attention-wave, and its recurrence periodic, but the stimulus constitutes a wave in its intensity change — rising from an imperceptible stimulus, through the just perceptible, to the more than just perceptible, and then back, receding to the non-perceptible. Corresponding to this, there is a complete wave of consciousness, for, on account of associated imagery, the subliminal part of the wave is as concrete in consciousness as the supra-liminal part.

According to another point of view, the act readily divides itself into two complete and distinct movements of attention, the maxima of attention being just before *To* and *Tu*, respectively, and the corresponding minima immediately after these points. The two waves in an act differ quite radically in character, but they both serve the same purpose, namely, rest through relaxation of attention. A moment of relaxation followed the perception indicated by *To* because the sound grew relatively strong during the united reaction-time of the observer and the experimenter; and, the approximate duration of this intensity was known from the preliminary practice. Then a moment of relaxation followed the perception indicated by *Tu*, from the conviction that the sound had gone below the threshold and there would be a certain appreciable time before it could return. Thus, in one case the attention relaxed for a moment because

the sound was so strong that it could be heard with ease, and in the other because the observer assumed that, for the moment, it was inaudible. Similarly the knowledge of the periodicity in the stimulus enabled the observer to concentrate attention at the probable appearance of the thresholds *To* and *Tu*.

In order to compare the work in which the second-wave is merely subjective with that in which it is also objective, Observer III. took a special test. The same apparatus was used as before but, instead of a sound varying about the threshold by actual change in intensity, the stimulus consisted of a liminal sound of constant intensity, and the observer recorded the subjective fluctuations by holding the key down while the sound was heard and free while the sound was not heard. In order to minimize the tendency to hallucination, a one-fifth second interrupter was substituted for the fork. A graphic record was taken by means of the multiple recorder.[1]

The experiment covered a period of two hours. From the facts learned in the foregoing experiments, it was evident that one intensity of sound would not remain liminal throughout that long period. Therefore we adopted the arbitrary method of raising or lowering the intensity of the sound by one step on the audiometer when the sound had been heard or not heard, respectively, for a continuous period of thirty seconds.

The experiment was made 3:17 to 5:17 p. m., April 14, '04. The introspective account follows:

I was in fairly good condition for afternoon work. Thought that probably I had remained at the same standard all the time because I was not aware of having had any periods long enough to call for the change. The subjective standard was retained satisfactorily throughout.

The wave seems to be dependent upon voluntary effort to a large extent. At times I would feel, 'now I have held it so long that I must give up in order to be able to continue.' Very many of the fluctuations are due to slight disturbances, both subjective and objective. The tendency to fall into an automatic rhythm is especially dangerous. For these reasons I do not place much significance upon the length of the waves. Yet the objective disturbances were only such as we notice when ordinary disturbances are excluded, and the rhythm is in part really what we seek.

[1] The recorder is described in *Univ. of Iowa Stud. in Psych.*, 1901, III., 1–16, as a part of the psychergograph. In its present form, fountain pens are used in place of the lead pencils, and an electric motor is used instead of the clock-work.

The heard sound varied within wide limits; at times it seemed as much as five points stronger than the barely perceptible, and I was able to notice distinct wavelike rises and falls in intensity. This experiment is more taxing on attention than the other experiments (Series I.).

Let us first observe the bearing of this special experiment upon the interpretation of the second-wave in the main experiment. Fig. 11 shows the numerical distribution of the different lengths of the attention-waves — the solid line for 'sound heard' and the dotted line for 'sound not heard.' The length is represented in seconds on the base-line and the vertical scale shows the number of cases at each level. The period for which the sound is heard most frequently is 6 seconds, and the period of greatest frequency for the sound not heard is 3 seconds. The curve rises at 30 because that point includes all that would

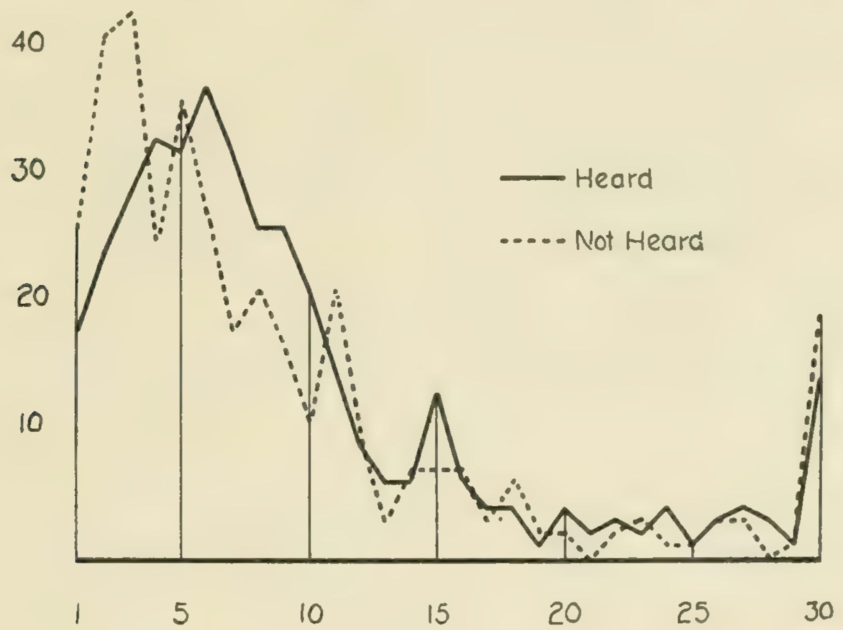


FIG. 11.

have been longer than 30 seconds if the standard had not been changed at that mark.

The most frequent length of the complete attention-wave in the special experiment is, therefore, about 9 seconds. The average duration of a complete act in the main experiment upon this observer (see Fig. III. in Plates I., II., and III.) was 8.4 seconds. That is, the typical attention-wave, purely subjective, coincides well with the objective wave in an act. In a way, the periods of supra-liminal sound correspond in the two experiments, so

that we may say with reference to both that the sound was heard during the attention-wave and not heard during the inattention wave, provided we count the threshold transition periods with the former.

This wave which coincides with the act is a wave of secondary passive (Titchener) attention.[1] Within it, we find two distinct waves of active attention. Such is the case both in the experiments in this series and in the classical experiments upon the attention-wave.

Since the average length of the act was 8.4 seconds for Obs. III., the length of each active attention-wave was about half of that, or 4.2 seconds, in accordance with the conditions of the experiment. It is safe to estimate that the period of effort occupied about 3 seconds out of the total, and the period of absence of effort the remaining 1.2 seconds.

The interpretation of the combination of the two forms of attention may be illustrated by the schematic diagram, Fig. 12. The scheme applies both to the special act in this series where there is an objective basis for the rhythm and to the familiar attention-wave where the rhythm is purely subjective. We shall apply the scheme to the former first.

The dotted line *ABC* represents the change in the intensity of the stimulus, and the horizontal base line the mean threshold value of the stimulus. Then *To* falls at *A* and *C*, and *Tu* at *B*. The curve *DEF* represents the form of distribution of the secondary passive attention, the part above the base-line indicating presence of this form of attention and the part below, absence. The curves *GHI* and *IJR* represent the form of distribution of the active attention-waves.[2]

The figure thus throws into clear perspective the result of the analysis of the complete attention-wave into its two component elements and suggests the general outline of the resultant of the two. There is a state of attention from *G* to *E*, but it differs in kind and strength, and the wave is not smooth, as

[1] For brevity, it will be spoken of hereafter as the passive, with the understanding that the *secondary* passive is meant.

[2] To coincide with the act as described in the main experiment, this diagram should really begin at *I* and make a complete cycle from that point instead of from *G*. No account is taken of the difference in the level of *To* and *Tu*.

has been supposed; it has three distinct prominences. The crests of the waves *GH* and *IJ* are the result of special effort, while the longer crest in *DE* represents no effort and yet a state of clear attention. But the three elevations are parts of a single phase of a long wave *GE*, because the attention is continuous during that period.

The period of inattention is short — only from *E* to *R*. But the period of absence of active attention does not coincide with the period of passive attention; they change off in part, as it were. The former runs from *J* to *R* and the latter from *E* to

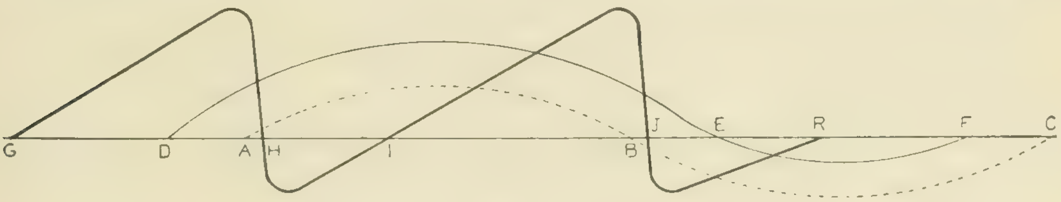


FIG. 12.

F. Therefore the rest from attention is not of uniform nature, any more than the attention was of uniform nature. From *B* to *C* no sound is heard; from *J* to *E* there is complete rest as regards active attention, but the passive lags; from *E* to *R* is a period of complete inattention; and from *R* to *F* only the passive attention is absent. Of course, the significance of these differences below the threshold must be interpreted in terms of the subconscious states.

Now, as has been suggested, the same analysis applies in a general way to the subjective attention-wave. To apply the scheme, we need to omit only the dotted curve, which represents change in the stimulus, and suppose that the stimulus is constant. The combination of the active and the passive attention-wave follows the same principle here as where the change is objective. It is easy to observe in the attention-wave experiment how the effort is exerted only at the point of coming in and the point of going out of the sensation; between these points the sensation holds the sway of consciousness and the clearness of the sensation during the middle period is in no way proportional to the effort of attention.

Have we not here discovered one of the secrets of endurance, a principle of economy and efficiency which applies to

all mental activity? Are not the two experiments here discussed — the one in which the change is objective and the one in which the change is only subjective — fundmental types of attentive consciousness? This most elementary periodicity is not peculiar to continuous work under pressure. It is a characteristic of the ordinary mental activity even if there be only a single act of a few seconds duration.

Observe its working in simple observation, sensory or logical pursuit, constructive imagination, reasoning, etc. — processes which require attention. Frequently, however, only the *To* period is present, there being no demand for *Tu*. Thus, in noticing whether a certain sensory stimulus is or is not present, there is a most effective spurt of active attention until we become aware of it (if it is perceivable) but, after that, it remains in consciousness for a moment although there may be no need of it, and there has been no objective strengthening of the stimulus. This is true not only of liminal stimuli but of stimuli of any strength which need to be selected by an effort of attention. The effort which lands the impression in consciousness is momentary and intense but the continuation of the impression in consciousness in its original, or even increasing clearness, is due to an after-beat, a pulsation of the secondary passive attention-wave.

The longer waves in this special experiment also deserve a passing notice. Fig. 13 represents the changes made in the standard during the two-hour period according to the prearrangement mentioned above. It is a crude way of representing the minute-waves and the hour-waves. The numbers at the left refer to the audiometer scale; and those at the base denote the time in minutes. A comparison of this figure with Fig. III. in Pl. I. and III. reveals a striking agreement of the two records. It is especially noticeable in the long hour-wave which starts with a high crest and then spreads over a long basin and finally rises again. This demonstration of the hour-waves proves that they are not peculiar to the kind of work done in the main experiment.

Progressive Change.

Next to the periodic change in these records, our interest centers on the question of progressive change. For the purpose of demonstrating any progressive tendencies which may be present in the record as a whole, the results of the two halves of each record are arranged for comparison in Table II.

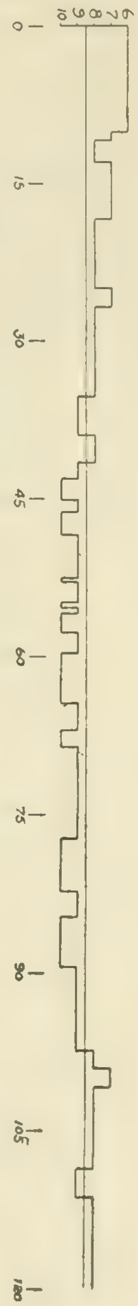


FIG. 13.

The first column shows the average *To* for the first half of each record, and the second shows the average mean variation of these on the basis of groups of ten. The third and fourth columns show the same for *Tu*. The fifth shows the width of the threshold — *To* minus *Tu*. The next five columns contain the corresponding facts for the second half. The eleventh and the twelfth columns give the differences between the two halves, the plus sign indicating loss and the minus sign gain in sensibility. The thirteenth column shows the difference in the width of the threshold for the two halves. The fourteenth shows the range of variation in *To*, *i. e.*, the difference between the highest and the lowest points in a record on the basis of one hundred acts as a unit. The fifteenth column shows the range of variation in the width of the threshold on the same basis.

In respect to sensibility, or height of the threshold (Col's 11, 12), the records may be divided into three classes: those which indicate gain (I., II., IV.); those which indicate loss (VI., VII., VIII., IX.); and, those which indicate no decided gain or loss in sensibility (V., X.). For fuller interpretation, the form of each curve should be taken into consideration. In Record III., *e. g.*, there is a progressive loss during the first two thirds of the period and a gain in the last third. It is certain that there is no general tendency in favor of loss or gain. The .5 (Col. 11) balance in favor of loss in *To* is only twelve per cent. of the range of variation in *To* (Col. 15) and is negligible; the corresponding balance in *Tu* is only .1. To what extent we may regard the records as revealing types of observers, remains to be demonstrated.

Table II.

Comparison of the First and the Second Half.

	First half.					Second half.					*To* 2d half minus *To* 1st half.	*Tu* 2d half minus *Tu* 1st half.	Width 2d half minus Width 1st half.	Range of var. of *To*.	Range of var. in Thr. Width
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Exp.	*To*	*m. v.*	*Tu*	*m. v.*	width	*To*	*m. v.*	*Tu*	*m. v.*	width					
I.	12.6	.7	9.5	.7	3.1	11.6	.6	8.7	.6	2.9	−1.0	− .8	− .2	3.9	.9
II.	16.8	.6	14.6	.6	2.2	15.1	.6	12.8	.6	2.3	−1.7	−1.8	+ .1	4.5	.5
III.	19.8	.6	17.1	.6	2.7	21.8	.5	18.7	.7	3.1	+2.0	+1.6	+ .4	6.9	1.1
IV.	20.3	.4	18.5	.4	1.8	19.6	.4	17.3	.5	2.3	− .7	−1.2	+ .5	3.6	1.1
V.	17.1	.6	14.9	.6	2.2	16.7	.4	15.3	.5	1.4	− .4	+ .4	− .8	6.2	1.7
VI.	11.4	.5	8.8	.5	2.6	12.3	.6	9.5	.7	2.8	+ .9	+ .7	+ .2	3.9	.8
VII.	25.4	.5	21.9	.4	3.5	26.1	.6	22.0	.7	4.1	+ .7	+ .1	+ .6	1.9	1.3
VIII.	16.0	.5	13.6	.4	2.4	17.6	.5	14.8	.4	2.8	+1.6	+1.2	+ .4	3.3	1.1
IX.	17.6	.9	13.8	.9	3.8	20.4	1.0	15.7	1.1	4.7	+2.8	+1.9	+ .9	4.9	1.3
X.	21.3	.7	18.5	.7	2.8	21.7	.7	17.1	.9	4.6	+ .4	−1.4	+1.8	3.2	3.1
Ave.	17.8	.6	15.1	.6	2.7	18.3	.6	15.2	.7	3.1	+ .5	+ .1	+ .4	4.2	1.3

In respect to mean variation there is still less evidence of progressive change (Col's 2, 7; 4, 9). In Records I. and V., the mean variation is slightly smaller in the second half than in the first, *i. e.*, the records tend to improve in regularity; and, in Records IV., VII. and IX., it is larger in the second half, but in no case is the difference very great. In five records (I., III., VI., VIII., X.), and in the average for the ten records the mean variation is practically equal for the two halves.

There is a more decided progressive tendency in respect to the width of the threshold. Eight records show an increase in width in the second half as compared with the first (Col's 5, 10, 13), and the average increase is .4, which is thirty-one per cent. of the average variation in width (Col. 15). In the two records which show a decrease in width, the change is very small in Record I. and, in Record V., it is partially explained by the introspective record.[1]

[1] According to the introspective account the exceptionally large increase of width in Record X. is accounted for in part as due to a change of standard of certainty. The uniformity in Record II. is due in part to a conscious effort to avoid the widening of the threshold.

Correlation of Changes.

In retrospect, we may review the three kinds of changes with reference to periodic changes, progressive changes, and the correlations of the three factors, by means of the juxtapositions drawn in bold outline in Pl. III. and the table of correlations, Table III.

Pl. III. contains outline reproductions of the ten records. The curves are drawn on the basis of averages for one hundred acts for each point and represent *To*, *m. v.*, and threshold width. For the present purpose, *Tu* would be similar to *To* which is used. The *m. v.* here used is the mean between the

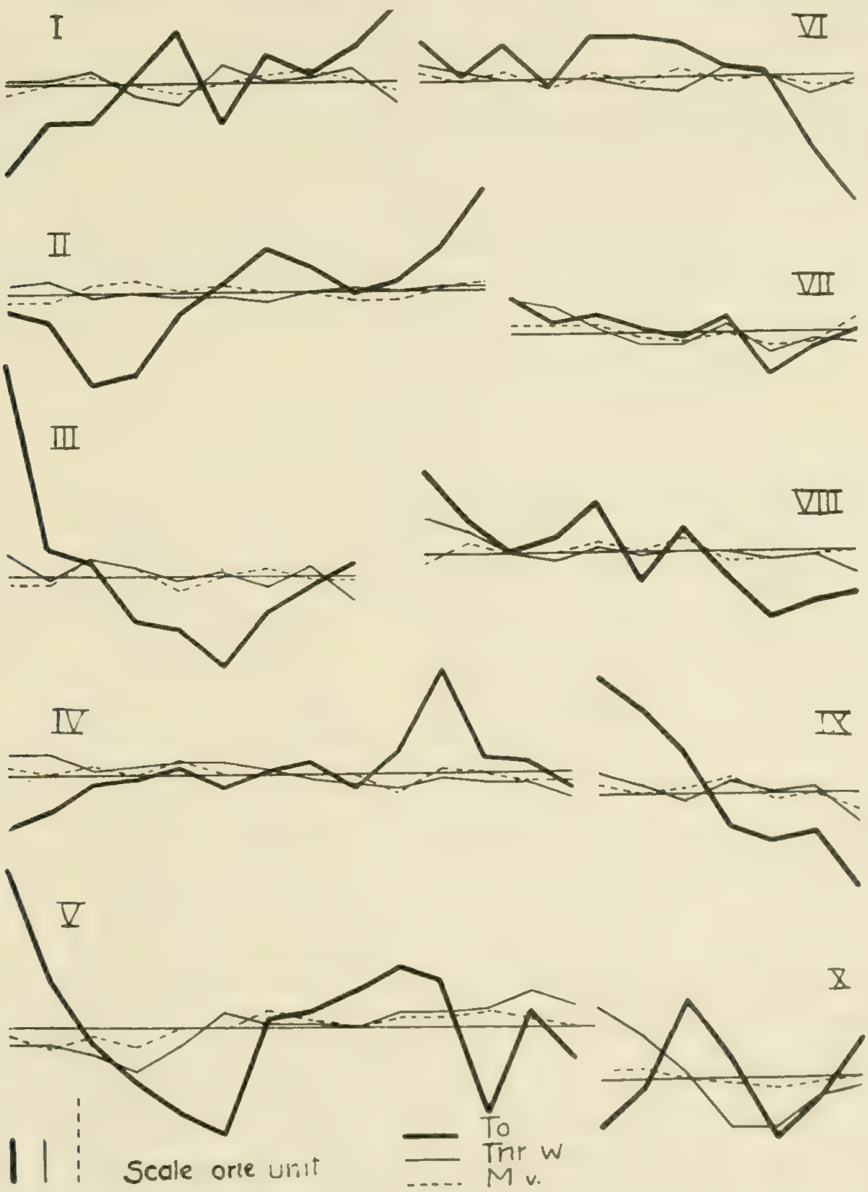


PLATE III.

average mean variation for *To* and the average mean variation for *Tu*. The ordinate scale is twice as large for the *m. v.* as for the other two curves. The horizontal line is a common base and represents the averages of the respective factors for the whole record.

The same data on which the figures in Pl. III. are based are treated by Pearson's formula of correlations. The results are presented in Table III.

For the purpose of this correlation, a point — *i. e.*, the average of one hundred acts, following the grouping indicated by the heavy horizontal bars in Pl. I. — is considered high or low as follows: for *To*, a point lying above the average for the whole record — *i. e.*, indicated by a smaller number than the average in the record of hearing — is considered high; for *m. v.*, a point represented by a smaller number than the average for the whole record is considered high; and, for threshold width, a threshold narrower than the average is considered high. Agreement of high with high, or low with low points is represented by the plus sign; disagreement by the minus sign. *n* denotes the number of points considered.

Table III.

Correlations of *To*, *m. v.*, and Threshold Width.

	n.	Correlation between *To* and width.	Probable error.	Correlation between *To* and *m. v.*	Probable error.	Correlation between width and *m. v.*	Probable error.
I.	10	−.64	.11	−.09	.21	+.54	.13
II.	12	+.05	.19	+.12	.19	+.49	.13
III.	9	+.40	.15	−.26	.20	+.25	.20
IV.	14	−.37	.14	−.06	.18	+.65	.09
V.	14	−.04	.18	−.12	.17	+.75	.06
VI.	11	+.31	.17	+.60	.11	+.34	.17
VII.	9	+.79	.07	+.86	.06	+.55	.14
VIII.	11	+.79	.06	+.36	.17	+.02	.20
IX.	7	+.62	.13	+.47	.18	+.79	.07
X.	7	−.10	.25	+.04	.25	+.90	.04

The column for *To* and threshold width shows six positive and four negative cases. This does not prove that no correlation exists, but rather points to the existence of radically different types of method in working. This view is borne out by intimate knowledge of the conditions of the experiment and by the introspective observations.

The columns for *To* and *m. v.* show a similar divergence, although there is a stronger preponderance in favor of a positive correlation.

There is a strong correlation between the *m. v.* and the threshold width. This probably means that *m. v.* (variability) and width (alertness) are signs of the same general central efficiency. If the *To* (sensibility), had remained constant in the record of a period, the decline in the central process would indicate that there was an increase in the peripheral efficiency. But in view of the conflicting types of the records we dare not draw the conclusion that there is a progressive increase in peripheral sensitiveness.

'*Before*'- and '*After*'-*Tests.*

The before- and after-tests were intended to show what effect, if any, the long test had upon the ear not used. A marked change in the relation of the sensibility of the two ears would indicate that a peripheral change had taken place in the ear used.

TABLE IV.

THE 'AFTER'- AND THE 'BEFORE'-TESTS.

	After as Compared with Before.				Ear Used as Compared with Average of Long Test.				Before-Test. Ear Used, as Compared with First 100 Reactions of Long Test.		After-Test. Ear Used, as Compared with Last 100 Reactions of Long Test.	
	Ear Used.		Ear not Used.		Before.		After.					
	To	*Tu*	*To*	*Tu*	*To*	*Tu*	*To*	*Tu*	*To*	*Tu*	*To*	*Tu*
I.	—10.0	—10.3	—4.6	—4.3	+10.9	+11.4	+ .9	+1.2	+8.8	+9.2	—3.3	—3.4
II.	— .7	— .3	—4.0	—4.6	— .9	— .7	—1.6	—1.0	—1.3	—1.2	— .1	— .2
IV.	— 2.9	— 3.3	—6.1	—7.0	+ 3	+ 2.9	— .2	— .4	+ .8	+1.1	— .3	— .3
V.	— 3.5	+ 3.9	—3.6	—3.1	— 4.5	— 4.7	—1.0	— .8	— .8	— .6	—1.8	—2.1
VI.	— 4.8	— 4.4	+ .8	— .4	+ 6.4	+ 6.2	+1.6	+1.8	—7.4	+6.8	—1.3	+1.0
VII.	.3	— .8	+ .7	— .6	— 1.8	— .4	—1.5	—1.2	— .9	— .3	—1.4	— .8
VIII.	— 3.7	— 5.1	+1.7	+1.4	0	— 1.1	+3.8	+4.0	—1.9	— .1	+2.8	— 3.6
IX.	— .4	— 1.8	—4.6	—5.8	— 1.1	— .1	—2.4	— 1.9	+1.6	+2.1	—3.3	—2
Average.	— 1.4	— 1.5	—2.2	—3.1	— 1.5	— 1.7	0	— .2	+2.1	+2.2	— .8	— .5
Median.	— 3.5	— 1.3	—2.6	—2.2	— .5	— .3	— .4	— .6	—1.2	+ .6	— .9	— .8

To supplement the graphic records in Pl. I., the significant features of the results are exhibited in Table IV. Here a minus sign indicates 'keener sensibility' and a plus sign the opposite.

The after-test as compared with the before-test shows a gain in sensibility, if we consider the average or the median for

the eight records, and the gain is practically equal for the two ears. But, in view of the radical divergences in the records, very little significance can be attached to the average or median.

The average and median for the ear used as compared with the average of the long test show an approximately equal distribution above and below that average, both in the before- and the after-tests, although the deviations on both sides are large.

There is a closer agreement between the end-tests and the respective adjacent ends of the long record; there is a slight tendency for the before-tests to be inferior to the first hundred acts of the long record, and for the after-test to be slightly superior to the last one hundred acts in the long record. This shows how the sensibility in the end-tests depends upon what portion of the hour-wave such a test is taken in.

There are special explanations for some of these divergencies. The receiver was not tied on, but was held to the ear by the hand, and slight changes in the adjustment would cause differences in the intensity of the sound. The pain from continued pressure may have influenced some observers in the adjustment of the receiver for the after-test. The subjective conditions of the end-tests as compared with the main experiment, and of the end-tests as compared with each other, seemed to influence different individuals in different ways. Thus, in the before-tests, the initial impetus of volition came in to the greatest advantage; in the main experiment, the calm adaptation to a uniform act was effective; and, in the after-test, the heightened irritability and the sense of opportunity for a final spurt played significant rôles. We had hoped to eliminate these and many similar sources of error by taking the test on both ears, both at the beginning and the end, and by making the end-tests of the same nature as the main experiment. But the data obtained are significant chiefly in pointing to individual differences and complexities of conditions. Three[1] of the records (I., VI., VIII.) show a decidedly greater gain in the ear used, one (VII.) shows neither gain nor loss in either ear, and four

[1] To these may be added a fourth, taken on Observer III., but not included in the table because it was taken under somewhat different conditions.

(II., IV., V., IX.) show a decidedly greater gain in the ear not used.

Introspective Accounts.

A general view of the experiences in a period, especially the difficulties and sources of error, may be obtained in part from the introspective accounts. In the following extracts from the accounts which were written by the observer immediately after the experiment, the language of the observer is used, but irrelevant material is cut out and much is abridged.

I. (D. S.) 10:25 a. m., April 16, '04.

The only disturbances that were noticed in this experiment were due to my changing position on the chair. * * * This occurred three or four times. It is my impression that the threshold was about the same at the end of the record as at the beginning, for the reason that I felt scarcely any fatigue from the work. There were places about the middle of the record where there was a tendency to become inattentive and sleepy. There was also a strong tendency throughout the entire period to react rhythmically.

II. (G. H. K.) 9:54 a. m., April 21, '04.

The observing-room was cold, owing to a mistake in the ventilating. A subjective sound was heard all the time, especially at first; this was very confusing. I expect to find great irregularities in the record. The duration of the sound varied greatly. There were several drowsy periods during which there was a tendency to fall into rhythmic action. The time seemed long. During the last half hour I felt much discouraged. I had visual imagery of what was going on in the recording-room. My imagination was very active the whole time.

III. (C. E. S.) 9:54 a. m., April 12, '04.

Only light work before the experiment. Air good, and a good day in general. The only discomfort I suffered was in holding the receiver. It should be tied on both to avoid fatigue and to secure constant adjustment. Slight movements of the receiver cause both qualitative and intensive changes in the sound.

I suffered no serious mental strain, still I found that I held my mouth open all the time so that my throat felt parched at the end of the experiment. I did not have any sleepy spell. There was nothing particularly wearying in the process. The quiet and darkness of the room are so soothing, the stimuli are so delicate and graceful, and the feelings of expectancy are so generally satisfied that I experienced an agreeable complaceny and comfortable adaptation as the experiment progressed. I did not get tired and felt no relief from the change at the end.

The duration of the sound seems to fit my attention-wave nicely. The gradual rise and fall of intensity led me to image a combined auditory, visual, and motor wave which was decidedly pleasing and had great carrying power. Any interruption in this wave was disturbing, but such disturbances sometimes served to make me more alert. There was a tendency for me to shorten the period in the rhythm and 'rush' the experimenter, and I had to break away from that periodically.

The experiment is conducive to mind-wandering. The noticing of the *To* takes but a small fraction of the time and then one soon learns to estimate the time for the *Tu* from the strength of the sound immediately after the response to the *To* so that there is a freedom from suspense which really should be present.

IV. (C. P. S.) 1:32 p. m., April 26, '04.

(The account of this observer gives a vivid description of the characteristic experience during a period, and is, therefore, inserted in full.)

My physical condition was not of the best, for a severe cold made it impossible for me to breathe through my nose, consequently the whole of the mucous membrane of my mouth and throat became exceedingly dry and parched, necessitating a considerable degree of effort and swallowing to moisten it. Invariably these muscular efforts of my throat and tongue made my sense of hearing seem very much less keen, and on nearly all occasions I was compelled to give the 'objective' signal. This condition was to be noticed more during what might be judged to be the first half of the period than during the latter half; but was nevertheless a frequent factor to be dealt with.

The first portion of the period seemed on account of its novelty and my comfortable position to pass rapidly, and to be full of interest. My attention turned naturally to the experiment in hand, and I felt that I was making a splendid record, for the intervals between my responses were very short. However, as the comfort of my position decreased, and the necessity for changing the position of my limbs and body grew, my attention waned also, until I was suddenly called back to the matter in hand by what seemed an unusually loud and prolonged sound in the receiver. I felt such a time to be proper for a change in position, which I made, at the same time giving the signal. Immediately thereafter I again became conscious of a greater degree of attention.

During about the middle portion of the period, and again later, an element of disturbance arose; namely, the penetration of sound caused by someone walking in the nearby corridor. This distracted me considerably and I gave the objective signal, after which my degree of attention again increased.

A desire to leave my position and stretch body and limbs became almost irresistable during the latter portion of the period; but the belief that the period might be nearly over kept me to my task. A further desire to know how much time had passed returned repeatedly; but this was thrust away by the argument that if I turned on the light and consulted my watch, my attention would be completely distracted, if for only a moment, and the validity of the experiment impaired. This train of thought and the first desire mentioned both contributed to my lack of success in quickly discriminating between silence and sound.

A frequent desire for deep inhalations of breadth, something like yawning came over me, and my yielding to the desire was the cause for several of my signals.

One thing I noticed at intervals throughout the experiment was the loud and violent beating of my heart. This usually followed some change of position and was quite a disturbance. I was most conscious of it when giving almost breathless attention to the receiver. Pulsation of the blood in my temples was also almost sufficient to drown the fainter sounds in the receiver.

Toward the end of the experiment I became conscious of a considerable pressure, almost equal to pain, in the pinna of my ear, caused by the receiver.

At one time a train of thought about my foot-racing started in my mind and I experienced the same violent throbbing of the heart and tingling of the nerves which I experienced just before every athletic contest. This was another source of disturbance, but concentration of attention was sufficient to cause it to disappear as quickly as it came.

The long sound in the receiver which indicated the end of the experiment proper caught me in a perfectly passive condition, responding automatically to the stimuli. I received it, however, with considerable relief, and yet with a certain reluctance for which I cannot account.

So far as I can tell, there was no difference in the preliminary test in the hearing ability of either ear. In the final test I noticed no difference in the relative hearing ability of the left ear as compared with the preliminary test or as compared with the experiment proper. When testing the right ear, however, the first few tests were very poor on account of an improper adjustment of the receiver. With the assistance of the left hand I then held this in a better position, responding with the right hand. The strain of the awkward position of the left arm made me very attentive to it as well as to the receiver which it supported ; and I found my hearing ability in this ear so far as I could tell, to be better than that of the left.

When the experiment was over I felt as though I had just finished a period of severe study. No great degree of physical fatigue was noticeable. At no period during the experiment was I able to judge of the amount of time which had passed.

During the first part of the period I sat with my eyes wide open staring into the dark. Many flashes of colored light were visible. Later I sat with my eyes shut because of fatigue in my eye-lids and the distraction caused by winking. With my eyes closed I also observed the many colored lights.

V. (M. B. C.) 9:35 a. m., April 15, '04.

I was in good physical condition. The period was quite free from disturbances, except such as came from slight changes in position. During the first third, I was conscious of my breathing and of both mental and physical strain. During the second third I was conscious of fatigue of both mind and body, and of mind-wandering. Expected the experiment to end. During the last third there was a sudden sense of relaxation and ease, mentally and physically. With the exception of pain from the pressing receiver, this period was the most comfortable and I felt a keener interest in the test than before ; but I was aware of making many mistakes. During the whole experiment I had about five or six distinct cases of mind-wandering.

VI. (M. C.) 8:42 a. m., April 19, '04.

For the first fifteen minutes or half hour, the test seemed entirely pleasant and I seemed to be in a sort of a dream. This part of the record is probably best. After that my head began to ache on account of the bandage, and that distracted my attention somewhat. During the entire test I could notice that my attention would be exerted in waves and it seemed that after a slight movement of the head the record would improve for a time. The headache made the test seem extremely long and tiresome. I began to anticipate the end of the test at what I should judge was about the middle of it. Early in the test, before it became unpleasant, I caught myself falling asleep although I did not

seem to be drowsy. The quality of the sound seemed to change from time to time. There also seemed to be a difference in pitch and the higher sounds were much easier to hear.

VII. (R. E. K.) 1:33 p. m., April 25, '04.

During the first half hour it was easy to keep the attention on the work and I think my keenness of perception was gradually increasing without any marked rise or fall. The next half hour was made up of irregular rises and falls, and it was harder to focus attention, due in part to the discomfort caused by the receiver and by the limitation of movements. It was easier to give attention to the work, after varying the position, *e. g.*, from the erect position to leaning on the table. Gradually I became drowsy and 'came to' with a start thinking that I had neglected to respond; this tended to focus my attention upon the sound for a while. These periods were quite short and were followed by drowsiness. At about the beginning of the last half hour, or twenty minutes, I succeeded in rousing myself. I think the record was gradually growing better at the end of the experiment. I am sure that my threshold was lower at the end than at the beginning of the experiment. During the last half hour it was not difficult to give all my attention to the work; no thought was given to time as was the case in the middle of the experiment.

VIII. (O. H.) 9:35 a. m., April 18, '04.

During the first ten or fifteen minutes of the experiment, the pulse beat was perceptible in the head. Perhaps this was due to the band which held the receiver against the ear. About the end of the first twenty minutes I experienced, but only for a moment, a peculiar lack of sensibility or a numbness all over the body. The experiment seemed long. For a few minutes near the end of the experiment I was disturbed by continual swallowing of saliva. Part of the time the experiment seemed rhythmical. I felt weary from continuing in the same position.

IX. (C. G.) 1:31 p. m., April 20, '04.

I was in good physical and mental condition. Felt sleepy about three times. Shifted the receiver twice because it hurt the ear, but do not think that that caused much disturbance. The time seemed long.

X. (A. W.) 10:17 a. m., April 9, '04.

I found the sounds of my own body, breathing, etc., somewhat of a disturbance at first. It was often necessary to take a long breath to catch up. My arm and hand holding the receiver went to sleep. A queer feeling was also felt in the other hand. At first I think I gave the signal for the disappearance of the sound before such was really the case, because I was listening for a certain quality of sound. About twenty or twenty-five minutes before the close of the experiment I got very drowsy and was conscious of responding almost automatically, and here I am sure that I listened for a certain sound — not merely sound — and signalled when this particular sound appeared and disappeared. After this I aroused myself and found that I could distinguish the beginning of sound which was much fainter than the 'certain' sound I had heard before.

The time seemed short, perhaps two-thirds as long as it actually was. I had a distinct feeling of aloneness when the sound ceased and a mental image

of the sound was present all the time. Sometimes I pressed the key to keep out the sound, which was then more like a presence than a mere sound.

Series II. Discrimination.

Problem, Apparatus, Method, and Observers.

The same plan as was pursued in the study of sensibility in Series I. was here pursued in the study of discrimination. Experiments consisting of an uninterrupted series of determinations of the sensible discrimination were carried on for two-hour periods. The act remained uniform throughout, accessory conditions were kept as constant as possible, and the observer was expected to exert the maximum effort in attention to the act.

The act studied consisted in *determining whether the second of two consecutive sounds, which differed in intensity only, was stronger or weaker than the first.* The same apparatus was used as in Series I. and the conditions of the observer were also similar. The threshold of hearing was first determined and then a point about ten units above that was made the standard. In minor details three different methods were used.

In the first two experiments, Records I. and II., the sound was started at the standard and sounded two seconds at this and at each successive point, the movement being either up or down, and the observer gave a signal as soon as he knew whether it was weaker or stronger than at the beginning. Two signal keys were used: pressing the right hand key indicated a stronger and, the left hand, a weaker sound. Immediately after the response the experimenter cut off the sound for an instant while sliding the carrier back to the standard. This interruption served as a signal for the beginning of the next act. An assistant recorded the responses and the amount of change required for each successive act of discrimination, as indicated by the audiometer.

The next three experiments, Records III., IV., V., differed from the first in the following respects. The sound was held at the standard approximately five seconds, instead of two as before, and the rate of change was not perfectly uniform because the experimenter did not use the metronome. The response was made by one key, one tap denoting stronger and two

taps weaker. In experiments III. and V. a time signal was given to the observer every fifteen minutes.

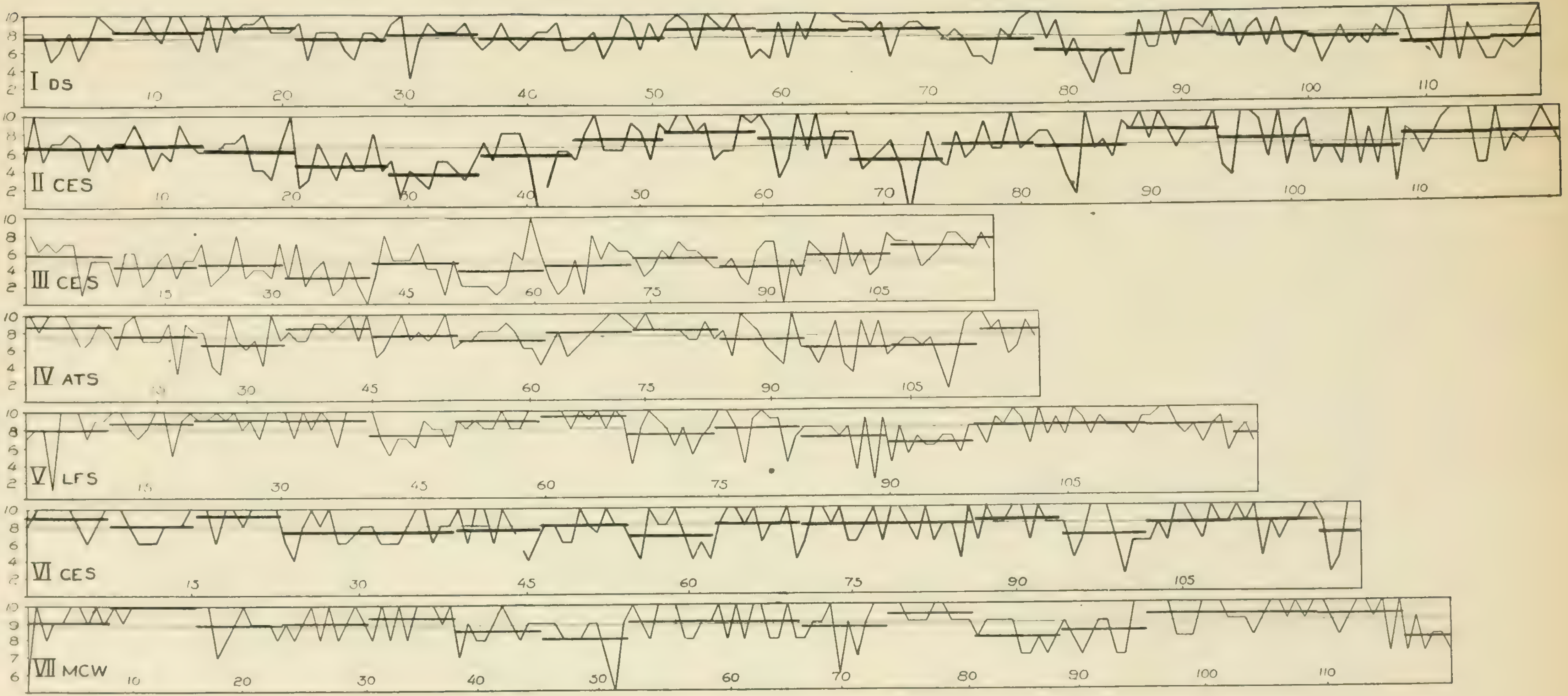
The results of these five experiments are represented graphically in Plate IV., Fig's I. to V. As it turned out that more than half of the responses were made for a single step of change, it was found practicable to use the number of correct responses in a single step of change as a measure of the efficiency. Since nearly all the responses which were not made for one-step changes were made for two-step changes, the method reduces itself to the tabulation of the number of correct responses on the smaller step. But this makes it necessary to deal somewhat arbitrarily with the incorrect responses on that step. Since the chances were equal that a response would be right or wrong, one correct response was deducted from the total with every error; thus, if there were eight correct responses and one wrong, the final record would read, 70 per cent. right. The zigzag line shows the average number of correct responses, on this basis, for each successive ten acts. The short horizontal lines denote averages for a hundred acts each. The long horizontal line shows the average for the whole record. The time is marked in minutes on the base-line.

Experiments VI. and VII. were made by the method of right and wrong cases. A metronome with relay was introduced into the circuit in such a way that the sound was cut out for a second every alternate second. Either the standard or a compared tone, perceptibly different in strength, could be sounded at the will of the experimenter; but it was agreed that the standard should never be sounded more than twice in succession and the compared sound not more than once at a time. The experimenter made the change by sliding the carrier while the sound was interrupted.[1] The observer was required to signal every time the compared sound was heard.

The carrier on the audiometer (to show whether the standard or the compared sound was given), the metronome (to furnish a time-line and to indicate the duration of each sound) and the observer's signal key (to register the signal) were con-

[1] The difference was three steps on the audiometer in experiment VI., and two steps in experiment VII.; and the compared sound was weaker than the standard in the former and stronger in the latter.

PLATE IV.



nected with corresponding pens in the multiple recorder[1] by means of which a continuous tracing was obtained showing the correctness or error of each response, and, in case of errors, the nature of the error.

Two kinds of errors occurred: signals on the wrong sound, and failures to signal on the right. In counting points for the graphic record, the correct responses and these two kinds of errors were taken into consideration, but the errors were not differentiated. This method leaves out of count all those cases in which the standard was sounded and the signal properly withheld. The results of these two experiments are represented in Pl. IV., Fig's VI. and VII. The curves show the per cent. of correct responses on this basis. The range is from 50 per cent. to 100 per cent. The zigzag shows the grouping by tens and the short bars by hundreds, as before.

Periodic Changes.

The interpretation of these records is simple, after we are familiar with Series I. We look for periodic changes and progressive changes.

The hour-wave is prominent in all the records. Its general outline can be traced most readily by following the trend of the short horizontal lines, *i. e.*, the averages by hundreds. In this series they appear both more distinct and more uniform than in the foregoing series.

Record III. shows a close agreement with records III. and XIII. on the same observer in the foregoing series, both in the long hour-wave of which there is only one phase, and in the shorter hour wave. These three records taken together indicate a characteristic individual wave series; but records II. and VI. which are on the same observer agree with these only in the shorter hour-wave.

The minute-waves are also very prominent and there is a striking similarity in the records. But the present method is not entirely adapted to bring out detail in these short waves.

Here, as in the foregoing series, there is a strong tendency for the second-wave to coincide with the individual act. The

[1] See reference, p. 60.

time was suitable, and the new effort made at each beginning on the standard was conducive to the adjustment of the attention-wave to the duration of the act.

The distraction of introspection was avoided, as far as possible, in the regular experiment. But afterward, some experiments were repeated for the purpose of determining by introspection whether the combination of active and passive attention follows the same principle in the act of discrimination as in the act of simple perception.

We may illustrate the conclusion by reference to the simplest form of comparison in discrimination. In Fig. 14, let the dotted figures *AB* and *CD* represent two successive tones. The dura-

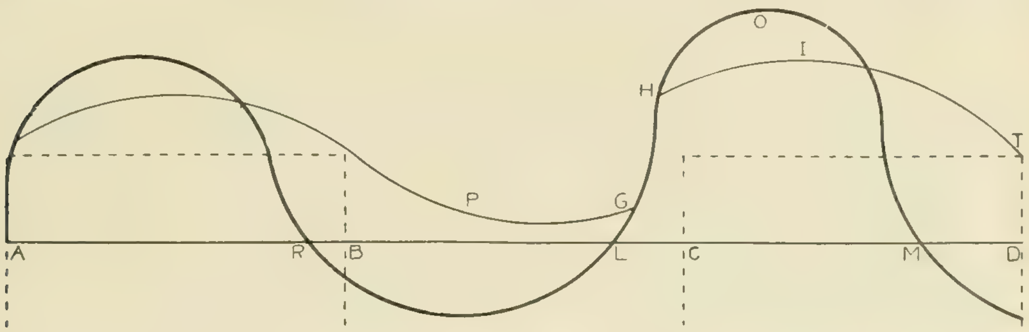


FIG. 14.

tion of each tone is one second and the interval between them is one second. The act consists in determining whether the two tones are equal or different in intensity.

The general form of the attention-wave in which the whole act is comprehended may be outlined by the composite curve *ANPOJ*.[1] But this may be reduced to its component elements; namely, the secondary passive wave *APGIHJ*, and the two active attention-waves *ANR* and *LOM*. That is, the combination follows the same principle as was illustrated for sensibility in Fig. 12, p. 63: the act is performed largely by secondary passive attention, but at the critical points, short intensive waves of active attention occur.

This illustration presupposes a trained observer performing a familiar act under the conditions named. The proportions of the components would vary with observers and conditions, but

[1] *N* was cut out from the etching inadvertantly; it represents the first active attention-crest and is symmetrical with *O*.

certain relative features are rather fundamental. (1) The two stimuli and the memory image of the former are grasped in a single wave of attention. (2) Active attention comes into prominence only at the critical moments. It does not cover more than the first part of each stimulus and the end of the memory image. (3) Both forms of attention rise higher for the last than for the first stimulus. There is also a wider scope of attention at the beginning of the second stimulus but this cannot be represented in the plane figure. (4) The early rise of the second active attention-wave is due to the effort to grasp the passing memory image and to take advantage of the sharp edge of the appearing stimulus. The rise of the passive wave at the same point is due in part to the feeling of suspense.

Progressive Change.

To determine the presence of progressive change roughly, the averages for the first and the second half of each record may be compared. Records II. and III. show a rise of 6 per cent. each, but in the latter the rise is accounted for approximately by the special impetus given for a final spurt by a time signal; and in the former, the absence of the usual high beginning for this observer is conspicuous. Five records show a decline in ability as indicated by the following per cents. respectively: I., 8 per cent.; IV., 3 per cent.; V., 10 per cent.; VI., 1 per cent.; and VII., 2 per cent. On the whole, therefore, there is a tendency toward decline in ability in this period of work.

Notes from the Introspective Accounts.[1]

I. (D. S.) 9:30 a. m., June 8, '04.

I was in good condition for the test. Felt drowsy for three-fourths of an hour about the middle of the experiment. I also felt drowsy for a quarter of an hour after the experiment. The duration of the sound at the standard seemed to vary; this was disturbing.

III. (C. E. S.) 3:10 p. m., June 4, '03.

Good physical and mental condition. In the fourth quarter I had difficulty in keeping awake. Each quarter-hour signal aroused me to a keener discrimination. The signal gave a feeling of relief. I often moved slightly and the change of position broke the monotony. The absence of light was exceedingly soothing.

[1] Abridged as in Series I.

There was no sense of increased fatigue during the second hour. In fact I felt brighter in the second hour than in the first, especially in the approach to the end.

The errors are due to various reasons. Among the objective, the following may figure : the uncertainty of the tap on the key, movements of the receiver, and irregularities in the time of the stimulus. Among the subjective factors, are, mind-wandering, failure to remember the standard, and hesitation in reaction.

The observer felt no after-effect of the experiment until 9 p. m. Then he felt an unusual pain above the eyes and a more decided feeling of general exhaustion than usual. There are no means of knowing whether these effects were or were not due to the experiment.

IV. (A. T. S.), 2 :45 p. m., June 3, '03.

The period seemed short. I felt sleepy once and caught myself distinctly mind-wandering twice. I enjoyed the experiment and took special pleasure in the opportunity to do my best.

V. (L. F. S.), 3 :35 p. m., June 3, '03.

I felt as if I had swayed backward and forward for fifteen seconds. In the last quarter, or half hour, I gritted my teeth as if I had been running an engine down grade. Do not feel tired.

VI. (C. E. S.), 2 :33 p. m., April 7, '04.

The experiment began immediately after an afternoon lecture from which I felt tired. The step seemed too large throughout. There was no considerable period during which I felt any distinct incapacity for discriminating. The mistakes seemed to come singly, and most of them could be traced to some temporary inattention, movement, distraction, or lapse into rhythm. I was quite comfortable all the time. The darkness and silence of the room were distinctly restful and quieting. I felt a distinct relief from the strain of the lecture room and from stimulation of the eyes, and thought that this room would be an ideal resting place.

The work was not exhausting. There was a distinct tendency to automatism. It was possible to carry on a process of reasoning without making any mistakes in the discrimination. (?) It may be that the tendency to perceive rhythmic accentuation (intensification) led me to think that I was right when I was not.

The time seemed to drag, *i. e.*, I kept accelerating and had to realize from time to time that my subjective standard of time had changed. I was sleepy once, at the end of the first fifteen minutes.

I had a distinct and helpful pitch association. The strong tone seemed to be the fundamental (do) and the faint seemed to be the fifth of the octave below it (sol). I frequently judged entirely by this pitch difference, which was probably due to the prominence of the first overtone in the stronger sound.

VII. (M. C. W.), 3 :40 p. m., June 11, '03.

The quality of the tone varied. The time passed quickly, and I did not feel tired at the end. I had short lapses of attention which seemed to serve as periods of rest, but I exerted a strong effort all the time. During the first fifteen

minutes I felt dizzy. There was a strong space association, the weaker sound being localized further away.

Series III. Memory.

Problem, Apparatus, and Method.

The following requirements were kept in mind in the planning of this test: (1) The work shall consist of a difficult act of memory which shall be repeatable under uniform conditions, without interruption, for any desired length of time. (2) There shall be only one variable element in the complex act; all variable associations and disturbing sensory stimuli shall be eliminated as far as possible and the motor process shall be reduced to a minimum and uniform act. (3) All elements in the complete act of memory, namely, impression, retention, reproduction, localization, and expression, shall be involved in the same way at every step. (4) The work shall be the result of continuous maximum effort. (5) A detailed record of efficiency shall be obtainable.

With these ends in view, the following act of memory was chosen: *Given four clearly distinguishable intensities of the same tone in succession, to signal the order of succession in a group after the order in the next following group has been observed.*

The psychological relations of the intensities of the tones may be represented by the relations of these lines:

Number 4 was so faint that it could just be heard distinctly, 1 was as strong as it could be without being disagreeable to the ear, and 2 and 3 were adjusted between these limits empirically in such a ratio that the steps 1–2, 2–3, and 3–4 were equally perceptible.

The procedure may be illustrated from the beginning of an experiment by giving the part of the observer, as follows:

Receives the first group, *e. g.*, 2 1 3 4.
Receives the second group, *e. g.*, 3 2 1 4.
Reproduces the first group, 2 1 3 4.
Receives the third group, *e. g.*, 2 4 1 3.
Reproduces the second group, 3 2 1 4.
Receives the fourth group, *e. g.*, 4 1 3 2.
Reproduces the third group, 2 4 1 3.
Receives the fifth group, *e. g.*, 1 4 2 3.
Reproduces the fourth group, 4 1 3 2. Etc.

Thus the same act, namely, *observing the order of four sounds in a group and reproducing it after another group has been observed*, could be repeated for any length of time without serious change in the setting or relative value of the elements in the group. The selection of this particular act made it possible to comply approximately with all the five requirements enumerated above.

The sounds were produced through a telephone receiver in the secondary circuit of an inductorium, the primary circuit of which was completed as a shunt around a 100 v. d. electric tuning-fork.

A system of four open-circuit keys was inserted in the primary circuit of the inductorium. Each of three of these was in circuit with resistance coils, respectively, as follows: key 2, 35 ohms; key 3, 670 ohms; and key 4, 4,845 ohms. By closing a key, the inductorium circuit was completed through the corresponding resistance and this change in the current produced the desired gradation of the sounds heard in the receiver. This gradation was determined empirically by a sufficient number of trials. The experimenter produced the stimulus sounds by playing upon these keys.

The observer signalled his reply by a similar system of keys, each of which was associated with a given sound. Thus, the sounds were numbered 1, 2, 3, 4, in order of their strength, beginning with the strongest, and the keys were numbered 1, 2, 3, 4, running from left to right. Hence, if the sounds appeared in the order 3, 2, 4, 1, the proper reply would be to press the response keys in that order.

A record of the stimuli and the responses was taken in a

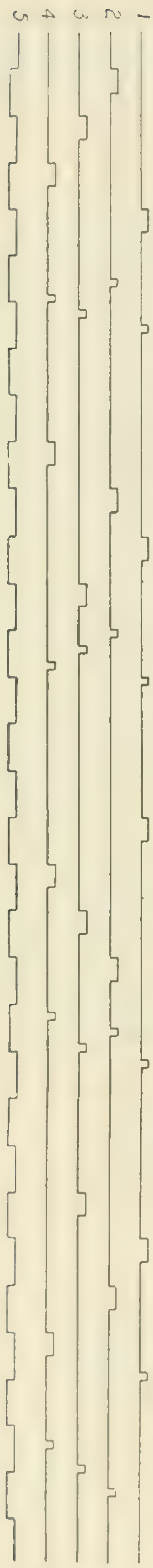


Fig. 15.

continuous tracing by the multiple recorder.[1] Each of the stimulus keys was connected with a pen on the recorder, and each of the response-keys was connected with the same pen as the corresponding stimulus key. The fifth pen in the recorder traced a time-line in seconds. A portion of a record is reproduced in Fig. 15.

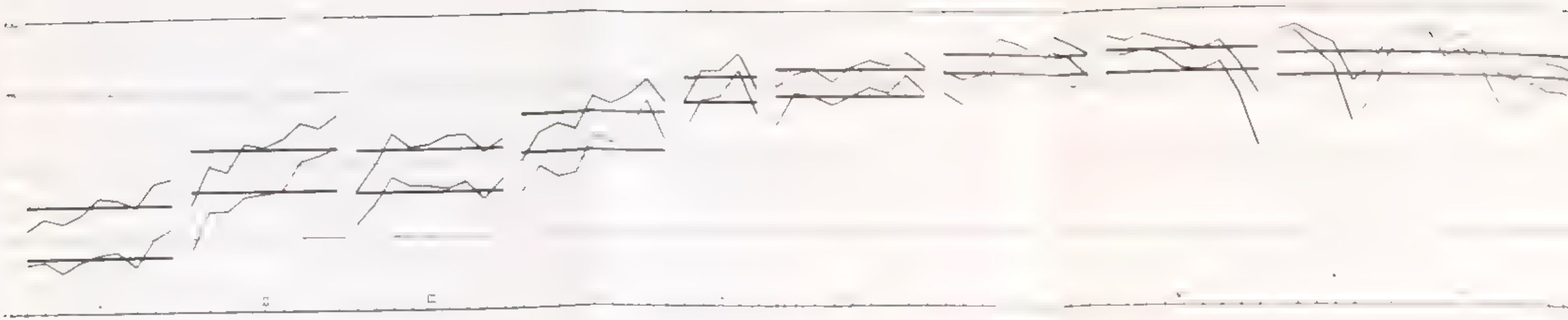
Each section contains first the group of four stimuli and then the group of four responses to the group received in the preceding section. Thus, the first response we cannot tell anything about because the stimulus is not on the record, but the second response is correct because it corresponds to the first stimulus group: the third response is wrong in part because it reads 4, 2, 3, 1, instead of 4, 2, 1, 3; and the fourth response, again, is correct. The record also shows the time of each stimulus and each response.

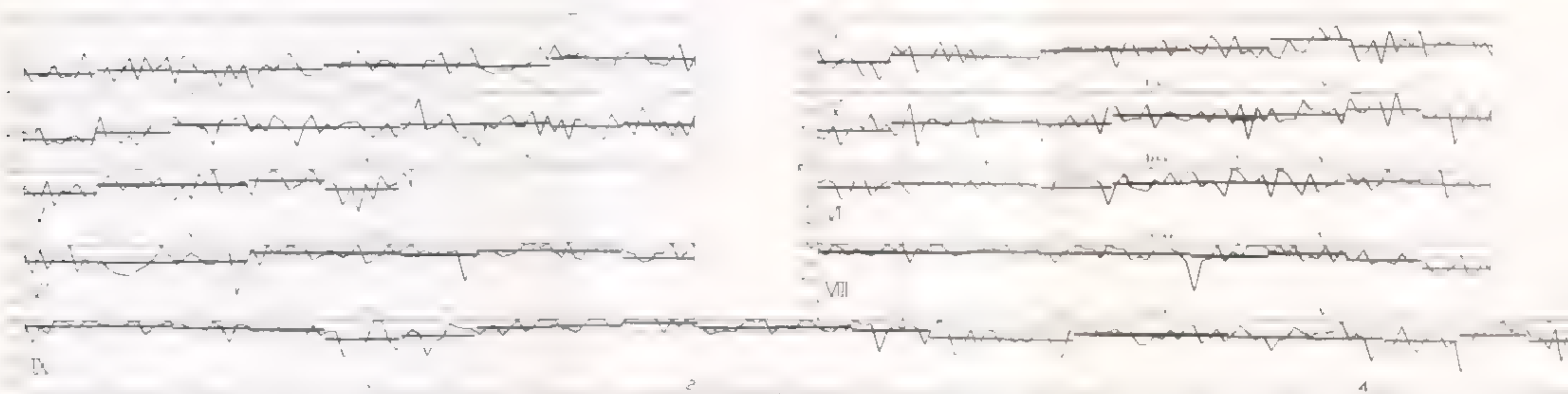
In regulating the time-order, the experimenter was guided by the beat of a metronome. The record shows that the sounds were produced at the rate of one per second, and that each sound lasted one half second. It also shows that an interval of four seconds was allowed for each group response and that the individual responses were given short and in quick succession so that the observer saved some time in which to change from the expressive to the receptive mental attitude.

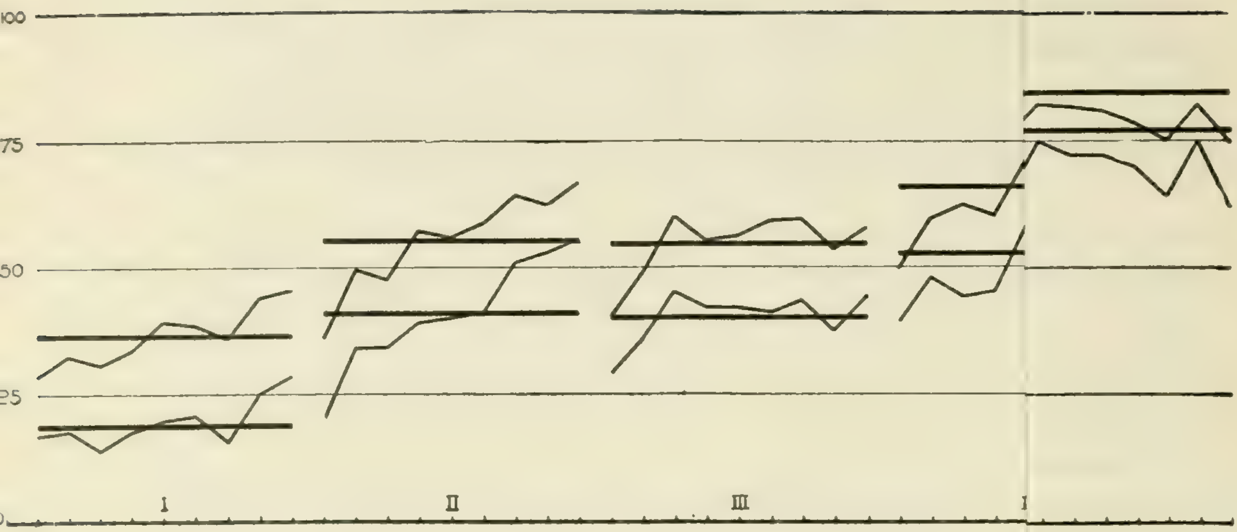
The apparatus was distributed in three rooms: the telephone receiver, the response keys, and the inductorium were kept in the observing room; the fork was kept in the distant battery closet, and the experimenter had the remaining parts in the measuring room. The observing room was moderately lighted with an incandescent light.

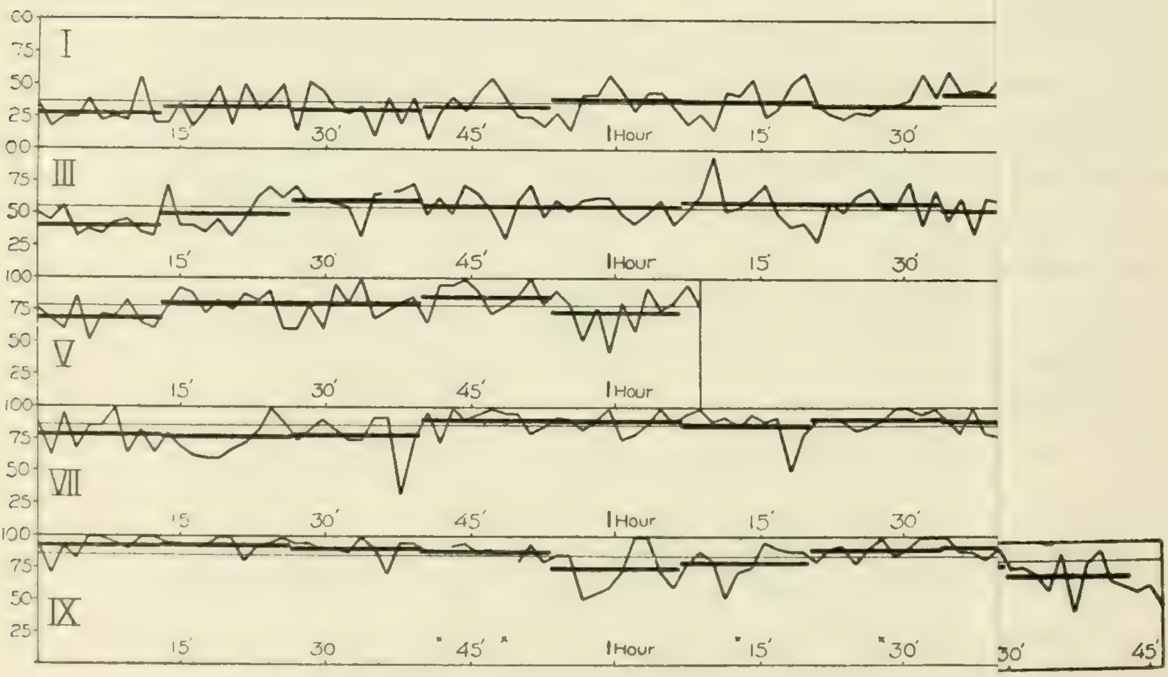
There are twenty-four possible mutations of the stimulus group with four sounds, but as two of these (1234 and 4321) are decidedly easier than the

[1] See references.




VIII
IX


100
75
50
25
I
II
III


I
III
V
VII
IX
15′
30′
45′
Hour

rest they were excluded and the remaining twenty-two mutations were used approximately in the order in which they would occur by chance.

The experiments here reported consist of a single series of nine periods taken upon one observer, Observer I., in Series I. and II., as follows:

List of Experiments.

Experiment.	Date.	Began.	Continued.
I.	Feb. 9	9:04 a. m.	2 hours.
II.	" 11	9:04 "	" "
III.	" 16	9:15 "	" "
IV.	" 18	9:08 "	" "
V.	" 20	8:37 "	1 hr. 13 min.
VI.	" 23	9:14 "	2 hours.
VII.	" 25	9:13 "	" "
VIII.	Mar. 1	8:57 "	" "
IX.	" 3	7:29 "	4 hrs. 48 min.

During experiment V., the repair man at the power house cut out the electric current without warning and, as the current was used in running the recorder, the experiment had to stop at that point. In experiment IX., the observer attempted to continue the test as long as he possibly could. After four hours and forty-eight minutes, he had to cease because his hearing had changed so much that he could no longer hear the faintest of the sounds in the stimulus group, although he had not reached the end of his endurance in other respects.

Explanation of the Records.

The tables of numerical results are too extensive to be introduced here, and they are not necessary, because the main facts may be represented in curves which give the true relief and show enough of the detail for the present purpose.

Pl. V. shows the results of the whole series *en gros*. Each link in the curve as a whole represents one experiment period. The base-line is laid off in units of one hundred acts. Since each act takes eight seconds, these units may also be considered time units of eight hundred seconds, or 13.3 minutes each. The time-line extends one unit to the left of each curve because the beginning point in each curve represents a hundred acts. The per cent. of success is indicated by vertical distances.

PLATE VII.

The degree of success is expressed in two curves which run nearly parallel. The upper indicates what per cent. of individual signals — out of a possible four hundred — were correct in each group of one hundred acts. The lower curve shows what per cent. of complete acts were correct in the same periods. The difference between the two curves is that successful parts of acts were counted for the former whereas only successful whole acts were counted for the latter.

For the two-hour periods, there are nine points in each curve. The average for the whole period is expressed by a heavy horizontal bar over each curve. By these means we are enabled to see at a glance the general trend of the progressive change and the existence of the long periodic changes. The figure as a whole is a fatigue-curve, work-curve, memory-curve, curve of ‘ learning,’ etc., according as it is viewed from one point or another.

Pl. VI. shows the same results more in detail. Here the record contained in the upper curve of Pl. V. — the per cent. of success, counting both whole acts and parts of acts which are correct — is represented in units of ten acts for each point. Each point in the zigzag curve denotes the average per cent. of success in ten acts. Averages for a hundred acts each are represented by the heavy horizontal bars, and averages for the whole record of a period by a light horizontal line. The numbers of the sections here, as in Pl. V., correspond to the list of periods, dates and durations in the above table. Pl. VI. shows the hour-waves and the minute-waves quite well.

Finally, to show the ‘ makes and breaks ’ in the continuity of power still more expressively, the data contained in the lower curve of period IX. are represented in a special manner in Pl. VII. Success in whole acts is represented by a line, and failures by breaks in the line. It shows the distributions of the ratios of success to failure. Thus, from the beginning, the record reads : Nine successes, one failure, one success, three failures, four successes, one failure, three successes, one failure, seven successes, etc. The three small-dotted parts indicate objective disturbances. The numbers indicate the ends of the successive hundreds of acts.

Extracts From the Introspective Accounts.

February 9. — At the beginning of the experiment, I attempted to find some method to aid in the remembering. I tried to remember the sounds by their numbers, by directing special attention to the first two sounds in a group etc., but abandoned these in favor of the attempt to retain the sensory image of the group as a whole. This change of method produced great irregularities in the first part of the record.

February 11. — To-day the series was not so fatiguing and no change occurred in the way of trying to remember the succession of sounds. Toward

the last it seemed comparatively easy and it seemed that a small number of sound conbinations were used over and over.

February 16. — I do not think that this record is any improvement over the last one. I did not feel so bright as last time — possibly on account of the close air in the room. I could not concentrate attention so well. The period seemed longer than the last one. * * * The strength of the sound series as a whole seemed to vary several times * * * in the latter part the sounds seemed fainter for a short time.

February 18. — To-day's record is better than the last one, and possibly better than any preceding record. There were no special disturbances except twice, probably at the change of classes.

February 20. — To-day's record is, I think, an improvement over the preceding. About 85 per cent. to 90 per cent. of the reactions may be correct. * * * The periods of attention and relaxation [minute-waves] seemed to have changed considerably. In the first and the second records I took, I should estimate that the periods of attention and relaxation are about equal, but in succeeding records the period of attention seems to have increased in length gradually.

February 23 and 25. — The observer wrote no introspective accounts on these days, because he had nothing special to record. He was aware of the continued practice gain and in both cases his estimates of the degree of his success was approximately correct.

March 1. — During the last part of this period the sound series seemed to grow stronger so that it caused confusion of the sounds. There were also some irregularities in the duration of the sound stimuli. The direct effect of these disturbances lasted for a short time only, but the thought of them tended to recur and was especially effective in distracting my attention because this took place in the last part of the record when I was fatigued and more subject to it.

March 7. — During the first few records I memorized the stimuli largely by visualization. I responded according to the impression of the successive louder or fainter sounds, not taking notice of particular ones of a group. Then I associated each stimulus with the key corresponding to it, and whenever a group was given I would glance from one key to the other in the same succession as the given stimuli. At the same time one or two of each group began to become more conspicuous on account of their position or intensity, *e. g.*, the first and last ones of a group or the place of the loudest or faintest ones, but especially the former. If the first one and the last one of a group were together, *i. e.*, if the corresponding keys were together, they would be the more conspicuous ones. So my memorizing depended largely upon the reference to the key board. Some groups were more easily remembered than others. Such are, 1324, 4231, 2134, 1243, 3421, 3412, 2341, 3214. In these the conspicuous features were noticed more easily and much earlier than in the others. But the more difficult ones were also memorized in the same way but more slowly, so that some were not thought of by their peculiar features until the last two or three records. These noticeable features of the various groups afforded a basis for 'names' of the groups so that at last I had a name for each group as follows:

(The keyboard was thought of not as a horizontal plane but as an inclined plane of which the end with the loudest sound was up and the end with the faintest sound was down.)

1324 = Zigzag down.
4231 = Zigzag up.
1342 = Two together, up.
4213 = Two together, down.
1432 = One up, three down.
4123 = One down, three up.
2341 = Three down, one up.
3214 = Three up, one down.
2134 = Both pairs from the center, up.
3421 = Both pairs from the center, down.
2143 = Both pairs up, up.
3412 = Both pairs down, down.
2413 = Parallel down.
3142 = Parallel up,
2314 = Two in the center, down.
3241 = Two in the center, up.
1423 = Up, down.
4132 = Down, up.
1243 = Straight down (except the last two are reversed).
4312 = Straight up (except the last two are reversed).
2431 = Triangle below.
3124 = Triangle above.

It never occurred to me until now that the remembrance of these groups depended so much upon the motor action of my right hand as it really does. As I am writing these groups and their 'names' I cannot give many correctly without actually performing the taps as if on the keyboard. After these names had become quite familiar, I had a feeling of confidence that I could give almost every response correctly unless I had been too inattentive to get the group correctly when it was given. Whenever I had clearly grasped a group I was certain that I could remember it with the aid of its names and give it correctly in the response. But before I had these names for the groups the occurrence of the second group would frequently cause me to forget the first, although I had it clearly in mind just after it had been given.

During the intervals between the stimuli I repeated the name of the group to be given after the next stimulus by actual movement of the mouth. I was unconscious of it and did not know it until I caught myself doing it. The movement of the hand in giving the response was almost entirely automatic so that I needed to attend only to the incoming stimuli. I was also unaware of breathing except when I happened to take a deep breath.

During the last record fatigue did not consciously affect me until about the end of the third hour. I became inattentive on account of a drowsy, sluggish or sleepy feeling. Whenever I would take a different position on the chair it would serve to make me more attentive. About a half hour before the end of this record I occasionally felt a stinging pain in my ear. This became more frequent and intense until the fourth or faintest sound was not perceptible at all, and then I decided to stop.

For nearly an hour after, I felt pain in the ear from time to time. I did not seem to be fatigued very much until two or three hours afterward. I felt as though I had done hard manual labor.

In reply to a request for fuller information in regard to the disposition of the time allowed for an act, the observer wrote, under the date of March 18:

As each sound of a combination was given, I glanced at the corresponding key and as soon as the third sound was heard, I knew the whole combination of four sounds so that, immediately after the last sound, I could turn to the preceding combination and respond at once. The reaction was almost automatic. After the response, I returned to the new combination and repeated its name but discontinued this long enough before the appearance of the next sound to be fully prepared. If I failed to allow time for preparation, I frequently failed to get the new combination.

Periodic Change: A. Hour-waves.

The hour-waves are quite as pronounced and uniform in this series as in Series I. and II. They may be seen both in Pl. V. and Pl. VI. Thus running the eyes along the curves in these plates, we see two and one half sinoid waves in each of Periods I., III., VII., and VIII.; three waves in Periods IV. and VI.; three and one half in Period II.; and one in Period V., which is incomplete. (Cf. the records on the same observer in Series I. and II. — Fig. I., Pl's I., and IV.)

About the same length of waves are found in Period IX., but they are here disturbed by the presence of longer waves which seem to be due to the awareness of the length of the task. The great depression of the long wave, one phase of which occupies two hours, is due in part to a temporary disturbance, the stopping of the fork. The points at which the forks stopped are marked with stars in the figures. These disturbances were only of a few seconds duration each and the loss is eliminated in the averaging for the groups in which it occurred.

Periodic Change: B. Minute-waves.

The same minute-waves which we are familiar with from the other series are here in evidence. They appear conspicuously in Pl. VI. in which their regularity is somewhat exaggerated by the want of detail. To show them with absolute fidelity, the record for Period IX. is represented in detail in Pl. VII. but here it is difficult to trace them on account of the confusion of these with the longer waves.

Periodic Change: C. Second-waves.

Here as in the preceding series the second-waves are present and can be obtained by analysis of the act and introspection. The form and combination of the attention-waves go through progressive changes throughout the series. For the purpose of simplifying the presentation, we may examine the distribution at a given stage, *e. g.*, the last record.

Fig. 16 shows in a schematic way the form and composition of the wave in a typical act at this stage, the stage of the highest mastery of method. The long bars 1, 2, 3, 4, represent the time of the four stimuli in the group which is to be impressed. The short bars 4, 3, 2, 1, show the time of the responses to the impression received in the preceding act.[1] There

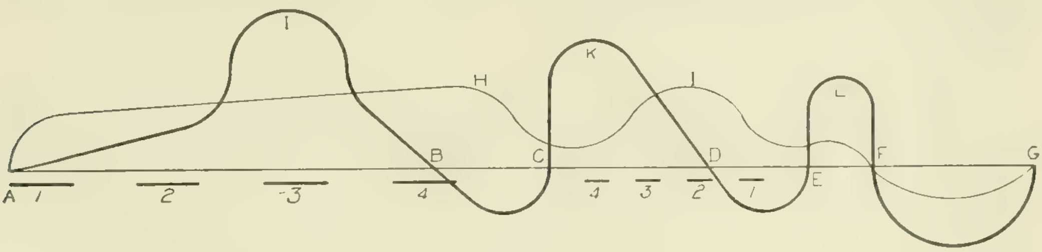


FIG. 16.

are three waves of active attention: *A–B*, to the act of receiving; *C–D*, to the act of responding; and *E–F*, to the repetition of the group which is to be retained. After each of these, there is a depression denoting the absence of this form of attention. The wave of passive attention is continuous for this act, although it shows certain characteristic fluctuations. A glance at the curve reveals these fluctuations better than a verbal statement.

The distribution complies with the general requirement, that the active attention shall be more impulsive and take more rest than the passive. Both forms have negative phases from *F* to *G*, but the active attention has two other pronounced negative phases, namely, *B–C* and *D–E*.

The receiving process clearly divides itself into active and passive stages. The crest, *I*, denotes the fact that, at the beginning of the third stimulus in the group, the fourth was pre-

[1] By mistake in lettering, the two orders, 1 2 3 4 and 4 3 2 1, which were not used, appear in the figure.

dicted and the order of the group was analyzed and impressed, by a well concentrated effort. After a moment's relaxation, there was another decisive mustering of forces in the act of recalling the order which was to be reproduced; the motor process of the response was semi-automatic. Following another moment of relaxation, there was a short but well regulated effort, *L*, in the reviewing of the image which was being retained. Then both forms of attention drop in preparation for renewed efforts.

The actual distribution varies from the type described, not only in the series as a whole, but in successive acts at this particular stage of practice. In fact, the progress of learning might be illustrated in terms of the progressive change in the composition of the attention-wave. So also every temporary fluctuation in efficiency may be described partially in the same terms.

This leads us to the question, Is it possible to maintain a perfect record for two hours in this particular act? All we can say is that the elimination of occasional errors would be an extremely slow and difficult process. The attainment of perfection depends upon the power to adhere to a most economic composition of the attention-wave. The distribution of failures, as shown in Pl. VII., indicates that it is possible to be systematic and successful for a considerable period at a time, but the hour and the minute-periodicities break in and the result is disturbance in the second-wave.

Progressive Change.

Examination of the curves in Pl. V., with reference to progressive change, reveals especially the following features: (1) There is a gradual increase in efficiency from the beginning to the end of each of the first seven periods, provided one neglects the periodic fluctuation and regards only the general tendency. The second and fourth periods show the greatest rise, and the third and sixth the least. (2) Periods VIII. and IX. both show progressive decline in efficiency. (3) There is a tendency for each successive period to start from the vantage-ground gained in the just preceding period. The rise of the series, however, shows three plateaus; the third day is on the same level as

the second; the sixth is on nearly the same level as the fifth; and the eighth and ninth are on about the same level as the seventh. (4) With increasing efficiency, the two curves tend to run closer together.

The curve as a whole is a practice-curve, but not pure and simple, for various other elements are involved in it. The act was appallingly difficult to the beginner. It was so much more than he could do that it required the greatest determination and perseverance in pursuit of the goal — mastery by practice. This goal he never quite reached, yet he came near it, and made a remarkable progress with practice. What was the nature of the improvement? The discussion of that might be considered the principal topic of this report, but we must content ourselves with merely a brief reference to it.

According to the observer's account, supplementing his written introspection, the following were important factors in the learning, or acquisition of power by practice: (1) Distributing energies, (2) finding method of receiving, (3) development of a visual scheme of associations, (4) automatism of the response, (5) naming, (6) repetition, and (7) rest.

Before the recording began, the observer had had no more practice with this act than was necessary to enable him to understand the requirements. At the first attack there was naturally a good deal of hesitation as to what part of the act he should concentrate his energies upon first. It was impossible to give uniform attention to the whole array of parts in the act.

This led to a skirmishing for methods — methods of receiving, methods of retaining, methods of reproducing. This skirmish was most noticeable during the first third of the first period. As a rule he was not aware of deliberately trying this or that method; there was a sort of desperate effort to get something in any way possible, and reflection shows that this resulted in the bobbing up of different methods which may be differentiated.

The general method soon found and afterward followed consisted in falling back upon a very concrete visual scheme of associations which grew gradually toward perfection and real-

ism. The scheme was not merely visual; the motor element was prominent and contributed much to the realism. Consciousness was distinctly visual-motor. The situation was alive! The observer became oblivious to all else and threw himself entirely into this melee. It was not abstract memory; it was a play. Each sound was merely the signal of a combatant who had to be located and his lunge parried.

The development of power in this act of parrying consisted in the formation of automatisms, just as the acquisition of skill does in the fencer. From this point of view, this act of memory resolved itself into a hierarchy of acts in which one grade after another was mastered in succession.

Still improvement consisted largely in progressive unification. Groups were gradually identified as wholes having striking peculiarities which gave rise to names. The names were descriptive and served as motor cues. The identification and naming led to the perception of the limitation of the number of variations, and this resulted in the development of a feeling of familiarity and ease which was decidedly conducive to endurance.

All these shortenings increased the spare time and it soon became possible to take advantage of that by recalling the impression which was to be retained while waiting. This repetition strengthened the retention as soon as it could be done without too much sacrifice of rest.

But the acquisition of time for rest and the habit of the most economical use of this rest constitute the most potent factors in the development of power of endurance.

Features like those here enumerated account for the profit by practice up to the highest point reached. That point seems to be approximately a physiological limit.

The observer accounts for part of the decline in Period VIII. on the ground of objective disturbance. That disturbance seems to have been overestimated and was probably chiefly subjective, but it brought on a mood or attitude which is the chief cause of the depression. For this reason, it is not safe to draw any conclusion in regard to progressive change in this period, except to state that a decline occurred.

The decline in the long final period is pronounced and decisive.

Part IV. General Conclusions.

The conclusions upon individual points are so tersely stated in the text that there is no need of repeating them here; but it may be profitable to restate some of the broader generalizations from the investigation as a whole, stripped of their technical garb.

Our primary aim was to develop methods of controlling and recording definite forms of continuous mental work in the hope (1) that we might pave the way to the experimental study of such facts as fatigue, adaptation, learning, and the effect of various conditions and stimuli upon the efficiency of continuous work, and (2) that certain general characteristics of mental work might be demonstrated in such faithful records. The results may, therefore, be summed up with reference to (1) method, and (2) certain characteristics of the efficiency of continuous mental work: (*a*) periodic change and (*b*) progressive change.

I. *Method.*

The methods here introduced for the measuring of prolonged mental work differ essentially in type from the methods in vogue, such as the adding method, the nonsense syllable method, routine work methods, the ergograph method, etc. Our methods seem to have advantage over every other method heretofore used in one or more of the following respects:

The work is mental.

The work is relatively homogeneous.

The work is reduced to fundamental types and relatively controllable conditions.

The measurements are continuous and in sufficient detail.

The measurements are on the work itself and not on injected tests.

The general principle is adaptable to different types of mental work.[1]

[1] It takes but little ingenuity to adapt the general principle of measurement here employed to the different senses, and to fundamental types of cognition and action. But this is only the first step; the methods must be used under specific conditions for specific purposes, *e. g.*, by Kraepelin's pause-method in the study of fatigue.

II. *Periodicity.*

1. *Periodicity a General Characteristic of Mental Work.*[1] — The three fundamental and representative types of mental activity studied, sensation, discrimination, and memory, exhibit alike a thoroughgoing periodicity. There is a continuous gradation from the period of the momentary active impulse up to the hour-long waves of mental efficiency. The efficiency in a given period, say two hours, may be represented by an irregular wave, the resultant of a series of partials.

FIG. 17.

Such a wave is analogous to the synthetic wave made by combining the fundamental and a dozen or more series of partials of a vibrating string. The typical fluctuations in mental efficiency under these conditions might well be represented by the well-known composite curve from the manometric flame shown in Fig. 17.

2. *Second-waves.* — Such periods of mental activity as are grasped in uninterrupted waves of attention.

Attentive work runs in elementary periods the length of which depends upon the individual and conditions of work but does not ordinarily extend more than a few seconds.

The attention-wave of Urbantschitsch is typical of this periodicity whether the stimulus is strong or weak, constant or variable, objective or subjective.

[1] The word periodic is not used here in the mathematical sense of exact equality, but in the sense of approximate or relative equality of period. The factors which condition the fluctuations are so exceedingly complex and variable that there is no reason to expect such beautiful and exact symmetry as is shown in Fig's 16 and 17.

This wave is composite: it consists of relatively long waves of secondary passive attention and one or more relatively short waves of active attention.

Active attention appears only at the crucial points; the bulk of the second-wave is secondary passive attention. Active attention constitutes the determining moments and secondary passive attention bridges the gaps between these.

In a complete attention-wave, *i. e.*, a second-wave, there is one moment at which both active and secondary passive attention are at rest (not present). Unless the act consists of a single impulse, there are two or more moments of rest from active attention, but the secondary passive attention never has more than one.

The second-wave is irregular in outline, being the resultant of two components which vary with the individual and the conditions of the work.

3. *Minute-waves.* — Periods which contain more than one second-wave but are less than twenty minutes in average length.

Beyond the second-waves, attentive work runs in short composite waves; these combine in series as partials of longer waves and exhibit the phenomena of interference and reinforcement.

When the work is such that it may be perfect for considerable periods of time, as *e. g.*, the memory work after long practice, these waves do not show in the objective record, but introspection reveals their presence through fluctuations in ease, certainty, concentration, etc., and through awareness of mind-wandering, ennui, dullness, and other more or less certain accompaniments of change in capacity.

4. *Hour-waves.* — Periods lying between the minute-waves and the well-known diurnal waves in length.[1]

Beyond the minute-waves, attentive work runs in one or more series of long composite waves of efficiency. These are distinguished from the minute-waves because they are probably the result of different conditions from those which underlie the shorter waves.

[1] By diurnal waves, we mean such daily rhythms in efficiency as are due to routine work, eating, sleep, recreation, etc.

The hour-waves tend to get shorter as the work progresses.

There is no evidence of any constant tendency of observers to begin the work at the moment of greatest efficiency, or any other particular phase of the work curve.

5. *Correlation of Changes.* — Keenness of sensibility (*To*) and alertness (threshold width) are not closely correlated.

Keenness of sensibility (*To*) and variability (mean variation), are not closely correlated.

In these two cases it is not so much a question of degree of correlation as of type of tendency; some individuals give a high positive correlation and others give an equally high negative correlation.

Large variability (mean variation) and lack of alertness (threshold width) are highly correlated.

6. *Individual Types.* — There are evidences in these records to show that each individual probably has a fairly characteristic type of waves for similar work done under similar circumstances but at different times. This is true of all, from the short second-waves to the long hour-waves. This individual characteristic may show even in as radically different work as the three forms here employed.

7. *Consciousness of Change.*—The shorter the wave, the more clearly the observer is aware of the change. Introspection can always grasp the second-waves, but hour-waves may have large amplitude without the observer's suspicion of their existence.

The feeling of dullness does not correlate closely with poor work in waves of medium length. The very feeling of dullness comes from awareness of exertion. Low efficiency correlates better with periods of unconscious neglect, absentmindedness, mind-wandering, etc., which become known only as we catch ourselves in such shortcomings and forthwith make a new start.

8. *The Significance of Periodicity.* — The experiments were not planned so as to test any theory of the cause of all this periodicity, but it is reasonable to suppose that they are all evidence of nature's way of protecting the organism; the periods of dullness are periods of relative rest from which the observer

comes forth refreshed. It is common to speak of the failing of voluntary attention as nature's safety valve; the same figure may be extended to the whole series of periodicities.

Fatigue, practice, and adaptation errors have received critical examination in experimental method, but in these periodicities we have a factor which in many forms of experiment is as important as any of those named. In measurements for comparison which run over half an hour or more, what is only a part of wave may be taken for progressive change. This is especially so if the experiment is repeated in the same order and on the same observer who may have a definite type of periodicity.

This periodicity favors our normal working in short periods. When composing, *e. g.*, one writes during a moment of lucidity and then relaxes into a state of ennui and feeling of restriction, only to take up the period again. In free work such as that, the fluctuations are very much greater (and perhaps more efficient) than in work under experimental pressure.[1]

It has often been observed that, even in a situation of life and death, these periods of relaxation and absence of power set in, to our moral shock and great discomfiture.

These fluctuations are a part of nature's great scheme of rhythm. They are a condition of endurance and progressive mental development.

Their significance is analogous to that of sleep.

One may discover his most favorable rhythms and adapt his work to these. The art of effective work consists largely in selecting the most favorable rhythms, both long and various partials.

III. *Progressive Change.*

1. *General Tendency of Change in Efficiency.* — These experiments were not planned to isolate the factors which form a basis for progressive change. The aim was rather to secure faithful measurements of the actual efficiency from moment to

[1] Thus, the end tests in Series I. on the effect of a liminal sound upon the ear used and the ear not used in a two-hour period depend upon what phase of the long waves they are taken in. This is the chief reason why we could not draw any definite conclusion from those tests.

moment, regardless of the underlying shift of elements. Yet certain important progressive tendencies may be seen in the records.

2. *Practice.*—There is no noticeable gain from practice in simple perception and simple discrimination, after the observer has clear knowledge of the nature of the stimulus.

The improvement in a complex cognitive act, such as memory, is very great.

The improvement in memory work may be ascribed chiefly to progressive systematizing of parts in the act, the development of automatisms, the association of concrete imagery, and economical rest.

The practice is effective in impression, retention, reproduction, localization, and expression; but there is a tendency to make gain in one of these factors at a time. This leads to the step-like or plateau series of progressions.

The marked practice improvement ceases when the observer has reached the limit of his inventive power in systematizing.[1]

3. *Fatigue.*—Continuous liminal or moderately faint sounds do not seem to lower the efficiency of the ear in a two-hour test.

In the long period of the memory test, the faintest sound, which was clearly above the threshold at the beginning, became inaudible during the fifth hour of the work. There is nothing to show whether this is due to the long continuation of the work or to the stronger sounds in the group; nor was the test continued to determine whether this loss of sensibility was evidence of progressive or periodic change.

We failed to differentiate between the central and the peripheral fatigue by means of the 'before'- and 'after'-tests.

4. *Adaptation.* — Since the sensibility tests show no unmis-

[1] From the suggestion contained in these records, and from the accumulation of experimental evidences not published, we are inclined to believe that there is a general law which expresses the probability of gain by practice in any form of mental activity. This law is, *The practice gain is somewhat proportional to the complexity of the act.* In other words, where there is room for noticing new factors and simplifying, there is promise of practice gain. Simple perception and simple discrimination in the above records do not show any practice gain because the acts were simple, but the conditions of either could easily have been made so complex that there would have been a greater practice gain than in memory.

takable general rise or fall in efficiency and the discrimination tests show a fairly decisive decline; and since the strain of attention is about equal in the two tests and change in peripheral sensibility would have but little significance in the latter, it appears that there may be a general decline of central efficiency in both tests and that this is counteracted in some cases of the sensibility tests by an increase in the peripheral sensitiveness through adaptation.

5. *Types.* — The observers conform to different types with reference to progressive change. They may be divided first into those which show general gain and those which show general loss. Then each of these may be subdivided with reference to the rate of change and with reference to the time and cause of change.

A CASE OF VISION ACQUIRED IN ADULT LIFE.

BY JAMES BURT MINER, PH.D.

The opportunity of studying a case of complete congenital cataracts in which vision was not acquired until adult life is seldom given to a laboratory. About twenty cases of acquired vision have been reported in the psychological literature of Europe; in all except a few of these, sight was restored early in life.[1] So far as I can discover, there has been no psychological investigation of a congenital cataract case in this country. The present Iowa case, moreover, seems to be the first attempt to utilize the modern laboratory equipment for testing systematically and quantitatively the senses and the learning process of a blind person who has been made to see. The young woman here reported was blind from birth by reason of complete cataracts in both eyes. She was operated on when she was 22 years of age.

In order that there may be no misunderstanding, it should be stated that persons having complete cataracts can distinguish light and darkness. In the opinion of Dr. Ware, who reported two cases to the Royal Society of England, patients with cataracts 'are never so totally deprived of sight as to be unable to distinguish colors.'[2] In the famous Chesselden case the boy could distinguish scarlet previous to the operation.[3] These individuals are 'blind' in the popular and medical acceptation of the word; they make no use of their eyes in their daily work.

[1] W. Preyer, *The Mind of the Child*, 1889, Vol. II., appendix C., excerpts from cases of Chesselden, Ware, Home, Wardrop, and Franz; references also to cases of Hirschberg, von Hippel, and Dufour.

B. Bourdon, *La perception visuelle de l'espace*, Paris, 1902, Chap. XIII., refers at length to the above cases and also those of Albertotti, Uhthoff, and Vurpas and Eggli.

R. Latta, 'Notes on a Case of Successful Operation for Congenital Cataract in an Adult,' *British Jour. of Psych.*, 1904, I., 135–150.

The Medical Index Catalogue of the Surgeon General's Office, U. S. Army, Second Series, 1898, congenital cataracts.

[2] Preyer, *loc. cit.*, p. 293.

[3] Preyer, *loc. cit.*, p. 286.

It seems safe to say that no person absolutely blind from birth has ever acquired sight. If the nervous mechanism were intact, a blow on the eye would give the sensation of light. With cataracts, there seems to be no reason why a decided difference in the intensity of light cannot be distinguished; some notion of the distance of objects would be acquired by this means. If there is a distinction between light and darkness, when an object comes between a patient's eyes and the sun he should be able to get some sort of visual idea of diffused outline. Previous to the removal of the cataracts he might, therefore, associate a visual impression with an accompanying movement. This would give some degree of visual space perception. Franz's patient could distinguish a vertical from a horizontal line on the first trial after the operation. It seems as if this would always be possible. The patient's vision is in much the same condition as if he had always been compelled to look through a glass of milk. Not only is the transparency of the lens affected, but the refraction is also disturbed. Previous cases seem to have established the fact that after the removal of the lens the patient is not able to recognise objects with which he was perfectly familar by touch. Objects also appear larger than they did to touch; solids look like surfaces. In the points above mentioned the present case corroborates the conclusions of previous observers.

History of the Case.

In this paper I can only attempt a brief preliminary report of the case which came under observation recently at this laboratory. The young woman, Miss W., besides having unusual natural ability for her age, brought to her new visual experience all the training of an excellent high school course. She is a graduate of the State School for the Blind at Vinton, Iowa. Moreover, she is a splendid introspector, as blind people often are. Having been inclined for many years, by reason of her blindness, to watch her own mental states with considerable care, she has developed a remarkably keen power of observing and describing her psychical experiences. One incident will illustrate this. When her retinal color fields were to be mapped

with a campimeter, one of her eyes being covered, she was told to fix her other eye on a certain point and, holding the eye still, to tell what happened as a disk of color was drawn slowly away from the fixation point. Usually the observer will note, when the disk reaches a certain place (the limit of the field), that it disappears or changes into a different color. She, however, besides noting these facts, made the additional remark that 'just before the color leaves, it seems to grow much brighter; it almost glows.' This change in intensity is one which a skilled introspector often has difficulty in observing. Miss W. would rank well with introspectors of many years' training. Another circumstance made Miss W. particularly valuable for the series of experiments which we conducted. Her eyes had fully recovered from the operations and had been so strengthened by use that she could carry out extended tests without fatigue. She had been fitted with both far and near spectacles, so that she was able to find her way about, and could even read print with some facility.

The first successful operation on Miss W.'s eyes was performed at the School for the Blind in March, 1902, by the surgeon for the school, Dr. Lee Wallace Dean, who is also professor of ophthalmology in the medical college of the University of Iowa. The operation was that of needling, or discission of the lens of the right eye. A year later a successful discission of the lens of the left eye removed that also by absorption. As it was not until November, 1904, that we had the opportunity to study her case, she had already acquired much knowledge of the visual world. Since the first few days of sight have been so completely reported by observers of other cases, this delay was found not to be serious. Miss W. was still completely naïve to many of the normal visual experiences of an adult. She had never looked through a stereoscope, opera glass, field glass, or telescope. She had never used both eyes together enough to find out any differences between monocular and binocular vision. She had not yet learned to translate her visual images into terms of movement with any degree of success, except in case of the most simple forms and numbers or with common objects of her previous touch experience. She

knew practically nothing about drawings or pictures. She had not even learned to identify people by their faces; those whom she thought she knew by their features were her mother, father, sister, a teacher at the school, and the nurse who was with her during the operations. Although I worked with her every day for over a month and she saw Dr. Dean often, I believe she cannot yet recognize either of us by sight.

There seems to be no doubt as to the congenital character of Miss W.'s blindness. Dr. Dean states that the lenses were completely cataractous at the time they were removed. He says: 'I am confident that their extent had not changed from the condition at birth.' The young woman's father says that a peculiar twitching of the child's eyes was noticed soon after birth, but the family did not realize that she was blind until she was about four months old. The family physician states that, so far as he can remember, he diagnosed congenital cataracts at about that age. It should be stated that the reason why the eyes were not operated on earlier was that the parents had been wrongly advised by an occulist whom they consulted when she was a child.

Early in her life Miss W. seems to have amused herself by trying to follow the heavy black letters in a primer. Her practice in this respect at first seems to indicate that she may have seen slightly. A further consideration of the facts as she remembers them indicates that it was only playing at reading. As she relates the experience she says she could hold a large letter close to the corner of her eye and by moving the book she could tell when she left the black of the letter. Keeping the eye still and carrying the printed letter in different directions she determined what the letter was by the movements of her arms. It may be possible that this process was aided when she was young by light reaching the periphery of her retina between the iris and lens. It is curious that the left eye which she used in this practice shows a retina which has been atrophied for nearly a third of the distance from the periphery. In Miss W.'s case there was always the ability to distinguish white from black, and as near as we can determine by questions, she could also distinguish red, blue and yellow. These colors were, however,

so much duller that she hardly recognized them after the operation. She says that when she first saw her clothing it seemed to her as if she had 'an entirely new wardrobe,' the colors were so unexpectedly bright and different.

The general plan of the investigation was to first arrange a series of tests for touch, hearing, and sight by which measurements could be made of the lower, upper, and discrimination thresholds. The tests were to be standardized so that they might be used subsequently in any other laboratory in similar cases. They were also to be the basis for comparison with records from a group of normal individuals, to determine the effect of the disuse of the eyes not only upon vision, but on the other senses. Unfortunately, as yet there has not been time to prepare the group of normal records for comparison, so that the conclusions expressed in this paper must be largely tentative in nature. Another series of tests was planned to investigate her process of learning in the new field of vision. This touched a long list of problems of special interest to education, psychology and philosophy.

Tests of the Senses.

There is a popular idea that the loss of sight in a blind person is compensated for by greater keenness in the other senses. The present case offered a chance to determine how far this conclusion is supported by the facts. The tests on hearing show that Miss W.'s range of pitch is very wide. As tested by the Koenig bars and the Galton whistle, Miss W.'s upper limit is approximately 50,000 vibrations per second, and a rough test of her lower limit indicates that it is slightly below 16. Although she has this wide range of tone sensations, we found that her discrimination between simple tones was not unusually keen. With the tuning forks she distinguished, nine times out of ten, a difference of eight vibrations from the international a′ (435 vibrations). Tests with the audiometer also indicated that she was not far from the average in her discrimination and liminal thresholds for sound. Her localization ability was tested by Mr. Starch with the Seashore sound perimeter.[1] The most

[1] Daniel Starch, 'Perimetry of the Localization of Sound,' *Univ. of Iowa Studies in Psych.*, 1905, IV., 1.

noticeable factor here was an inordinate tendency to move her head in order to localize the sound.

With the sense of touch, the pith-ball test showed no peculiar sensitivity for passive touch. The æsthesiometer tests on the tip of the forefinger were not numerous enough to be accurately stated, but they indicated that two points could be distinguished when 1½ mm. apart. Active touch was tested by a more satisfactory method. A piece of very fine steel wire (No. 35 B. & S.), 7 cm. long, was laid on a plate of glass. It was then covered with 43 sheets of letter paper (Brother Jonathan Bond, 17 × 22, 20 lbs. to the ream, 500 count). Miss W. determined correctly ten times in succession whether the wire was in a vertical or horizontal position. With 44 sheets she was right eight times out of ten. It is impossible as yet to give normal records for comparison.

So far as these tests on hearing and touch go, it seems to me that they give good evidence that training has improved her active touch and probably increased her interest in overtones to such an extent as to somewhat enlarge the range of pitch. She recognizes people entirely by their voices. For this purpose the noticing of overtones is more important than fine discrimination of simple tones. I would conclude, therefore, that the effects of training in active touch and hearing are evident, but there is little evidence that the native untrained capacity of other senses than sight has been increased. We may suppose that persons who have always seen could, by similar training from birth, make equally good records in hearing and touch.

In the examination of Miss W.'s vision, it was found that, without spectacles, she could read letters on the Snellen charts at 20 cm. distance which normally are read at 500 cm. Considering the fact that she has no lenses in her eyes, this may be regarded excellent. Dr. Dean has provided her with distance glasses having a spherical correction of + 10 diopters. According to his record her vision with these spectacles is 6/36; this means that she reads at a distance of six meters what she ought to read with normal eyes at a distance of 36 meters. Her reading glasses have lenses of + 13 diopters. With these

I found that she could read much smaller than the usual newspaper type (Snellen's D. 1.2) when she held it close to her eye. The campimeter and ophthalmoscope show that about one third of the outer part of the left retina is useless. Otherwise the relations of the visual area and color fields seem normal. Aside from the lack of lenses, Miss W.'s vision is also affected by the constant twitching of her eyes (nystagmus) and by cross-eyedness (concomitant convergent strabismus). These often accompany congenital blindness. There is some indication that the strabismus may be partially overcome, as will be explained in the discussion of binocular vision. Tested with prisms, the horizontal convergence cannot be corrected by a twenty-degree prism. A prism of from 4 to 6 degrees is necessary to correct the vertical disturbance.

The color vision of Miss W., it may be safely said, is decidedly above the average. She can detect color in solutions that are perfectly transparent to those of us who have been working with her in the laboratory. She can also discriminate differences in tint which are considerably below our threshold. In looking at the spectrum, she can apparently see ultra-violet which is beyond the usual field of view. The interpretation of her observations of the spectrum is a matter of some perplexity, so that I would not be sure of the present record without further check experiments. Preliminary work with the spectroscope indicates that her spectrum is about one fifth longer than the average of ten students. The length is added to the violet end. I suspect that this difference is largely due to a difference of interest in the test. In the examination of the color threshold and discrimination, the Lovibond tintometer was arranged for standard daylight, which was regulated and measured photometrically each day. With the intensity of light which was used, she was able to discriminate 16 times out of 20 a difference in red amounting, in the units of the instrument, to 1 in 700. Tested also for the threshold of color, she was able to name correctly red, green, blue and yellow nine times out of ten when the slides .2 were used. In the slides supplied with the instrument, .1 is the only lighter tint that is furnished. Two students who were tested became insensitive to color at slides .5 and 1.0;

they failed to discriminate reds, under the same conditions as Miss W. worked, when the difference in shade was 1 to 20. Another series of tests was started to check these results, using standard solutions of blue and red instead of the Lovibond tinted glass slides. As yet I have no records for comparison. The reaction time for discriminating white, black, red, green, blue, yellow and orange has been obtained. The examination of Miss W.'s perception of color contrast and after-images shows that in these respects she is practically like other individuals.

These facts of color vision seem to me to have some importance for biology. Twenty-two years of almost complete disuse of the retinas have caused no degeneration of the color process so far as can be determined. The tests point rather to a color vision beyond that of the normal adult. This seems somewhat contrary to what might be expected. The fact may be explained in several ways. It is possible that further tests on other adults may show normal individuals who can reach Miss W.'s record in color discrimination. Should equal records be made by any other adult, I should be inclined to believe that the pronounced difference manifested between Miss W.'s color vision and that of us in the laboratory is a difference due to her decidedly greater fascination for color. On the other hand, if the normal adult cannot equal her record, we seem to have a suggestion that the color process of the retina may degenerate with use. On account of the difficulty of interesting children in the tests, negative records made on those who are younger would hardly determine this point. The fact that there are no lenses in Miss W.'s eyes is also to be borne in mind; this may give her a clearer vision of color. So far as the evidence at present stands we seem to have several possible conclusions. The lens may obstruct our view of color, the color process may deteriorate with age, or a phenomenal interest in color may increase our liminal and differential sensitivity for light far above the average.

Apart from her remarkable sensibility to color, one of the most surprising facts thus far discovered is that Miss W. sees black objects larger than white objects of the same size. This reverses the usual illusion of irradiation. Measurement of the illusion shows that a white isosceles triangle with a base of 5 cm.

and altitude of 8 cm. appears to Miss W. as equal to a black triangle of 7 cm. altitude. The average variation from the altitude 7 is only .2 cm. for 16 trials. Using a special form of the method of right and wrong cases, a white square of approximately 5.5 cm. was selected as equal to a black square 5 cm. on a side. One of the most interesting features of this anomaly occurs with the illusions which are thought to depend on irradiation, the Münsterberg figure of the shifted checkerboard and the kindegarten pattern which is related to it.[1] Miss W. says that she is able in both these illusions to perceive the lines tilted in either direction with about equal facility, although at first she saw the checkerboard illusion reversed.

It is difficult to say what may be the explanation of this curious contraction of the white field or expansion of the black. I have suggested that it seems to indicate that the illusion here depends more on a brain process than on a retinal process, that it is connected with our general interest in bright things and disregard of dark objects. In Miss W.'s case the reversal of the white and black square illusion would be explained by the fact that during her twenty-two years of blindness it was really black objects which were the most important in her experience. When something dark came between her and the light, it was an obstacle to be avoided. The normal child, on the contrary, always actively demands what is bright and is continually interested in the more intense colors. It might be well to measure the illusion in persons suffering from melancholia, among whom darker colors are said to be more appreciated. The hypothesis that the central process is most important in Miss W.'s experience of this illusion is corroborated by the fact that she is able to reverse the kindergarten pattern. I believe that the irradiation illusions will acquire their usual form with her after more experience; I even found some indications of this during her stay at the university. The facts seem to establish that the peripheral diffusion of the light stimulus, if it occurs, is easily outweighed by the central condition. It is possible that irradiation is always central rather than retinal.

Aside from irradiation, Miss W. seems to obtain the common

[1] A. H. Pierce, *Studies in Space Perception*, New York, 1901, p. 213.

visual illusions normally, unless it be the illusions of interrupted space and those dependent upon perspective. In these latter cases her introspections at different times are in conflict. The Müller-Lyer and the cross illusions were measured for comparison between Miss W. and other individuals. I found no reason to suspect that the results with the Zöllner and Poggendorf figures were any different for Miss W. than for others. Records of the reproduction of horizontal lines by sight and by touch were obtained for comparison. They afford some evidence as to the relative value of visual and tactual space.

Investigation of the Learning Process.

Besides testing Miss W.'s senses of vision, hearing, and touch, the main effort has been to study the process by which she learns to interpret what she sees. Undoubtedly the most fascinating work along this line was in connection with the development of binocular vision. It is a prevalent belief among physicians that the ability to see objects single when using both eyes must be acquired early in life or not at all. It has been suggested that the necessary association paths in the brain cannot be developed in adult life. The occulists point to many cases where a condition of crossed eyes has been corrected in adults, by operations on the eye muscles, and yet single binocular vision has not been attained.[1] In such cases the individual neglects the image of one eye. Miss W. was in much the same condition as any cross-eyed person, except that she had used her eyes for only two years. Some idea of the progress which was made in the few weeks during which she was at the laboratory may be gathered from the following incidents.

While she was still naïve on this subject, I asked her to look through two small tubes, one held before each eye in such a manner that, if she desired, she could look with both eyes at the same object without moving the tubes. Under these conditions and looking at a single cone standing on the table, she said: 'I see two cones, one with the right eye and the other with the left.' She was quite emphatic about seeing two cones

[1] The question is in dispute, Landolt gives instances in which he trained patients, who were formerly cross-eyed, to get single binocular vision. Norris and Oliver, *System of Diseases of the Eye*, Philadelphia, 1900, IV., 151.

on the table. This was undoubtedly the usual way in which she interpreted the images from her two retinas.

The same effect was obtained in even a more striking way when Miss W. was provided with spectacles having differently colored glasses. Asked to describe how a large white surface appeared through the spectacles, she said: "Why, I see a large sheet of red cardboard with my right eye and a sheet of green cardboard with my left eye. They are both in the same place and I am just as sure that I see them both at the same time as I am that I am standing here." When carefully questioned if one card was not seen after the other or behind the other, or if one part of the surface was not red and the rest green, she persisted in her first statement. She said that she could not understand how there could be two different surfaces in the same place at once but that was the way she saw them. Under the most careful experiments with gelatines of unknown color before her eyes and instantaneous exposures by an electric spark (conditions under which others in the laboratory were able to see but one color, on account of the tendency to retinal rivalry), Miss W. still maintained her perception of two surfaces of full size and of different colors, not overlying each other in any way.

If the psychologist should say that probably Miss W. did see two surfaces in the same place at the same time, we might be somewhat confounded by the mathematical axiom. However, it seems to me that we are forced to admit that she really did see two things in the same place at the same time. Furthermore, I am inclined to think that this may be the usual impression in childhood under like conditions. Moreover, there may be two moons for the child. Our later interpretation of what we see is a matter of education. We learn, of course, that there are not two objects, so we neglect the doubling of our eyes; or we disregard the image of one eye thus developing our phenomenon of retinal rivalry. It is possible that this rivalry of retinal images arises somewhat late in the child's life, and is only gained after the visual experience is tested by touch. From a subjective point of view, we may be quite confident that, for Miss W. at least, two differently colored surfaces were seen in the same place at the same time, and, also, that she naïvely believed that she saw two cones when there was only one.

Under these circumstances the experiences of Miss W. with the ordinary stereoscope were exceedingly interesting and suggestive. As soon as it was discovered that she had never looked through a stereoscope, every precaution was taken to leave her completely naïve as to the effect of the instrument. The series of stereoscopic views used by occulists and the imported Martius-Matzdorff set were employed in the experiments. After some practice, it became apparent that there were hints of single binocular vision. At times she would say that she saw one figure, instead of stating that she saw one figure with her right eye and another with her left. The latter interpretation, however, continued when there was any marked difference between the two parts of the stereoscopic view. In a few cases I believe that I succeeded in getting her to combine views in which the picture before each eye had other differences than those necessary for giving relief. She thought also that she finally succeeded in seeing the sheen which results from combining black and white.

Before the experiments stopped, she had so far progressed with single binocular vision that she had no difficulty in seeing the ordinary stereoscopic picture in full relief, and she readily picked out views with no relief, and with a pseudoscopic effect. The usual precautions were taken to make sure that she was not merely saying that she saw single with both eyes, if she was unconsciously neglecting the image of one eye. As checks against this possibility, the partition between the prisms was removed, and she noted the three pictures visible; one side of the slide was covered, and she noted the shifting of the picture from the center to the side. Finally, accurate tests were made upon her ability to discriminate distances with both eyes compared with her monocular ability. Different sized balls were hung at varying distances from her. Using only one eye she judged them to be at the same distance when one was 15 cm. farther away. But the difference between the two balls was narrowed down to 6 cm. when both eyes were converged on one ball and then on the other. Her error was thus cut in half by using both eyes together. On account of her long standing strabismus hese tests were extremely fatiguing. At best she was able to

keep her eyes converged only a few minutes at a time. The improvement was so marked in the short time she was being trained, however, that her ambition to overcome her cross-eyedness does not seem entirely hopeless.

The introduction of Miss W. to a clearly outlined view of nature in perspective, which she first had in looking through the opera glass, afforded another series of introspections which mean much for the theory of space. The limits of the present article do not permit publishing the dialogues between Miss W. and the experimenter over this experience as it developed. They covered inquiries as to how she knew the picture was real, the interpretation of objects seen in unnatural size with the glass reversed, etc. Her æsthetic preference for blurred over clear outlines in certain circumstances is an interesting contribution to the defense of impressionistic art. These experiments were followed by an attempt to find out how she learned what perspective in pictures meant. Beginning with simple line drawings, she was gradually led to the interpretation of complex scenes and even cartoons. The latter still distress her very much. 'Why do they make all the people look so ugly?' and 'What are all those lines on their faces?' were some of her comments about newspaper drawings. Considerable material has been gathered which may give helpful hints for teaching drawing. The child every day lives through modified forms of the experiences which Miss W. had in the laboratory.

From the psychological point of view, it was important to determine whether the development of her conceptions of form, solidity, distance, and number always required the translation of visual images into movement and touch terms. Her all-powerful impulse to explain anything new by referring it at once to the language of her sightless experience, makes the interpretation of her visual consciousness very difficult. By showing some novel figure to Miss W. for a few hundredths of a second through an exposure shutter, it was possible to study how she perceived, imaged, and interpreted it. After repeating the exposure many times, her method of counting the sides of a figure could be observed. With practice she was able to obtain an indistinct image and then count the sides after the picture had been withdrawn, although she could not count the sides

during the time of the exposure. Kinæsthetic sensations undoubtedly play a most important part in her conceptions of number and space. When Miss W. was directed to count the sides of a hexagon, but to shut her eyes the instant she caught herself making any movement, and then begin again, I found that she was not sure of the number of sides after observing the figure a total of five minutes. She would not look at it continuously more than 10 to 20 seconds without beginning to count its sides by using some muscular contraction to mark each corner as she changed her attention from one part of the visual impression to another. She would tap with her fingers or foot, press her teeth together or her tongue against her teeth, move her head, regulate her breathing, or even slightly wink at each corner, in order to register that as number one before passing to the next. On account of this irresistible impulse to move and to touch, it may be doubted whether a blind person who acquires vision is a suitable subject to decide whether visual images have spatial meaning apart from movement. In no case, I believe, has Miss W. ever questioned the extension characteristic of her visual sensations. So far as the experiments went, they corroborated the current hypothesis that shape and number get their meaning from touch and movement.

Many instances might be cited to show the difficulty Miss W. has in interpreting her visual experience. For a long time, shadows were quite troublesome to her. They seemed like real objects. Once in a while she still catches herself walking around a shadow on the sidewalk, or stepping over it as she would any obstacle in her path. Dishes were upset at the table because she could not judge their position. A cat a short distance away was mistaken for a chicken. The color of an old waist appeared so different that she could not recognize the garment for some time after touching it. Except for the color, she would probably have known it at once. She finally made sure by feeling the pin holes in the cuffs and by looking at it with the eye which had not been operated on. The morning after she looked through the university telescope at the stars, she anxiously inquired: 'Could you see any points on the stars?' Her previous touch experience had associated star with a pointed figure. Although her numerous natural blunders in trying to

understand her new experience are exceedingly interesting, they do not seem nearly so remarkable as the marvelous ability she constantly manifested to interpret novel experiences which she knew nothing about previously or knew only by description. The compass, for example, was recognized by sight at once from what she had learned about the magnetic needle, although she had never seen one. All sorts of strangely shaped blocks, complicated pieces of machinery, scenes in the field glass, etc., were accurately described without touch. The fact that so much could be done with the little practice that she had had suggests that too much emphasis ought not to be given to a current theory concerning the non-spread of training. If training is a greatly specialized process, we should expect in Miss W.'s case that the ability to memorize by touch and sound would be markedly better than by sight. When I suggested testing her visual memory, she said at once that she could not remember what she read in print. The preliminary tests which I have been able to carry out, however, indicate that there are only slight differences between auditory, touch and visual memory of a series of letters or of sentences. It is true, no doubt, that the thought to be remembered in all these cases is translated at once into movements of the vocal organs. She repeats to herself what she is reading or hearing. But the fact that visual sensations can be associated so accurately and permanently with the vocal movements of the larynx is still important evidence of the spread of memory training. Unfortunately the tests have not been carried far enough to be conclusive. I can give little more than a general impression. Her reproduction of passages from 'The Greatest Thing in the World,' read by point type, by sound, and by sight was recorded, as was also her memory of twenty letters irregularly arranged and read in the three different ways for several days in succession — until she had committed them all. No weakness of visual memory, or of the translation of visual terms into muscular, if that is the way we remember, was apparent in either of these tests. Further experiments on Miss W. along this line seem to offer a most promising field for educational investigation.

Before the more complete technical description of the experiments and the quantitative results are published, it is desira-

ble that the tests should be repeated on a representative group of normal adults. The conclusions here given are necessarily very guarded because they must depend to a large extent upon impressions gathered from a general survey without adequate opportunity for comparison. In a paper of this length I can only hope to hint at the bearing which the facts discovered have upon various psychological theories. New problems opened up almost daily. The case promises quite as valuable results along other topics of eductional, biological and philosophical importance as those taken up. In the field of psychological æsthetics, naïve preferences were expressed which have interest in connection with primitive conceptions of visual beauty. In the genetic aspect of the case, more work might well be done.[1]

Briefly summarized, the suggestions from a review of this case are: (1) While hearing and touch show great keenness in some respects, there seem to be no records which cannot be explained on the basis of greater interest and training, and without supposing a compensatory change in the capacity of these sense organs. (2) Color vision is so far above normal as to contradict any supposition that 22 years of disuse would cause degeneracy. On the contrary, either the color process deteriorates with use or the removal of the lens and unusual interest produce a remarkable ability to discriminate colors. (3) The reversal of irradiation indicates that a central process may readily outweigh the retinal process. (4) The absence of retinal rivalry suggests that this process is developed by education; and the tendency to regard a single object as double indicates that this is the nature of the first visual experience with two eyes. (5) Contrary to a prevalent opinion, single binocular vision may be acquired, at least temporarily, by an adult born blind. (6) Number and space perceptions are apparently dependent upon movement. (7) An unexpected spread of training, especially in memorizing the visual impressions of printed letters and sentences, suggests a caution as to the amount of training which an intelligent adult may transfer from one field to another.

[1] I desire to express my thanks to Prof. Carl E. Seashore, Mr. Daniel Starch, and Mr. E. A. Jenner. Their suggestions and assistance made it possible to carry out many experiments which could not otherwise have been performed. To Dr. L. W. Dean, also, for his hearty coöperation in the present study.

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PERIMETRY OF THE LOCALIZATION OF SOUND.

BY

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PART II.

CONTENTS.

The first part of this investigation was published under the same title in *The University of Iowa Studies in Psychology*, 1905, IV, 1–45. The object of Part I was to determine the accuracy of localization in various representative directions. The present research is an effort to study in detail some of the numerous problems to which those experiments gave rise. The following is a report of five series of experiments which were made during the academic years 1904–06.[1]

SERIES I.

Sensibility to Sound in Different Directions.

Problem, Method, Apparatus and Observers.

One of the most frequently recurring observations in the experiments of Part I was the fact that a sound of uniform intensity and at a constant distance from the observer seemed to be nearer and correspondingly louder in some directions than in others. The problem then was to determine the extent of this variation in apparent intensity and apparent distance of an objectively uniform sound.

How can this subjective variation be measured in a definite way? Intensity of sound varies in some ratio, inversely with its distance; we estimate the distance of a sound by its intensity and the intensity is interpreted partly in terms of distance. We want to measure, in a selected group of directions, the actual variation in intensity that is necessary to make the sound seem uniform to an observer. Assuming that the sensibility or threshold of hearing remains fairly constant during the time of the measurements, we may first measure the acuity of hearing in representative directions in which apparent changes of intensity have been noticed. The differences in the thresholds of

[1] The writer wishes to express sincere gratitude to Prof. C. E. Seashore for suggesting the original problem in Part I, for his many suggestions of new problems and methods of experimentation throughout the entire investigation, and for the time he has given as observer in several series of experiments. Acknowledgments are also due to all the observers who participated in the experiments.

hearing among these directions may then be regarded as measures of the differences in apparent intensity.

For this purpose, apparatus was needed to produce a sound variable in intensity according to definite units but uniform in all other respects. An electric fork of 100 v. d. was driven by a current of three amperes and three volts, which were kept constant throughout the experiments. A shunt from the fork was completed through the primary coil of Seashore's audiometer,[1] of which the secondary coil was connected with a telephone receiver. The audiometer is an instrument devised for the purpose of controlling and measuring the intensity of sound. The essential part of the audiometer is a primary and a series of secondary coils by which the strength of the current and consequently the intensity of the sound can be varied. The scale of intensities which rises from one to forty, is based on the psychophysical law so that the ratio of any two successive increments on the scale is psychologically the same.

The apparatus was distributed in three rooms. The fork was mounted in the battery closet, the audiometer operated by the experimenter was in the measuring room, and the receiver was mounted in the observing room which was moderately lighted by incandescent lamps, and practically sound-proof—a condition necessary for successful experiments of this kind. The receiver was mounted on a tripod and, in order to avoid resonance, was insulated from the iron support by heavy felt. A pasteboard tube, six inches long and two inches in diameter, lined with felt cloth, was attached to the face of the receiver for the purpose of directing the sound toward the observer. Throughout the tests the receiver was kept in the same position and at a constant distance from the observer, namely one meter from the center of the head.

The observer was guided in keeping the proper position by a ring of wire suspended from above and hanging freely about the head. In finding and keeping the various positions the

[1] For the original description of the audiometer see, Seashore, "An Audiometer," Univ. of Iowa Studies in Psych., 1898, II, 158–163. A briefer description can be found in the Univ. of Iowa Studies in Psych., 1905, IV, 48–49.

observer turned to the desired position in each case and was guided by labels on the walls.

The threshold measurements were made in a series of directions in the right half of the horizontal plane through the aural axis at points 15° apart. The points were, 0° front, 15° right front, 30°rf, 45°rf, 60°rf, 75°rf, 90°r, 75° right back, 60°rb, 45°rb, 30°rb, 15°rb, and 0° b.[1]

In determining the threshold of hearing we may proceed in two ways, following the method of minimal change; we may begin either with a subliminal sound and increase it until it is just perceptible, or with a supraliminal sound and decrease it until it is just not perceptible. In the former case we get a determination (*To*, threshold over) just a little above the determination obtained in the latter case (*Tu*, threshold under). The average of the two is regarded as the threshold. For the present purpose, however, it is better to use only one of the determinations and thereby have the advantage of a simpler computation and evaluation of the results. *To* is preferable to *Tu* because it is easier to determine the appearance of sound than its disappearance.

The measurement proceeded as follows. The observer comfortably seated on a stool in the observing room, held in his hand a strap key which was connected with a sounder and battery in the measuring room. All communication between the observer and the experimenter was by means of signals through the sounder and the receiver. The experimenter began by giving a loud sound in the receiver as the signal for starting. After an intermission of two or three seconds he started at a subliminal point, usually from five to seven units below the threshold, and increased the sound at the rate of approximately one step a second until the observer heard it and responded by a tap on his key. The step on the audiometer scale at which the response occurred was then recorded. This constituted one determination. In the same manner ten determinations were made in succession for a given standard direction. In

[1] Cf. Fig. 2, page 6, in Part I.
In this system of designating directions 0° front is directly in front, 0° back is straight back, and 90° right is opposite the right ear.

order to eliminate from the results the possible disturbance coming from the change in position on the part of the observer, in passing from one direction to another, the first three measurements at each direction were not recorded. After ten records had been made in one direction the experimenter again gave a loud sound through the receiver. The observer then turned to the next position and signalled as soon as he was ready to begin again.

The observer also had the right to throw out of the record any trial in which he had anticipated or delayed his response or in which some other disturbance had occurred to invalidate the measurement. But this did not occur often when conditions were normal. The observer had to indicate immediately his desire to discard a measurement, by signal to the experimenter. A complete record consisted of twenty determinations in the double fatigue order for each one of the thirteen chosen directions. The time required for taking such a record was approximately forty-five minutes.

Records were obtained from eight persons, N. C., D. S., C. E. S., E. A. J., N. B., E. G. Q., R. W. S., and H. S. B. Two of these, N. C. and N. B., are women. C. E. S. and D. S. were experienced observers and the others were students in the technical laboratory course. After two or three records had been taken on each observer, it was noticed that they fell into two distinct types according as the threshold for front was higher or lower than for the back. In one type the threshold for front was higher than for the back, and in the other type the reverse was true. One representative of each type was chosen (N. C. and D. S.), and a series of ten records was obtained from each one of these two. In all, thirty-five records were obtained from the eight observers, making 700 determinations for each direction, or 9,100 in all. After these tests, N. C. and D. S. each made three more records using only one ear.

Before the regular tests were begun the threshold was found for each ear separately in the case of every observer, as a large difference between the two ears might affect the records. An observer never made more than one record a day. They were at the same hour on successive days, barring a few exceptions.

The order of the directions was reversed for successive records; instead of beginning all the records in front, each alternate one was begun in the back so as to make the conditions as uniform as possible for all directions. The observer was alone in the room, which was thoroughly ventilated before each test.

Binaural Threshold.

Method of recording—The following is a typical record in the statistical form in which they were originally made during the course of the experiments. The numbers are the readings on the audiometer scale for the separate determinations.

Specimen Record.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
	22	20	18	16	14	14	14	13	12	12	13	14	15
	22	20	17	16	14	13	13	12	14	13	14	14	15
	19	20	17	16	14	14	12	12	12	13	15	14	15
	21	19	16	16	15	13	12	12	14	13	15	15	15
	20	19	15	15	15	12	13	13	13	13	14	15	14
	21	19	17	16	15	13	12	12	13	13	13	15	15
	20	19	17	15	15	14	13	12	13	14	14	15	16
	20	18	16	15	15	14	12	12	12	13	14	15	15
	20	20	15	15	15	14	12	12	13	13	15	15	16
	20	18	17	15	15	14	14	12	13	13	15	15	15
m. v.	.8	.6	.8	.5	.4	.6	.7	.3	.5	.2	.6	.4	.4
Second half.													
	17	15	16	13	13	12	12	13	13	14	14	17	20
	17	17	16	14	13	14	14	12	13	14	14	14	20
	18	16	15	14	13	13	11	12	12	13	13	15	17
	18	16	14	14	13	13	12	13	14	12	14	15	16
	16	16	16	13	13	14	12	13	13	14	14	16	16
	17	16	16	14	13	14	12	13	13	13	14	14	14
	17	16	15	15	13	13	11	12	14	13	15	17	17
	17	15	15	14	13	13	13	11	13	13	14	15	17
	19	15	16	13	12	14	12	13	14	13	14	15	16
	17	15	15	15	14	12	12	12	13	13	15	15	15
	18.9	17.5	16.2	14.7	13.9	13.4	12.4	12.3	13.1	13.1	14.1	15.0	16.0
m. v.	.6	.6	.6	.5	.2	.6	.6	.6	.5	.5	.4	.8	1.4

The curves—Instead of giving all the records in this form, the results may be represented more advantageously and clearly in graphic form. Figures 1 and 2 contain the records of the two observers from each of whom ten records were obtained. Fig-

ure 3 presents the composite curves. Each one of the light curves represents the averages of all the determinations made at

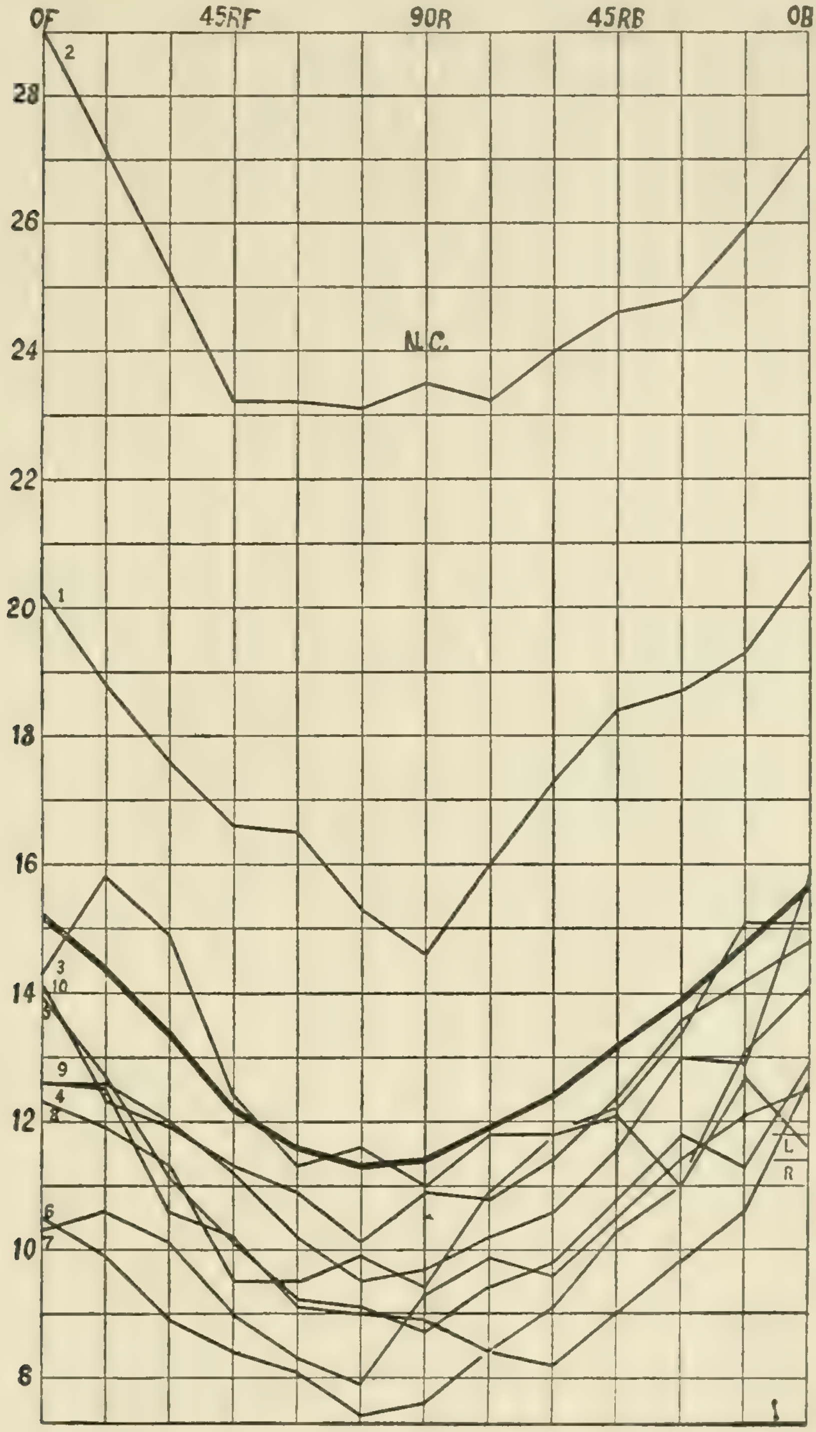


FIG. 1

each direction in one record. The heavy curves are the average curves. The numbers at the top are the designations for the

directions. The large numbers on the left are the steps on the audiometer scale, and the small numbers at the beginning of each curve indicate the order in which the records were made. The two bars at the right indicate the thresholds of the two ears obtained before the records were begun. The lower a point is in the curve the lower is the threshold; that is, the keener is the sensibility.[1]

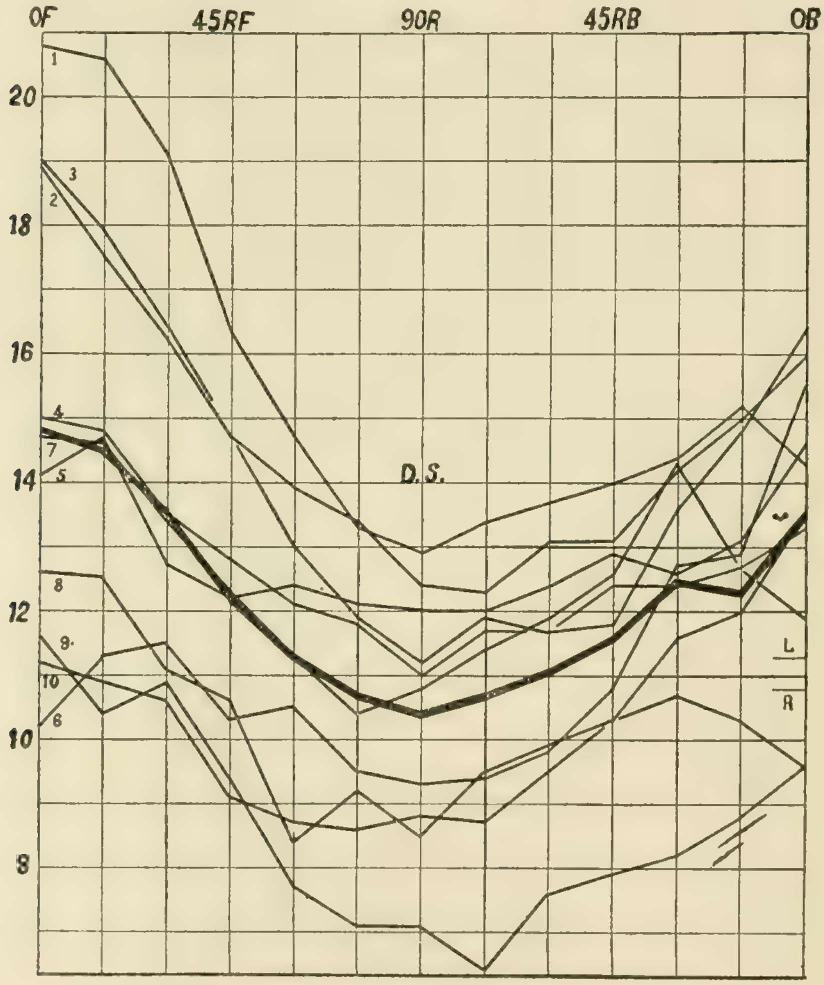


FIG 2

The individual records of the other observers are not presented in individual curves but in the statistical form of Table I. Each horizontal row of numbers is a record. Each num-

[1] The first two curves in Figure 1 are considerably higher than the others. This is due principally to the conservative standard of responding which the observer had adopted. She did not respond until she had "verified the hearing of the sound." In the latter records a less conservative standard was adopted.

ber is the average of the twenty measurements. The upper part of Figure 3 is a composite curve based upon this table.

TABLE I.

0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
C. E. S.												
15.6	14.4	13.9	11.8	11.2	10.4	10.0	11.1	11.2	11.9	15.3	15.5	16.5
15.1	14.6	12.7	12.3	11.2	10.5	10.1	11.0	11.5	12.2	13.8	14.5	16.5
E. A. J.												
20.5	18.5	14.9	13.6	11.7	10.5	10.2	10.8	10.8	12.1	13.8	15.3	15.3
16.1	14.7	14.0	13.1	11.4	10.2	10.1	11.3	11.3	11.2	14.2	14.7	15.3
N. B.												
13.2	13.1	12.2	11.6	10.9	10.1	9.7	10.3	11.0	12.6	13.6	13.8	12.7
21.7	21.5	20.9	18.7	17.6	15.9	15.7	16.8	16.8	18.6	19.6	19.2	20.2
20.1	20.0	19.5	17.5	17.0	15.6	14.5	15.8	16.9	18.4	18.9	18.2	17.9
E. G. Q.												
29.3	28.6	27.3	26.3	25.1	24.5	24.2	25.3	25.6	26.6	27.4	28.1	27.7
27.3	27.3	26.3	25.4	23.8	22.3	22.8	22.9	24.1	24.8	26.5	27.5	28.0
32.3	33.0	32.3	30.2	30.2	30.0	29.5	30.4	31.1	31.9	30.0	31.5	34.6
R. W. S.												
20.4	18.7	16.0	14.9	13.3	12.7	12.8	13.5	13.0	14.2	15.9	17.2	17.6
15.0	15.6	15.1	14.7	13.8	13.2	12.9	13.6	14.6	13.8	14.9	14.8	14.5
H. S. B.												
24.3	22.3	20.9	20.1	20.2	18.4	18.0	18.2	20.6	21.4	22.1	22.8	24.2
17.7	14.4	13.7	12.1	10.9	10.9	10.7	11.7	12.9	13.5	16.3	19.9	20.6
26.6	26.0	24.4	23.8	23.0	22.5	22.3	23.7	25.0	25.8	26.9	28.6	27.2
Averages.												
21.0	20.2	18.9	17.8	16.8	15.8	15.6	16.4	17.1	17.9	19.3	20.1	20.6

The mean variation for these records is not given because it is relatively small in all the measurements. The mean variation in the sample record above is representative.

Data in the curves—(a) The region of keenest sensibility is at the side, at 90°r. (b) Front and back are considerably less keen than the side. (c) There is a difference among the observers in regard to the acuity in front and in the back, which divides them into two distinct types: those who have greater keenness in *front* and those who have greater keenness in the *back*. (d) The remaining directions occupy intermediate positions so that

the curves are free from breaks and present a relatively smooth appearance. The curves approach the shape of a flattened U.

If we compare the curves of Figure 3 with the other curves we notice at once the general prevalence of these features. Although there is no absolute guarantee that the stimulus from the fork remained constant through the tests, there is every reason to believe that it did remain practically constant. We may, at any rate, safely assume that there were no serious changes

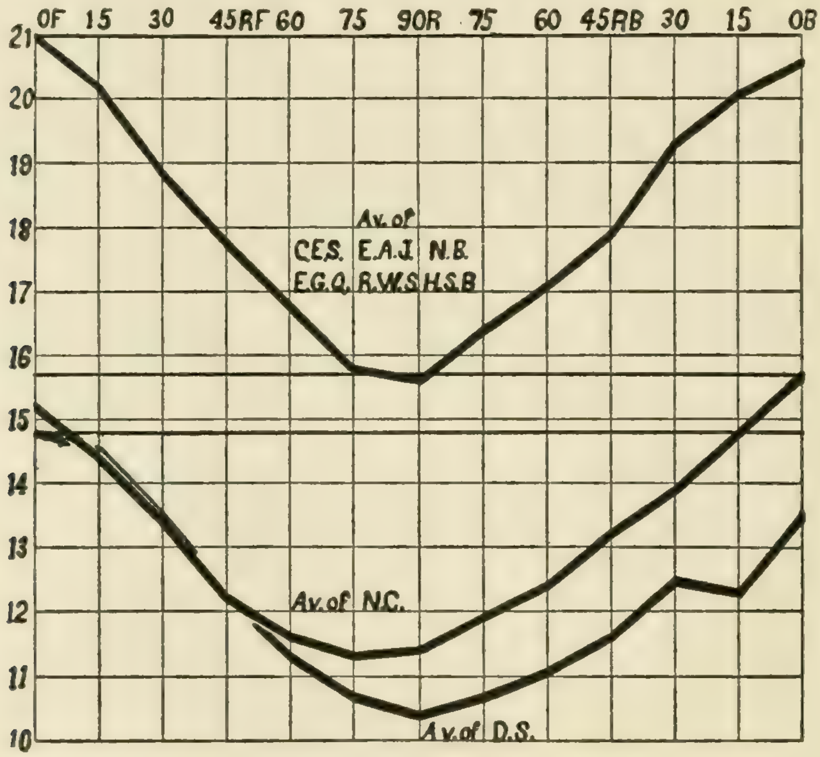


Fig. 3

for periods as long as the taking of a complete record. In order not to disturb the uniformity of the stimulus the accessories of the apparatus, such as the wiring, fork contact and the battery current (from three Edison-Lelande cells), were kept as uniform as possible.

Comparison of side with front and back—The keenness of the side as compared with front and back may be expressed in a ratio. In the average curve of N. C., Figure 3, the lowest, i. e., the keenest, point at the side is 11.3 and the average for front and back, 0°f and 0°b, is 15.5, giving the ratio 11.3 to 15.5 or

.73. The ratio of the average curve of D. S. is .72 and for the average curve of the other observers .75. It seems then that irrespective of the absolute thresholds, without regard to the relative keenness or obtuseness of hearing, the ratio is constant, being approximately 3 to 4. Whether the curve is higher up in the scale or lower down, its general form will be the same. See for example the individual records in Figures 1 and 2 and in Table I. This undoubtedly finds its explanation in the fact that the graduated scale of intensities on the audiometer is based on Weber's law, making the increments on all parts of the scale equally perceptible.

Comparison of threshold with "distance" tests—In Part I[1] a series of tests was made to determine the apparent distance of a sound of uniform intensity and distance in the same group of directions for which the threshold has been measured. It was there found that the same sound was estimated nearest at 90°r and farthest away in anterior and posterior directions. At 90°r it was estimated to be at a distance of 38.3 inches, while at 15°rf it was estimated to be 44.4 inches, and at 15°rb, 44.6 inches.[2] Thus the curve of the distance tests agrees with the threshold curves. The threshold is lowest at 90°r where the sound seemed nearest; and it is highest in the anterior and posterior directions where the sound seemed farthest away.

Introspections—The following introspective notes are quoted as they were written by the observers immediately after each test. They express characteristic observations and experiences of the observers during the experiments. Unessential parts and repetitions in successive notes are omitted.

D. S. 10.00 a. m., Oct. 25, '04.

Had my eyes closed. Have a cold in my head. Mind wandering was rather disturbing in the first half. Felt quite comfortable and was not particularly wearied at any time. Sometimes it seemed that I could hear the sound all the time and that I responded when it seemed to become louder. At the point 0°b the sounds seemed to be 15° or 20° to the left of 0°b. There was no tendency toward rhythmic response.

[1] See Part I, 20–23.

[2] 0°f and 0°b seem to be exceptions; they were estimated to be nearer than 15°rf and 15°rb.

D. S. 10.00 a.m., Oct. 27, '04.

Still have quite a severe cold in my head and occasional singing in my left ear. Noticed especially the rushing of blood in the head and the heart beating. Sometimes the sound seemed to come in pulsations corresponding to the heart beats but I could not determine whether they coincided. Could not notice any qualitative differences in the sound from different directions. Sometimes it seemed that the source of the sound was nearer at the side than in front or in the back. I do not think that my standard of certainty in responding changed noticeably during this test. My attention varied considerably. About half of the time it was mostly passive. Had my eyes closed because I feel that I can then pay attention better.

D. S. 10.00 a.m., Oct. 29, '04.

I anticipate that the thresholds in this record are lower than in the preceding ones because I seemed to attend better to the stimuli, and my cold is considerably better. At the side the sounds usually came in pulsations corresponding to the heart beats felt in the ear, and the distance of the receiver seemed less at the side than in front or in the back. It seems also that somehow it was easier to hear the sounds from the side, that is, I feel sure more quickly of hearing the sound there than at the back or in front.

D. S. 10.00 a.m., Nov. 1, '04.

Had eyes closed. Felt fresh and vigorous. Mind wandering was most disturbing at 75°rb, 30°rb, 15°rb and 0°b. At these positions there was a dull feeling in my ears. Sounds at the side seemed more piercing and usually came by pulsations. The standard of certainty was the same throughout and about the same as in previous tests. It seemed also that the process of perception and the feeling of certainty were simpler and easier at the side than either in front or in the back.

D. S. 10.00 a.m., Nov. 3, '04.

Felt fresh and bright. Had eyes closed. Sometimes the sounds seemed to come in by pulsations but I had no especial associations with them. The attention fluctuated more than in any preceding test. Often I was attending only passively to the stimulus. Frequently at the side and in front I seemed to hear the sound all the time and consequently it was difficult to stick to a standard. My visualization which is always present underwent several very noticeable changes. Beginning with 0°b in this test I visualized a line extending from the back of my head to the receiver. At 30°rb this changed and the line seemed to extend from the right ear to the receiver, until we came to 15°rf and there the line seemed to extend from the center of my forehead to the receiver.

D. S. 2.30 p. m., Jan. 28, '05.

I was in excellent condition for the test. Had eyes and mouth closed. There was a tendency toward mind wandering during the first fifteen minutes, covering the first three points. The rising of the sound to and above the thresh-

old reminded me of breathing that begins slowly and increases in rapidity and volume as the air is expelled from the lungs. The visualization had the same peculiarities as in the last record. There was a slight tendency toward rhythmic response.

D. S. 8.00 a.m., Feb. 1, '05.

Was not subject to hallucinations so much as before. Noticed very distinctly the changes in the process of visualization, mentioned before. I felt very sure of such changes and possibly they may have been a cause for the difference of threshold in different directions. I also noticed clearly a feeling of strain in my right ear when I listened 'mainly with that ear,' i. e., on the side, which I did not notice in the front or in the back. I also felt quite sure that subjectively the main difference between the side and front or back was that I felt certain more easily and quickly in the measurements on the side, as to whether I heard the sound or not; and possibly that may be one reason for the threshold being lower at the side than in front or in the back. I seemed to feel more at ease when the sound was at the side.

D. S. 8.00 a.m., Feb. 3, '05.

Was somewhat disturbed by hallucinatory sounds especially at the first few points in the beginning of the second half. Also noticed a decided feeling of strain and heaviness in both halves in the range of 45°rf to 45°rb. In the first half at 75°rf it seemed that the sound became a little higher in pitch and apparently remained there during the rest of the experiment. Sometimes it seemed that the perception of the sound was not so much the observation of a sound coming above the threshold as a gradual discrimination of it, as it became louder, from the subjective sounds which were conspicuous on account of the quietness of the room.

D. S. 8.15 a.m., Feb. 8, '05.

Began the test with a special determination to make as uniform a record as possible, that is, to make the mean variation as small as possible and have the standard of certainty the same throughout as well as I could. Again noticed very distinctly the change in visualization. Also observed that as the sound came into consciousness I heard the overtones before I heard the fundamental. First I heard the fifth, then the third, and finally the fundamental. Sounds scarcely ever came above the threshold in any other way in today's record, especially in front and on the side. Did not notice it so frequently when the sound was in the back.

N. C. 3.30 p.m., Nov. 8, '04.

The quality of the sound seemed constant, but at 0°f and 0°b the sound was hardest to hear. The places where it was hardest to hold the attention were about 45°rf and 45°rb. From 45°rb to 0°b the distance seemed twice as great as in the other directions. Fatigue was not noticed until the last quarter of the experiment.

N. C. 3.30 p.m., Nov. 9, '04.

Several images (associations) were noticed; at one time the sound seemed like the singing of a mosquito, and again like the buzzing of a track when the cars or train is a long distance away.

C. E. S. 8.00 p.m., Oct. 22, '04.

Kept mouth open because of cold. Felt more wearied just before the middle than at any other time. The first mind wandering was in the first round at 45°rb. Kept eyes closed.

The sound had entirely different qualities in different directions. In the back I do not get certain overtones. There is a point about 30°rb where there is difficulty in choosing whether to listen to the overtones or to the fundamental. The sound comes in by pulsations, about three a second, which may be heard as long as the sound lasts. The certainty is not great. I was more certain in the front quadrant than in the rear; that is, the sound seemed more distinct, rather than a change in my standard of certainty.

C. E. S. 9.00 p.m., Nov. 1, '04.

Kept mouth closed all the time. Very few trials had to be repeated. In the first half I seemed to judge by fainter standards than last time. In the last half of the latter part of the record, the standard seemed a good deal clearer than before but it was impossible for me to hear it until it had this clearness. The sound came in as beats. These beats had association with distant sounds, e. g., it was difficult for me to avoid thinking of the sound as coming from a rooster, a dog, distant singing or speaking. There were periods when I judged by these associations instead of thinking of the sound as a meaningless threshold sound. The difference in quality was not so marked this time as last. Both active and passive attention are present—the former only for a short period. Can it be that the beats are due to the 'vibration' of active effort? I located the sound in one ear all the time. The perception of direction was much less certain than for strong sounds. The sound was of higher pitch than the fundamental. Did not determine what it was—perhaps it was the fifth or octave.

H. S. B. 9.00 a.m., Nov. 8, '04.

In the experiment I heard overtones rather than the fundamental tone from 60°rf to 60°rb. In many cases I heard only a sort of fluttering noise which did not seem to have tone, but the presence of which could be distinctly detected. In nearly all cases the beginning of the sound as it came above the threshold was indefinite; but cessation of the sound was much more clearly distinguished. This gave a feeling of satisfaction in the reater certainty that the sound had really been heard and was not an illusion.

Most of the sounds were localized somewhere within my skull, some being rather high and to the front, and others being in the lower back part. About 45°rf and 45°rb there was a less degree of certainty as to when the sound began and stopped than was true of other locations.

In order to procure more specific information on some points the following list of questions was submitted to the observers after all the tests had been completed:

1. Did the sound seem different from different directions? Did you notice overtones?
2. Did you have any associations with the sound? Did the sound come by pulsations?
3. Did the receiver or source of sound seem to change in distance for different directions?
4. Was it easier to perceive the sound in some directions than in others? Which and Why?
5. Did your standard of certainty in perceiving the sound change?
6. Was there a tendency toward hallucinations and rhythmic response?
7. Did you visualize the position and direction of the sound? Was the visualization different for different directions?
8. Did you have difficulty in attending to the stimuli?
9. Did you notice any motor sensations?
10. Eyes and mouth closed or open?

The more significant observations brought out by the introspective notes and the replies to the questions may be summarized as follows:

In the beginning of the tests there was more or less uncertainty as to just when to feel sure that the sound was heard. But as the experiments progressed a fairly constant degree of confidence was adopted by each observer.

In several instances the sound was heard in pulsations, about three per second in one case, and apparently correlated with the heart beats in another.

The attention fluctuated very noticeably in most cases, which is indicated not only by these introspections but also by the actual fluctuation of sensitivity (see Figure 5). The introspections also seem to agree that it was more difficult to hold the attention during the measurements in the lateral directions than in the other directions. Some claimed to be able to pay attention better when the eyes were closed and others when the eyes were open.

With some observers visualization of the source of sound and associations with the sound were very prominent and apparently of significance to them in the process of perception. Several striking contrasts and changes accompanied the measure-

ments in the different directions. The lateral directions were characterized by entirely different visualization processes than the anterior or posterior directions.

Interpretation of the curves—The three facts which demand consideration are, that the threshold for front and back is approximately the same, that there are two types of persons in respect to the relative keenness in front and in the back, and that the threshold for the side is decidedly keener than for either front or back.

The first fact plainly shows the error of the prevailing belief and statement sometimes made that we hear sounds from the front better than from the back. If we notice the conditions present in these two directions, front and back, and the similarity in their location, we are prone to ask, why should the two be very different? Indeed the agreement in the keenness in these two directions, might have been predicted on theoretical grounds, inasmuch as these two directions are located symmetrically with respect to the ears, and the ability to localize sounds in these two regions is the same.[1] The belief that hearing from the front is finer than from behind undoubtedly rests partly upon the shape of the concha and partly upon the observation that we tend to face the source of sound when we wish to hear well. An observation made by v. Kries[2] undoubtedly finds its explanation in the above results, "———wir konnten z. B. nicht finden, dass etwa der schwächere Klang mit Vorliebe nach hinten, der stärkere nach vorn verlegt worden wäre."

In regard to the keener sensibility at the side let us first consider the introspective remarks on the characteristics, conditions, and processes of perceiving the sound in the lateral directions. An observation frequently recorded by the majority of the observers is that the sound from the lateral directions seemed nearer than from the anterior or posterior directions. But this is simply a naïve statement of what is objectively shown by the experiments, namely that since the threshold is lower at the

[1] See localization charts in Part I.

[2] Ueber das Erkennen der Schallrichtung. Zeitsch. f. Psych. u. Physiol. d. Sinn., I., 1890, 246.

side a sound is comparatively higher above the threshold, that is, relatively stronger and hence nearer.

A still more significant fact is the repeated statement in the introspective notes that it is 'easier to perceive the sound' when it comes from a lateral direction. The same observation was stated in various ways. 'I feel more at ease,' or 'more confident' when the sound is on the side. It is 'more piercing' on the side. 'Hard to pay attention,' or 'was not so sure in response when the sound was behind me.' 'Hardest to hear in front and back.'

Then there were striking changes in the forms of visualization in symmetrical places in the front and rear quadrants. For example, see above the note of D. S., Nov. 3. The shifting of the visualization process and the changes in the facility of the attention seem to have accompanied each other. The question arises, were there similar changes in the process of perception in passing from anterior or posterior directions to lateral directions? One observer mentioned (see above H. S. B., Nov. 8), that the overtones in the directions between 60°rf and 60°rb were much more prominent. The fact that the source of sound at the side is more favorably located with reference to the ear on that side no doubt accounts for the lower lateral threshold and for the qualitative changes mentioned by the observers. Do we not actually take advantage unconsciously of the keener sensibility on the side? In an audience one may frequently observe people trying to hear the speaker better by turning the side of the head toward him.

The individual difference among the observers in regard to the acuity for front and back divides them into two distinct types: (a) those who have greater keenness in the front and (b) those who have greater keenness in the back. The discovery of this difference in the first few tests led to the two extended series of records shown in Figures 1 and 2. The object was to determine whether this distinction would be maintained permanently or whether it was only an individual deviation which would be counterbalanced by additional tests. But the results show that the two types were clearly maintained. The difference for front and back for these two types is .5 for

N. C., Figure 1, in favor of front, and 1.3 for D. S., Figure 2, in favor of back. This difference is even more conspicuous in the monaural records of the same observers, Figure 4, being 2.7 for N. C. in favor of front and 1.3 for D. S. in favor of back. The amount of difference is too great to be accidental, as in each case except the first it is more than one unit of measure on the audiometer.[1]

What accounts for these two types? One factor that might be suggested is undoubtedly the differences of the anatomical structures of the ears, especially the pinnae. A slight difference in the course of the meatus and in the adjoining structure may also possibly render the perception of sound easier from the rear in one individual or easier from the front in another individual.

Another reason for the keenness ofdetecting sounds from the rear may be sought in the phylogenetic development of the race. The ear rather than the eye has been the means of detecting sources of danger in the rear, and consequently the auditory habits have adjusted themselves to serve this purpose.

Only one series of directions, the horizontal plane through the aural axis, was tested in this series, but the results may safely be generalized and applied to the vertical planes as well, on the ground that there is uniformity in the localization records for the horizontal and vertical planes and uniformity in general conditions for these two sets of planes. If one of the composite curves, Figure 3, were revolved on the point 90°r as the center in such a way that its axis would coincide with the aural axis, it would generate a saucer-like surface which would probably represent the keenness of sensitivity for all possible directions of the right hemisphere and analogously also for the left hemisphere.

Monaural Threshold.

In order to measure the monaural threshold and to compare it with the binaural, six monaural records were obtained from N. C. and D. S. The conditions and method of measurement

[1] One unit can be perceived as an increment with a fair degree of certainty.

were exactly the same as in the preceding experiments, excepting that the left ear was heavily bandaged.

Table II gives the individual records. In Figure 4 the average monaural and binaural curves of the two observers are presented together for direct comparison. The binaural curves are taken from Figure 3.

Table II.

N. C.

0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
13.8	12.5	11.9	12.0	10.8	10.3	10.0	10.8	11.7	12.2	14.0	15.8	16.9
13.2	11.6	11.3	10.7	10.3	9.2	9.2	9.0	9.8	10.8	11.7	12.0	15.0
12.2	11.9	10.6	10.5	9.7	8.7	8.6	9.3	9.5	11.0	12.7	14.4	15.6
13.1	12.0	11.3	11.1	10.3	9.4	9.3	9.7	10.3	11.3	12.8	14.1	15.8

D. S.

16.5	14.9	13.7	13.2	11.4	10.8	10.7	10.8	11.1	11.5	12.5	13.2	14.6
16.8	16.8	15.3	13.9	12.5	11.1	11.3	11.7	12.4	12.1	12.5	13.8	14.8
15.5	15.7	14.5	12.3	11.1	10.8	11.6	11.9	12.4	12.8	13.5	14.2	15.5
16.3	15.8	14.5	13.1	11.7	10.9	11.2	11.5	12.0	12.1	12.8	13.7	15.0

The mean variation for these records is about the same as for the binaural measurements, about .6.

In general the results present no new features: front and back are approximately on the same level; both observers maintain their respective types, N. C. with lower threshold in front and D. S. in back; the side is considerably keener than either front or back.

The monaural threshold in the lateral directions is practically the same as the binaural, but in the anterior and posterior directions the monaural threshold is a little higher. The ratios of side to front and back in the monaural tests are .64 (N. C.) and .69 (D. S.) while in the binaural tests they are .73 (N. C.) and .72 (D. S.). Evidently the exclusion of the left ear does not affect the lateral thresholds on the right side, but the anterior and posterior are slightly higher.

It is evident then from the close agreement of the monaural with the binaural thresholds that the acuity of hearing is not dependent upon any coöperation of the two ears so far as the hemisphere on the side of the active ear is concerned. On the side of the hearing ear we hear as well with one ear as with two. But in respect to discriminative processes, such as are involved in the auditory perception of direction, the combined action of two ears is decidedly better than monaural perception. This will be considered later in greater detail.

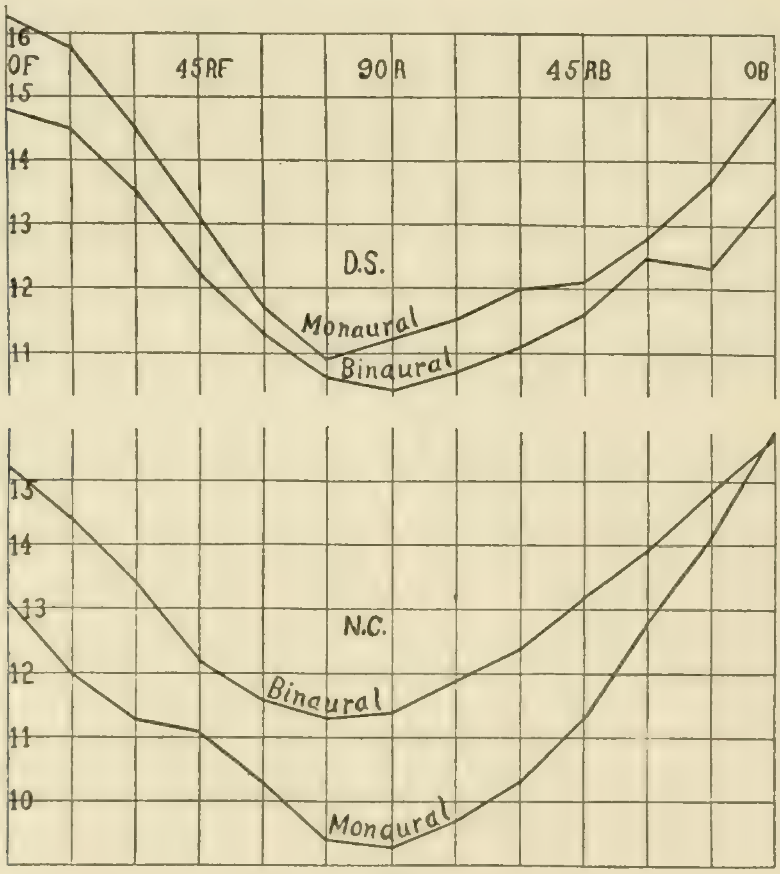


FIG. 4

The Bearing of the Threshold Experiments upon the Localization of Sound.

The localization of sound has been shown to be much less accurate on the side than in front and in the back. The threshold of hearing, on the other hand, is considerably lower, that is, keener on the side than in front and in the back. The curves of localization and of the threshold measurements take

directly opposite courses. Hence, acuity and localization do not run parallel and are not direct functions of each other, nor are the two curves reciprocals of each other. The ratio of the thresholds of the side to front and back is, as shown above, 3 to 4, while the same ratio for the localization is about 4 to 1.

Localization depends upon discrimination, while hearing ability or acuity is an expression of sensitivity. Localization is primarily a binaural process while hearing ability is mainly a monaural matter. Localization is most accurate in anterior and posterior directions where the coöperation of the two ears is at a maximum; it is least accurate in the lateral directions where the coöperation is at its minimum. If one ear be excluded the localization will be greatly impaired in those directions in which the coördinating activity is most important,[1] but on the other hand, the hearing ability will remain practically unaffected. Localization involves processes of discrimination and is primarily binaural, just as the perception of visual space and volume is dependent largely upon binocular vision, whereas the sensibility to light is probably as acute in one eye as in two. Of course, monaural localization is possible just as monocular space perception is possible, but it is not as accurate and reliable.

Sensibility and discrimination do not necessarily depend upon each other nor vary together. A person with a low threshold will not necessarily localize more accurately than one with a high threshold.

The significance, then, of the present threshold measurements for localization is this: They demonstrate precisely what had been merely a supposition, namely, that a sound on the side in the region of the aural axis does seem stronger and nearer than in front or in the back, and of this difference we have obtained a quantitative measurement. A sound seems stronger when near the aural axis than when farther away and these variations in intensity are potent factors in rendering our perception of direction accurate.[2]

[1] See the last series in this report on monaural localization.

[2] These results corroborate the discussion in Part I, 17 ff. Further, the equal sensitivity to sound in front and in the back contradicts the popular assumption that we tend to place weak sounds toward the rear and loud sounds toward the front.

General Observations.

There are two main individual differences in auditory sensibility. First, in respect to the relative acuity in front and in the back, individuals are divided into two classes. Second, the threshold is much higher for some than for others. Compare for example the records of C. E. S. and E. G. Q. in Table I. The threshold of the latter is about twenty units higher. This may be due to differences in mental attitude during the tests, in the standard of certainty in responding, or in the anatomical structure of the sense organs.

The auditory acuity is greater on some days than on others; compare, for example, records 6 and 10 in Figure 1. These daily fluctuations find their explanation in the variation of subjective conditions and perhaps also to a slight extent in the unavoidable objective variations.

The records on successive days[1] show some improvement. There is a tendency for successive curves to be progressively lower in the scale. This improvement is probably not in the sensibility of the ears but rather in increased familiarity with the sound and consequently increased power to direct and control the attention in the experiments, and possibly also in the adoption of a fairly constant standard of certainty. The uncertainty as to whether the sound was heard or not, disappeared after the first record. The improvement is thus due to practice, and to familiarity with the situation of the experiment, rather than to any increase in the actual sensitivity of the sense organ.

The recent demonstrations of fluctuations and periods in mental acuity and application[2] show that these factors enter into the results of all forms of psychological experiments and particularly into continuous work such as was required in these threshold measurements. Each record represents practically continuous activity for approximately forty-five minutes. The

[1] The order of the records is indicated by the small numbers at the left end of the curves.

[2] Kraepelin, "Die Arbeitscurve." Phil. Studien, XIX, 1902, 459–507. Seashore and Kent, "Periodicity and Progressive Change in Continuous Mental Work." Univ. of Iowa Studies in Psych., IV, 1905, 47–101.

only breaks occurred when the observer had to turn from one direction to the next. Although no special effort was made to follow any set time, the stimuli and the reactions followed quite uniformly at intervals of from 5 to 8 seconds.

To show the fluctuations and to bring out the types of periods discovered by Seashore and Kent the following two curves, Figure 5, are presented from the records of N. C. and D. S. Each curve represents one complete record. The points in the zigzags are individual reactions. The horizontal bars represent the averages of groups of the ten determinations which were made at a time in a given direction. The breaks in the curve between the groups of tens represent the change in position on the part of the observer from one direction to the next. Each curve is composed of two parts, being the two parts of the double fatigue order from 0°f to 0°b and then from 0°b to 0°f. The continuous line in each curve is the composite of the ten records obtained from the two respective observers, and is to be regarded as the basis upon which the zigzag curve is drawn, the latter being one of the ten records in detail. The composite curve is used as the basis because all the detailed fluctuations are obliterated in it and it thus affords the only standard by which to detect the individual deflections of the separate records. It must be borne in mind that the wave-like form of the average lines is the typical variation in the threshold for the different directions as pointed out above, and not the fluctuation in mental work. Regarding the continuous lines as the base lines we notice that each record shows two complete waves of fluctuation. For example, the upper curve begins above the base line, gradually falls below it, rises above again at 0°b, then falls below reaching its lowest point at 45°rf when it rises to the level of the base line at 0°b. These large waves correspond to the small hour waves of Seashore and Kent.[1] The zigzags of the separate reactions probably coincide with the crests of the "second-waves" six to eight seconds long, which was approximately the period of one reaction. Then in the groups of tens there are frequently smaller groups of two to seven reactions

[1] Op. cit. p. 55.

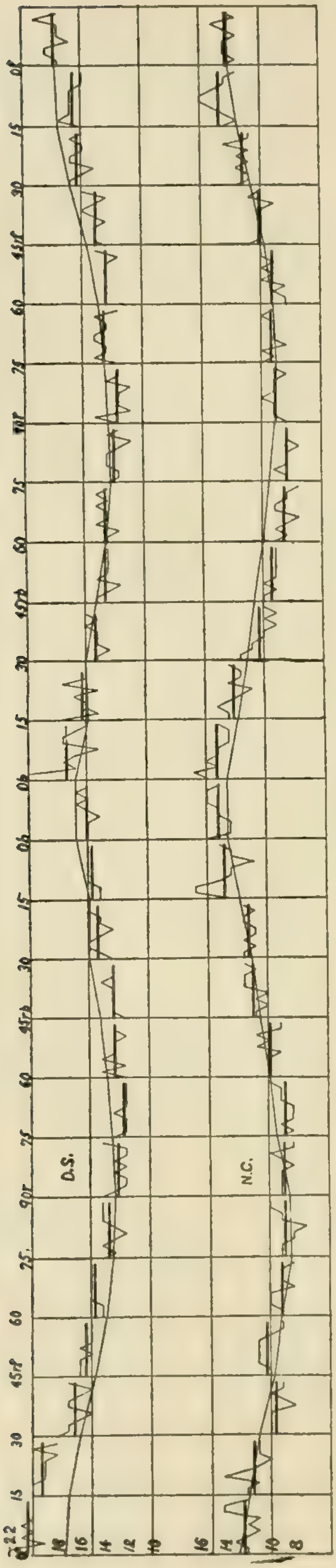


Fig. 5

which are either all above or below the horizontal bars. These correspond to the 'minutes waves.'

The periods of drowsiness and difficulty in paying attention which were experienced by the observers during the experiments probably coincide with the larger fluctuations. From the point of view of the present tests these fluctuations in mental work are disturbances and account for the deviations of the individual curves from the averages. But their effect is not serious because it is neutralized in the composite curves.

SERIES II.

Discrimination of Intensity and Pitch in Different Directions.

The differences in threshold in different directions as found in the last series of experiments suggested the problem as to whether the keenness of discrimination between intensities or pitches or other qualities of sound would also vary with different directions. The aim of this series of experiments was to determine whether the discrimination varies in some regular manner for the series of directions in which the hearing ability measurements had been made.

Discrimination for Intensities.

The apparatus, accessories, source of sound, location and distribution of the apparatus, were exactly the same as in the threshold experiments. The only difference was in the method.

Before the regular tests were begun the threshold of hearing was found for each observer and then a sound ten units stronger (on the audiometer scale) was chosen as the standard intensity. This sound was strong enough to be easily heard.

In making the measurements the experimenter gave the standard sound for one second and then after an intermission of one second, sounded for one second either the standard or the sound one unit stronger. The problem for the observer was to deter-

mine whether the second sound was the same or a stronger. If he judged it to be the same he responded with one tap on the strap key; if stronger, with two taps. The increment of one unit was found to be large enough to be perceived as an increment and yet not too large to make the observer absolutely certain. Ordinarily about 75% of the judgments would be correct.

Twenty determinations were made in one sitting, in the double fatigue order, for each one of the thirteen directions on the right side. This constituted one record.

The tests were made on three observers (men), C. E. S., E. A. R. and D. S. For the first observer, the standard intensity on the audiometer scale was thirty, for the other two, twenty-five. Two of the observers made three records each, and the third made two records. Thus 160 determinations were made for each direction, or a total of 2,080.

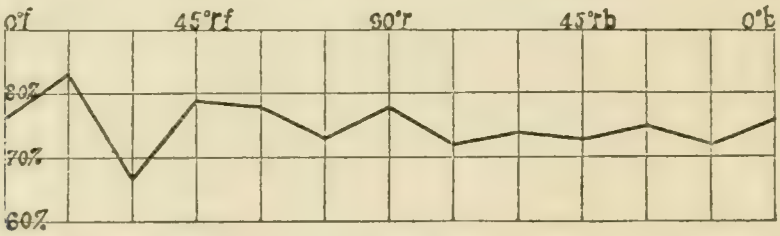


Fig. 6

The results of these measurements are represented in the curve of Figure 6, which is the composite of all the records. The points in the curve were determined by calculating the percentage of correct responses.

It is evident from a glance at the curve that the discrimination between intensities is the same in all directions, each showing about 75% correct judgments. If a still larger number of records had been obtained the curve would probably have become almost a straight line. The results, although largely negative, corroborate the suggestion made above that keen sensitivity is not necessarily accompanied by accurate discrimination. It also demonstrates that the greater accuracy in localization in front and in the back than at the side, is not due to greater accuracy in discriminative ability in those directions.

Discrimination between Pitches.

The apparatus employed for the purpose of determining the discriminative ability between pitches was a set of tuning forks customarily used in the Iowa laboratory. It is a set of eleven forks of uniform size and shape—11.5 cm. long—tuned to produce successive increments above the pitch of international A, 435 v.d. as follows: ½, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v.d.

The sound of the forks had to be augmented by a resonator because the source of stimulation had to be at a distance of one meter from the observer's head. An upright glass tube served as resonator. The experiments were made in the quiet room of the laboratory.

In making the tests the standard fork—435 v.d.—and one of the differential forks were sounded in rapid succession by striking them uniformly and holding them over the resonator. The observer was allowed a choice of only two answers, namely whether the second tone was higher or lower than the first. The approximate discriminative sensibility of each observer was found by a few preliminary trials. Ten trials were made at one time in one direction. If less than 70% were correct a larger increment was taken, if more than 80% were correct the increment was decreased. Twenty trials for each of the thirteen directions were made in one sitting in the double fatigue order.

The increment required for 75% correct judgments was calculated from Fullerton and Cattell's table.[1] The narrow limits of 70% and 80% were chosen as the data for the calculation in order to make the calculated increment reliable. An increment calculated on the basis of a record in which 60% or 90% are correct would not be empirically valid, especially when the increments are so small as in the present tests, being within .5 and 2. v.d.

Twelve records were obtained—from C. E. S. one, from E. A. J. six, and from D. S. five. These gave 240 trials for each direction or a total of 3,120.

[1] "On the Perception of Small Differences," Univ. Penn. Phil. Series, No. 2.

The variation of the observers from one another was within a narrow range (.8 to 2.1 v.d.) so that it is not necessary to present the records individually. They are summarized in the composite curve in Figure 7. The abscissae represent the directions and the ordinates represent the amount of difference in pitch, measured by vibrations.

The interesting feature of the curve is that the discrimination is considerably poorer on the side than either in front or in the back, the front is slightly better than the back. Although the difference in discrimination between 0°r and 0°f is only .5 of a vibration, it is relatively large, amounting to over 40%. All the

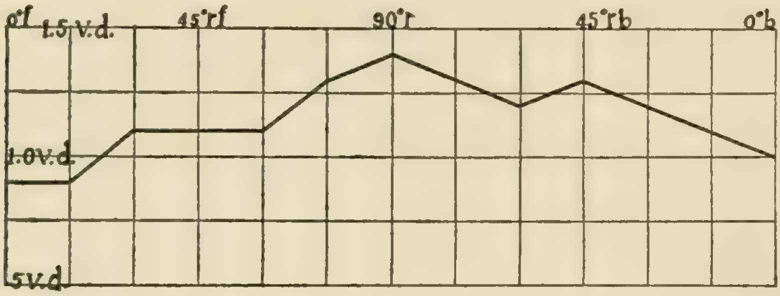


Fig. 7.

records agree in this respect. As an explanation of the poorer discrimination at the side it might be suggested that the ear probably catches more overtones when the source of sound is on the side. This may be confusing and consequently cause poorer discrimination.

The localization of sound is more accurate in front and in the back than on the side. Likewise the discrimination between the qualitative characteristics of tones, according to the pitch tests, seems to be better in front and in the back than on the side. It appears reasonable to infer that the greater accuracy of localization in front and in the back may be due in part to the greater accuracy in discriminating between the qualitative characteristics of sounds in those directions.

SERIES III.

Quality[1] of Sound in Localization.

It became evident from the experiments on localization described in Part I that the character of the sound is of considerable importance as a datum for discerning direction. As demonstrated by some special tests[2] the sound seemed clearer, richer, and fuller in some directions than in others, and these qualitative differences are constantly made use of unconsciously to differentiate the directions of sound.

The problem then is, can a sound whose complexity and qualitative characteristics are reduced to a minimum be localized as accurately as a richer and more complex sound? To what extent does localization depend upon these characteristics?

It is quite evident that this problem can be approached by choosing a variety of stimuli differing as widely as possible in their nature and complexity, and comparing the accuracy with which they can be localized. The following stimuli were selected.

a. The singing flame. This was produced in the usual manner. Ordinary illuminating gas, containing about 60% hydrogen, was used. The resonator tube (glass) was 25.5 cm. long and 1 cm. in diameter. The height of the flame was 8 mm., and the pitch of the sound about 730 v.d.

It was necessary in the experiments to interrupt the sound when moving from one direction to another. This was accomplished by shutting off the supply of gas for an interval long enough to stop the tone, after which the flame would again rise to its usual height and produce the sound. Although the singing flame is not strictly pure it was considerably purer than any one of the other sounds used.

b. The Galton whistle. Three pitches were used, 10,000, 20,000 and 30,000 vibrations. The air current was supplied by constant pressure tanks.

[1] The term quality is here used as physicists use it, in the sense of timbre.
[2] Part I, 18 ff.

c. The voice. The word 'now' was pronounced with the intensity and clearness of ordinary speech.

d. An electric hammer. A small wooden hammer, 2.5 cm. long and 2 cm. in diameter, was struck against a block of wood 5.2 cm. long and 2.7 cm. in diameter. The hammer and block were mounted on an ordinary electric bell, the block was put in place of the gong and the hammer fastened to the armature. The automatic make and break was removed so that the strokes of the hammer could be controlled as the experiment required. The use of magnets in the circuit of a constant current by which the hammer was struck made it possible to produce strokes of uniform amplitude.

e. A clapper. A small board, 12.5 cm. x 6 cm. x 5 cm., and another piece of wood, 15 cm. x 2 cm. x 1.5 cm., were fastened together at one end by a hinge and a spring. By opening the clapper and releasing one wing, it would strike against the other wing producing a clashing noise. The clapper was manipulated by a string.

f. A whiff of air. The air supplied by pressure tanks was conducted through a rubber tube terminating in a small glass jet. The whiff was produced by opening and closing the rubber tube which furnished the air current.

These various devices for producing stimuli were attached to the arms of the sound perimeter. In case of the voice the experimenter stood in such a position that his mouth took the place of the mechanical devices. The method followed in the localization experiments of Part I was also employed here and the results were treated in the same way.

Three representative directions were chosen, 0°f, 45°rf, and 90°r, in the horizontal plane through the aural axis. It was not necessary to test more than these three directions for the purpose of comparing the different stimuli, because they represent the typical extremes in localization, and as demonstrated by the earlier experiments the curve of the right front quadrant is symmetrical with the curve of the right back quadrant.

The results are summarized in Tables III–V. The observers and the number of trials are mentioned in connection with each table.

TABLE III.

The Singing Flame.

Observers.	0°f	45°rf	90°r
N. C.	10.6	37.4	34.5
N. B.	6.4	15.8	29.8
H. S. B.	9.5	30.8	42.7
E. A. J.	14.9	30.0	*
Averages	10.3	28.5	35.7

* The discrimination at this point seemed to be too crude to make a satisfactory measurement.

TABLE IV.

The Galton Whistle.

Observers.	Pitch	0°f	45°rf	90°r
C. E. S.	10,000	5.4	*	46.1
	20,000	3.1		29.0
	30,000	6.2		54.6
D. S.	10,000	2.9	9.2	43.5
	20,000	2.5	5.8	34.8
	30,000	2.6	5.1	25.0
R. W. S.	10,000	2.1	12.5	28.5
	20,000	1.7	7.5	18.7
	30,000	1.7	8.0	23.1
A. K.	10,000	6.2	6.5	50.0
	20,000	8.8	13.1	35.9
	30,000	5.0	12.2	40.8
Averages	10,000	4.1	9.4	42.0
	20,000	4.0	8.8	29.8
	30,000	3.4	8.4	35.9

* No tests were made.

TABLE V.

Noises and the Voice.

	0°f				45°rf				90°r			
Observers.	Hammer	Clapper	Whiff	Voice	Hammer	Clapper	Whiff	Voice	Hammer	Clapper	Whiff	Voice
C. E. S.	1.8	1.5	2.6	1.5	4.4	3.4	7.2	2.6	10.6	37.7	25.6	9.5
D. S.	1.6	1.5	1.5	1.5	4.0	2.2	3.1	2.2	26.3	14.4	7.3	3.6
A. K.	2.4	1.5	1.6	1.5	3.6	3.1	3.6	2.4	10.5	12.8	11.6	9.2
C. L. V.	2.6	2.2	2.4	1.5	4.4	2.5	8.2	2.2	29.0	29.0	25.6	11.6
Averages	2.1	1.7	2.0	1.5	4.1	2.8	5.5	2.3	19.1	23.5	17.5	8.5

With each stimulus 50 determinations were obtained from each observer for each direction, total 2,400.

The figures in the table represent in degrees the just perceptible difference between directions. One hundred determinations were made with each observer for each direction, in all 1200.

In order to compare more directly the accuracy of localization of the different stimuli the results are presented graphically in the curves of Figure 8. The radii represent the directions and the arcs represent degrees of just perceptible difference between directions. The curve shows only the results obtained with the 30,000 pitch for the Galton whistle.

The main results may be summarized as follows: The singing flame, which is an approximately pure tone, is localized very poorly. The high tones of the Galton whistle which are

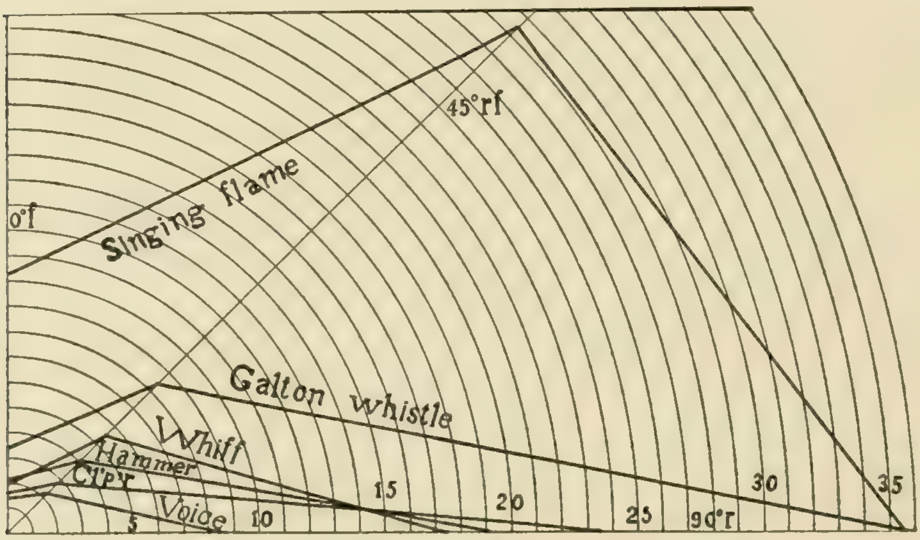


Fig. 8

quite free from overtones are also localized rather inaccurately. The noises are localized more accurately and the voice most accurately. The voice is probably not more complex than some of the noises but its overtones are more regular and continuous and within the customary range of hearing. In general, then the richer and more complex a sound the more accurately can it be localized. The complex sounds are the most frequent sounds in our experience, and the two factors, frequency and complexity, have coöperated in our learning to localize them

most accurately. These experiments corroborate the results obtained by Professor Angell in so far as a comparison can be made.[1]

The problem of quality was approached in still another way. An effort was made to obtain a tone that might be purer than the singing flame. So Helmholtz's method of using a fork and a resonator was adopted.[2] A fork of 250 v.d. and a Helmholtz resonator were mounted on a convenient wooden frame, which was attached to one of the arms of the sound perimeter. All points of contact were supplied with soft rubber in order to avoid any resonance from the frame. To eliminate the buzz caused by the interrupting contact, the current was interrupted by another fork of the same vibration frequency, located in a distant room. A double circuit was arranged so that the driving fork would be running constantly and yet at any moment the current could also be sent through the magnets of the stimulus fork for any desirable length of time. The tone thus produced was a close approximation to a pure tone, at least none of the other resonators in the Helmholtz series brought out any tones.

For the purpose of comparing the localization of this tone with a complex tone the sound from an ordinary telephone receiver was used. The current for this was interrupted by a 100 v.d. fork. The rest of the apparatus consisted of the sound perimeter by means of which the stimuli were controlled.

The method of experimentation may be described as a type of the method of production. Twenty-four directions, 15° apart, in the horizontal plane through the aural axis were used. The observers were told before the experiments that the stimuli would come from any of these points. The stimulus was given from one of the points and the observer then answered by means of the adopted nomenclature with which each observer had been made familiar before the experiments.[3] The experiments with the fork and the telephone were made simultaneously, that is,

[1] Angell, "A Preliminary Study of the Significance of Partial Tones in the Localization of Sound." Psych. Rev., X, 1900, 1–14.

[2] Helmholtz, "Sensations of Tone." Trans. by Ellis, third ed. 120, 339.

[3] See Figure 2 in Part I.

an equal number of determinations were made with both stimuli in each sitting. This was done in order to make the effect of practice and other conditions uniform for both. Thirty-six determinations were obtained for each of the twenty-four directions from six observers, four men and two women, in all 864 trials.

The results are given in Table VI.

TABLE VI.

Telephone.

Observers.	Number correct	Misplaced 15°	Confusions	Right-left misplaced	Others
D. S.	98	43	2		1
F. O. S.	74	56	5		9
R. W. S.	99	41	2		2
F. V.	78	47	8	1	10
E. L.	69	65	1	1	8
C. P.	97	43	1		3
Totals	515	295	19	2	33
Percent	59.6	34.1	2.2	.2	3.9

Tuning Fork.

Observers.	Number correct	Misplaced 15°	Confusions	Right-left misplaced	Others
D. S.	22	31	27	9	55
F. O. S.	12	39	26	14	53
R. W. S.	19	33	22	13	57
F. V.	25	30	16	12	61
E. L.	33	41	13	13	44
C. P.	70	50		6	18
Totals	181	224	104	67	288
Percent	20.9	25.9	12.0	7.8	33.3

The significance of these figures is clear. They serve to emphasize the importance of quality and complexity of sound as data in the perception of direction. The complex sounds of the telephone are localized correctly 59.6% times as compared with 20.9% for the fork. But the difference is even greater when we compare the other columns of the table. More telephone sounds are localized approximately correctly, i. e., misplaced only 15°, so that if we add to correct localizations those

approximately correct we get for the telephone 93.7% while for the fork only 46.8%.

The column headed 'confusions' refers to those sounds which were located in a symmetrical position, or within 15° of the symmetrical position, in the quadrant on the same side. For example, if a sound at 60°rf was located at 60°rb, or within 15° of that, it is counted as a confusion.[1] These confusions are much more frequent with the pure tone than with the complex tone, the telephone 2.2% and fork 12.0%. The column of right-left misplacements refers to confusions, not necessarily symmetrical, of the right and left sides, and these also occurred oftener with the pure tone, 7.8%, than with the complex tone, .2%. The last column contains all those which are not accounted for under the preceding headings. These experiments, therefore, fully corroborate the conclusions of the preceding experiments, that a complex sound is much more accurately localized than a relatively pure tone.

It has been suggested that a pure tone can not be localized. While this cannot be positively contradicted on the basis of the preceding experiments, it nevertheless seems quite probable that an absolutely pure tone can be localized with some degree of accuracy. Even if the qualitative data in the perception of direction were completely eliminated in the case of a pure tone, the binaural ratio of intensities would still remain, by means of which some clue to direction would be indicated.

SERIES IV.

Misplacements in Localization.

Closer scrutiny of the errors and misplacements of the telephone sound reveals interesting tendencies to misplacement toward certain directions. The localizations of the telephone sound are therefore presented in detail in Table VII, in which the objective positions of the stimuli, the number of correct localizations, and the number misplaced (the coefficients) to

[1] Cf. Pierce, "Studies in Space Perception," p. 70.

the designated positions (in parentheses) are given. The four cardinal points, 0°f, 90°r, 0°b, and 90°l, are not included in the summary for reasons given below. The localizations of the telephone sound, rather than of the fork, were used because they are more accurate and bring out the misplacements more consistently and more rigidly.

TABLE VII.

Standard directions	Number correct	Misplacements.					
0°f	32	3(15°rf)	1(15°lf)				
15°rf	17	13(30°rf)	(0°f)	1(0°b)	3(45°rf)		
30°rf	21	12(45°rf)	1(15°rf)	2(60°rf)			55 backward
45°rf	26	5(60°rf)	3(30°rf)	2(75°rf)			15 forward
60°rf	23	10(75°rf)	2(45°rf)	1(90°r)			3 confusion
75°rf	19	7(90°r)	7(60°rf)	2(75°rb)	1(60°rb)		
90°r	19	6(75°rf)	6(75°rb)	4(60°rf)	1(45°rf)		
75$_{o}$rb	12	15(90°r)	3(60°rb)	3(75°rf)	1(45°rf)	2(45°rb)	
60°rb	14	9(75°rb)	12(45°rb)	1(90°r)			45 forward
45°rb	22	5(60°rb)	6(30°rb)	1(75°rb)	1(0°b)	1(15°rb)	39 backward
30°rb	21	8(45°rb)	5(15°rb)	1(75°rb)	1(45°lb)		4 confusion
15°rb	21	4(30°rb)	9(0°b)	1(0°f)	1(45°rb)		
0°b	33	1(0°f)	1(15°lf)	1(15°lb			
15°lb	24	3(30°lb)	5(0°b)	2(0°f)	1(15°lf)	1(15°rb)	
30°lb	23	7(45°lb)	4(15°lb)	1(75°lf)	1(0°b)		
45°lb	23	3(60°lb)	8(30°lb)	2(75°lb)			42 forward
60°lb	13	8(75°lb)	10(45°lb)	4(90°l)	1(75°lf)		30 backward
75°lb	16	14(90°l)	2(60°lb)	2(75°lf)	1(60°lf)	1(60°rb)	6 confusion
90°l	24	7(75°lf)	4(75°lb)	1(60°lf)			
75°lf	20	9(90°l)	6(60°lf)	1(75°lb)			
60°lf	20	8(75°lf)	7(45°lf)	1(90°l)			
45°lf	26	7(60°lf)	2(30°lf)	1(75°lf)			45 backward
30°lf	24	9(45°lf)	3(15°lf)				22 forward
15°lf	22	10(30°lf)	4(0°f)				1 confusion

It is evident that the majority of the misplacements in the anterior quadrants are backward and in the posterior quadrants forward. In other words, there is a strong tendency to shift sounds away from 0°f and 0°b around toward the aural axis. In the front quadrants the sounds are shifted rearward, 100 backward and 37 forward; and in the rear quadrants they are shifted forward, 87 forward and 69 backward.

A possible explanation was thought to lie in the suggested overestimation of angular differences between directions, for the reason that in the nomenclature here used the two most prominent directions are 0°f and 0°b, and all other directions are designated as so many degrees to the right or to the left of these two reference points. For example, if a sound was given at 30°rf the question that at once arose in the observer's mind was, how many degrees is it from 0°f? And similarly in the rear quadrants 0°b served as the point of reference. Since the tendency was to shift the sounds away from 0°f and 0°b it seemed possible that it might be due to an overestimation of angular differences.

Another observation which seems to support this supposition is the fact that two of the observers, E. L. and C. P., show a decided tendency in the rear quadrants to shift the sounds rearward instead of forward as in the case of the other observers. This accounts for the fact that the predominance of the forward shiftings in the rear quadrants is not as great as the rearward shiftings in the front quadrants. In looking for an explanation of these two exceptions it was noticed that these two observers had a natural tendency to use 90°r and 90°l as the two points of reference for the localizations in the rear quadrants instead of using 0°b as the other observers did, while in the front quadrants they used 0°f, just as the others did. If, for example, a sound appeared to be at 60°rb they would almost invariably say that it was 30° back of 90°r. Since this method of designating directions was not contrary to any specific conditions of the tests the experimenter did not object to it. Now the thing to be noted is that if 90°r and 90°l were their points of reference for the rear quadrants the overestimation of the angular difference between these points and any given direction in these quadrants would tend to shift the sounds backward.

In order to determine this more specifically the tests for the rear quadrants were repeated with these two observers. They were told that they should always locate the sounds with reference to 0°b and not with reference to 90°r and 90°l. The number of determinations is the same as in the original tests so that the results are directly comparable.

E. L. made 8 forward and 10 backward misplacements in the original tests. The suggestion to change the method of designating the directions seems to have been successful at least t; the extent of having approximately the same number of misplacements in each direction. C. P. made 3 forward and 25 backward misplacements as compared with 7 forward and 22 backward in the original tests. Here there is no essential change in the results. The suggestion to change the method does not seem to have been sufficient to overcome the more natural method of designation.

Some special experiments were planned to test in a more crucial way the possible overestimation or underestimation of angular differences between directions. Several small angles were chosen in two regions, in front and on the side, as follows: 3°, 5°, 10° and 15° in front with 0°f as the standard; and 10°, 15°, 20° and 30° on the side with 90°r as the standard. These experiments were made with the sound perimeter, using the telephone sound as stimulus.

The method of average error was followed. For instance, to obtain estimates of the angle 5° in the anterior region, the experimenter gave the stimulus at 0°f, then moved the receiver 5° either to the right or to the left and there gave the sound again in the same way, and then moved the receiver back to the original position. The observer was then told to open his eyes and by means of a pointer push the receiver from 0°f to the point where he thought the second sound was. Then the reading in degrees was taken and recorded. The observers were told that the angles varied in size but did not know the size of the angles, nor that a definite series of angles had been chosen, nor were they told of the nature of their judgments until the tests were finished.

Table VIII gives the results. Each observer gave twenty estimates of each angle, total 440. The figures in the table are percentages of overestimation or underestimation, plus meaning overestimation, and minus underestimation.

The results are decisive. The angles in front are considerably overestimated and those on the side are decidedly underestimated. There are only two exceptions and these are small.

TABLE VIII.

	0°f				90°r			
Angles	3°	5°	10°	15°	10°	15°	20°	30°
G. K.	60.	54	33.	32.2	..	−22.	−22.	−22.5
P. K.	26.	20.5	4.	−1.5	−45.	−38.5	−36.5	−11.5
P. L.	..	55.	16.	4.8	5.5	−16.6	−25.7	−34.3
Averages	43.	43.2	17.7	11.8	−19.8	−25.7	−28.1	−22.8

The angles chosen for the region in front are smaller than those used on the side for the reason that our discrimination between directions is there much finer.[1] The ratio is about 1 to 4, so that a difference of 3° in front is as easily perceived as 10° or 12° at the side. Two places in the table, one under 3° and the other under 10°, are blank because these differences were too small for those observers to detect with certainty and so no estimates were obtained.

It seems quite probable that this difference in discriminative ability in these two regions accounts for the overestimation and underestimation. To put it boldly, we overestimate in front because we can discriminate between smaller angular differences in direction than we think we can, and we underestimate on the side because we do not discriminate between as small angular differences in direction as we naïvely suppose. It frequently occurred during the course of these tests that in testing, for example, the angle 10° in the lateral region, the observers would say that the difference is very small and then they would move the receiver perhaps 7° or 8°, whereas in testing the same angle in the frontal region the difference would seem considerably greater and consequently the receiver would be moved 13° or 14°. This illusion may appropriately be called the 'auditory small angle illusion.' The overestimation and the underestimation seem to be primarily central processes and not peripheral.

[1] See Part I, Figure 3, p. 10.

SERIES V.

Monaural Localization of Sound.

Monaural localization presents various problems whose solution would throw considerable light upon binaural localization. In the experiments on the monaural localization three classes of observers were employed: (a) Persons in whom monaural conditions were produced artificially, (b) one person whose left ear was partly defective, and (c) two persons who had been completely deaf in one ear for many years.

Artificial Monaural Conditions.

The accuracy of discrimination between directions was investigated. The directions, 0°f, 45°rf, 90°r, 45°rb and 0°b in the horizontal plane through the aural axis served as standards. The modified form of the method of right and wrong cases which has been adopted for all the discrimination tests throughout this investigation was also followed here. The telephone was used as stimulus.

The tests were first made upon two observers in whom monaural conditions were produced artificially by closing the left ear by means of inserting a finger firmly in the meatus of the ear.[1] The intensity of the stimulus was so adjusted that when both ears were thus closed the sound could not be heard at all. The figures in Table IX represent in degrees the smallest differences that could be perceived between directions.

Table IX.

	0°f	45°rf	90°r	45°rb	0°b
C. E. S.	5.8	14.3	17.4	12.0	9.5
D. S.	6.6	7.3	14.5	7.3	11.6
Average	6.2	10.8	16.0	9.7	10.6

[1] The main objection to this method of closing the ear is that the circulation of the blood becomes quite noticeable in the closed ear, and consequently disturbs the attention. But this method was preferred to bandaging because it was almost impossible to bandage one ear sufficiently to exclude the sound.

Twenty-five determinations were made for each direction by each observer. The closing of the left ear considerably lessens the accuracy even on the right side.[1]

The tests were then continued on the left side and the first direction tested was 45°lb. The surprising result was that the observers were wrong in almost every answer. They felt sure that they clearly noticed a difference between the directions and yet their answers were consistently reversed in nearly every case. It was thought that this might be due to some peculiar reflection from the walls of the room. The experiments were, consequently, repeated out of doors on four observers, giving the following results:

TABLE X.

	0°f	45°rf	90°r	45°rb	0°b	45°lb	90°l	45°lf
F. O. S.	4.8	14.3	13.6	8.6	4.8	14.3	20.0	21.8
H. S. B.	3.4	29.0	14.3	12.0	2.9		20.0	7.5
D. S.	4.8	19.0	4.1	3.4	7.3		16.0	8.0
G. P. K.	3.4	8.0	8.0	10.2	4.0		23.3	5.8
Averages	4.1	17.6	10.8	8.5	4.8	(14.3)	19.8	9.0

Twenty-five determinations were obtained from each observer for each direction. The results are very similar to the indoor tests. Three places are left blank under 45°lb because there the same characteristic reversals occurred that were noticed in the indoor tests. None of the observers except D. S. knew anything of this observation before the experiments. Evidently it was not due to reflection of the sound from the walls.

The explanation undoubtedly is that under normal conditions when both ears are in use a sound in front of the standard, 45°lb, seems stronger than one back of it. But when the left ear is thrown out of activity the sound that then seems stronger is the one back of 45°lb, but according to the habitual method it would be located in front of the standard, and the outcome is the reversing of the actual positions of the stimuli because of the reversal of the data of localization. Similar reversals might

[1] Cf. Figure 3 in Part I.

be expected to occur in the left front quadrant but for unknown reasons they did not occur.

One observer did not reverse his answers but made the same observation as the others. Instead of placing the stronger sound in front of the standard he placed it back, evidently for the reason that he was conscious of the fact that the left ear was closed and consequently he thought the stronger sound must be the one nearest to the right ear. The introspection of another observer on the same point corroborates this and clears up the matter of reversals. He remarked: "The stronger sounds I call forward and those weaker back. [This of course reversed the actual positions.] That is the way they seemed when I had my attention on the left ear, but if I placed my attention on the right ear, that is, if I made myself conscious of the fact that I was using only the right ear, I would tend accordingly to correct myself and call the weaker ones forward and the stronger ones back." The other observers had no suspicion of reversals and assumed that most of their answers were correct.

In the next group of tests the aim was to determine the accuracy of localizing the twenty-four standard directions in the horizontal plane through the aural axis according to the method described in Series III. Table XI contains the results.

TABLE XI.

Monaural.

Number correct on the right side: 11 out of 44, or 25%	} 63.6%
Number on right side misplaced 15°: 17, or 38.8%	
Number of confusions on right side: 4	
Number correct on left side: 3 out of 44, or 6.8%	} 22.8%
Number on left side misplaced 15°: 7, or 15.9%	
Number of confusions on left side: 12	

Binaural.

Number correct on both sides: 32 out of 88, or 36.4%	} 81.2%
Number on both sides misplaced 15°: 43, or 44.8%	
Number of confusions on both sides: 10	

The distribution of cases was about equal for the two sides.

These tests were made on four observers, each one going through the series of twenty-four standards, once monaurally

and once binaurally in order to compare the two. In the monaural tests the left side has a much smaller percentage of correct or approximately correct localizations than the right side—22.7% against 63.6%. It has also more confusions—12 against 4. In the binaural tests 81.2% are correct or approximately correct as compared with 63.6% on the right side in the monaural tests, showing that the exclusion of the left ear by artificial means affects the right side to a material extent.

One Ear Partly Defective.

The same two sets of tests were made upon an observer whose left ear had been defective for several years. Table XII gives the results of the discrimination for directions, and Table XIII for the localization of the twenty-four directions.

TABLE XII.

	0°f	45°rf	90°r	45°rb	0°b	45°lb	90°l	45°lf
Out of doors	4.0	14.5	19.0	29.0	5.8	25.3	17.0	12.0
In doors	7.3		14.5		8.0			

TABLE XIII.

In doors. Right side.	Out of doors. Right side.
Number correct: 1 out of 11	Number correct: 2 out of 11
Number misplaced 15°: 4	Number misplaced 15°: 1
Number of confusions: 1	Number of confusions: 5
Left side.	**Left side.**
Number correct: 1 out of 11	Number correct: 0
Number misplaced 15°: 2	Number misplaced 15°: 4
Number of confusions: 1	Number of confusions: 1

The localization is poorer than for the artificially monaural observers, particularly in the results of Table XIII. But there is practically no difference between the two sides, which can be accounted for by two reasons. First, the actual difference in acuity between the two ears was not as great as the observer thought. The threshold as found by the audiometer was 22.6

for the right ear and 37.9 for the left ear. Second, the observer had been defective for a number of years and had accustomed himself to the difference, whereas in the case of the artificially monaural observers the change was sudden.

Completely Monaural Observers.

The two observers, O and B, employed in these experiments were completely deaf in the left ear. Most of the data which are presented were obtained from O, a university student who has been deaf in his left ear since infancy but whose right ear has normal acuity, 16.7 as measured by the audiometer.

B is a middle aged physician who has been deaf in his left ear for thirty-two years. The deafness was consequent upon cerebro-spinal meningitis. Careful diagnosis by an otologist

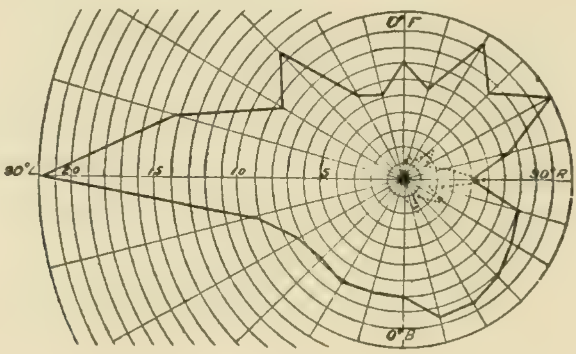


FIG. 9

showed that there is a lesion in the left auditory nerve. Otherwise the mechanism of the ear seems to be in perfect condition. The data obtained from this observer are in Table XIV.

Discrimination for directions—The first series of tests had in view the measurement of the discriminative ability between directions. The same method that has been followed in all the discrimination tests was followed here. The measurements were made out of doors.[1] The curve in Figure 9 contains the results obtained from O. It is based upon 1200 judgments, fifty for each of the twenty-four directions in the horizontal plane through the aural axis.

[1] The apparatus was a simplified form of the sound perimenter, consisting of only one arm and a scale. This was more convenient for outdoor experiments than the perimeter.

The main facts embodied in the curve are: (a) The most accurate region is at 90°r, just opposite the intact ear. A change in direction of four degrees can be detected there. (b) The most inaccurate region is at 90°l, opposite the deaf ear. A change of twenty-two degrees is necessary there to be noticeable. (c) In all the other directions the accuracy of localization is practically the same.[1]

This curve may be regarded as the typical monaural curve, comparable with Figure 3 in Part I as the typical binaural curve, which is represented by the dotted line in this figure.

There are, however, several interesting contrasts between monaural and binaural localization. In binaural localization 0°f and 0°b are the most accurate regions, and 90°r (or 90°l) is the poorest. In monaural localization, on the other hand, 90°r (or 90°l, in persons deaf in the right ear) is the most accurate region, while 0°f and 0°b are very much poorer. The accuracy at 90°r is about the same for a monaural as for a binaural person. This corroborates the statement[2] that in the vicinity of the aural axis the perception of direction depends almost entirely on one ear even in a binaural person.

While the monaural curve reveals some very interesting features and presents a remarkably symmetrical appearance, there is really nothing surprising about it. Its form might almost have been predicted on the basis of the facts known about binaural localization. We should expect the keenness of localization in the region near the intact ear to be the same in monaural as in binaural localization, because there the coöperation of the two ears is at a minimum. We should expect this to be the most accurate region because it is nearest to the active ear. Localization on the side of the deaf ear must necessarily be crude and unreliable because the sounds do not reach the intact ear directly. We should also expect the monaural localization to be much inferior to binaural localization in front and in the back, be-

[1] A few skirmishing tests on B revealed the same facts. His localization at 90°r was quite accurate, fully as accurate as in a normal individual. A change of five degrees could be detected. On the left side localization was quite poor.

[2] See Part I, p. 24.

cause the great accuracy in those directions in binaural localization is due to the maximum coöperation of the ears.

What are the data upon which monaural perception of direction depends? Both observers were careful in their intropsective analysis and agreed closely in their statements. According to observer O, from whom most of the introspective accounts were obtained, the perception of direction depended upon two factors, viz: intensity and his 'sense of direction.' When he was asked why he located a sound in a particular place he would generally attribute it to greater or less intensity than the sound had when it came from another direction. If intensity did not seem to be the means of recognizing the direction, which was seldom, he would reply that he did not know just why he located the sound at that place. And if he was further urged to analyze the basis of his judgment, he would simply reply that his 'sense of direction' told him that it was there. The term 'sense of direction' is undoubtedly an abbreviated expression for a number of unconscious elements, perhaps mostly qualitative characteristics, which were so interwoven in the perceptive process through habit and experience that the observer was unable to disentangle them.

The intensity factors were by far the most important, in the estimation of the observer, but probably not as important as he considered them to be. It is, however, true that in his localizations he seemed to depend very consistently upon the variations in intensity.

He had a sort of system of relative intensities which may be described as follows. There were two poles, one of maximum and the other of minimum intensity, located on opposite sides of the head. The pole of minimum intensity, at which the sound seemed weakest, was at 90°l, opposite the deaf ear; and the pole at which the sound seemed strongest was apparently not at 90°r but just a little in front of it, at 75°rf. All other directions in the circumference between these two poles were distributed according to a regular scale of diminishing intensities from right to left. The observer's judgments seemed to depend almost wholly upon these intensity differences. In determining the position of a sound with reference to the stand-

ard he would locate it on the right if it seemed stronger than the standard, and on the left if it seemed weaker. In all the standard directions from 75°lf to 60°rf in the anterior quadrants, and from 75°lb to 90°r in the posterior quadrants, the sounds on the right of the standard seemed stronger than those on the left of the standard. At 90°l where a sound in front of the standard would have the same intensity as a sound an equal distance back of it, the discrimination between the directions was very crude. Both the one in front of the standard and the one back of it seemed stronger, and consequently it was almost impossible to distinguish between the two. At the pole of maximum intensity on the right side the same conditions would obtain, but it seemed that the qualitative data were more conspicuous in the perception of direction.

In a normal binaural person the monaural process is narrowly limited to the immediate vicinity of the aural axis. The monaural and binaural curves show equal accuracy only at 90°r. At 75°rf and 75°rb monaural localization is poorer.

The results of Bloch,[1] who has made similar experiments on the discriminative ability in the horizontal plane through the aural axis, are different. The only respect in which the curve in Figure 9 agrees with Bloch's curve is in the greater accuracy on the side of the active ear. Otherwise his curve is rather irregular.

The difference between the two curves is probably due to two reasons. In the first place, Bloch's observer was not strictly monaural. Monaural conditions were produced by closing the left ear. This does not insure absolutely monaural localization, and the sudden change by bandaging one ear does not give sufficient time to the individual to adjust himself to the new situation. The results in Table XI obtained under artificial monaural conditions, are quite different from the results of Figure 9. The observer upon whom the experiments here reported were made, had been deaf in the left ear since infancy. Secondly, Bloch's curve is based on only one third as many measurements as the curve in Figure 9.

[1] Bloch, "Das Binaurale Hören," 35.

Localizing the twenty-four directions—The twenty-four standard directions were given in chance order and the observer located the sounds. Table XIV contains the results.

TABLE XIV.

Positions of sound.	Judgments O		B	
0°f	15°lf	15°lf	30°rb	45°lb
15°rf	30°rf	30°rf	15°rf	15°rf
30°rf	15°rf	15°lf	60°rf	60°rf
45°rf	30°rb	30°rf	45°rf	75°rb
60°rf	45°rb	30°rf	45°rf	60°rb
75°rf	30°lf	45°rf	45°rf	60°rf
90°r	75°rb	90°r	90°r	45°rf
75°rb	75°rb	60°rf	60°rb	0°b
60°rb	45°rf	45°lf	90°r	90°r
45°rb	30°rb	30°rb	45°rf	60°rb
30°rb	45°lf	60°lf	90°r	30°rb
15°rb	30°rb	75°rb	45°rb	75°rb
0°b	15°rb	15°rb	30°lb	45°rb
15°lb	0°b	45°lf	0°b	45°lb
30°lb	45°lb	30°rb	0°b	0°b
45°lb	45°lb	15°lb	45°lf	30°lb
60°lb	30°lb	30°lb	45°lb	60°lf
75°lb	45°lb	30°lb	90°l	45°lb
90°l	60°lb	0°b	45°lb	45°lf
75°lf	75°lf	90°l	90°l	90°l
60°lf	60°lf	45°lb	45°lb	90°l
45°lf	90°l	45°lb	60°lf	60°lb
30°lf	60°lb	45°lb	45°lb	45°lb
15°lf	15°rb	75°lb	30°lf	45°lb

Total correct on right side: 7 or 15.9%
Misplaced 15° on right side: 12 or 27.3%
Confusions on right side: 6

Total correct on left side: 3 or 6.8%
Misplaced 15° on left side: 11 or 25.0%
Confusions on left side: 9

Total correct: 10 or 10.4%
Total misplaced 15°: 27 or 28.1%

The contrasts between the right and the left side, and between monaural and binaural localization, are clearly brought out.

The right side has more correct localizations (15.9%) than the left side (6.8%). The number misplaced 15° is nearly the same on both sides. The left side has more confusions, 9, than the right side, 6, implying that the localization is less reliable on the left side.

Comparing these figures with the same tests on normal binaural observers (upper part of Table VI), we notice that binaural localization is considerably better. In the latter 93.7% are correct or misplaced 15° as compared with 38.5% in the former. Another significant contrast is the fact that monaural localization has numerous confusions of the right with the left hemisphere. For example, 75°rf was placed at 30°lf. O made seven such confusions in 48 trials, while such confusions scarcely ever occur in a normal person. Only two such cases occurred in 864 trials, Table VI.

The monaural observers were also much more reluctant in giving their judgments. Many trials had to be repeated several times, which was seldom necessary with the binaural observers.

The data of localization seemed to be the same as in the foregoing experiments, both observers agreeing that intensity was the most potent factor. When a sound seemed weak it was placed somewhere on the left side, and when it seemed strong and clear it was placed on the right side. The observers depended largely upon the relative intensities of the successive stimuli. On the left side the sounds seemed fainter and farther away, while on the right side they seemed stronger and clearer. O frequently said, "This sound is stronger than the one before, it must be on the right side," or "This sound is fainter, it must be on the left side." B stated when the sound was given at 60°lb, "This is a good deal farther away than the preceding sound" (which had been at 45°rf). He located it at 45°lb.

Elimination of intensity—Since intensity seemed to play such a large rôle in the perception of direction, a special set of experiments was planned to determine how important a factor it was. An attempt was made to eliminate the characteristic intensity differences. The telephone stimulus of the sound perimeter was carried through the audiometer by means of

which the intensity could be accurately controlled. By a few preliminary trials a stimulus of intensity 35 (audiometer scale) at 90°r was judged to be equal to intensity 38 at 90°l. On this basis the stimuli from the directions between these extremes were graded, and the intensities were distributed accordingly among the directions as follows:

From 45°rf to 45°rb intensity 35 was used.
From 30°rf to 0°f and from 30°rb to 0°b intensity 36 was used.
From 15°lf to 45°lf and from 15°lb to 45°lb intensity 37 was used.
From 60°lf to 60°lb intensity 38 was used.

The observer was told that the strength of the sound would be different in different directions. Each of the twenty-four directions was given twice in chance order, yielding the following results. Of the forty-eight trials only three were correct, and eleven misplaced 15°, that is 29.2% were correct, or approximately correct, while in the same tests with uniform intensity, Table XIV, O had 41.6% correct or approximately correct.

Then the "discrimination" tests were repeated on Observer O at 45°rf, 45°lf, 45°lb and 45°rb. In each of these directions 100 trials were made, 50 in which the intensity (35) was unchanged, and 50 in which three intensities (33, 35 and 37) were used in chance succession. For example, the intensity at the standard direction might be 35 and the intensity at the side might be either 33, or 35, or 37.

Table XV.

	45°rf	45°lf	45°lb	45°rb
Difference	10°	10°	15°	10°
Intensity uniform	74% correct	86% correct	84% correct	78% correct
Intensity varied	64% correct	78% correct	58% correct	76% correct

These measurements clearly demonstrate that intensity is a very important factor in localization. When the intensity was varied so that the observer could not rely upon a certain intensity to indicate a certain direction, the localization became considerably inferior—on the average 12%. But intensit probably did not play as important a part as the observers thought. Otherwise, the localization, in which the different intensities were used, should have been even more inferior. The fact that the observer was able to localize with some certainty the sounds of varying intensities, indicates that qualitative factors played a considerable part.

GENERAL CONCLUSIONS

The results and conclusions may be stated under three heads, (a) a summary of the specific facts demonstrated by the experiments of Part II, (b) a summary of the factors which enter into the localization of sound, and (c) the bearing of these results upon the traditional theories of localization.

Summary of the Results in Part II.

1. The threshold of hearing is considerably lower, that is, sensitivity is keener, at the side than either in front or in the back.

2. There are two types of observers: Those whose threshold is lower in the front than in the back, and those whose threshold is lower in the back than in front.

3. The ratio of sensitivity of the side to front (or back) is the same irrespective of the absolute threshold. This ratio is approximately 3:4.

4. The fact that the threshold is lower at the side than in front or in the back means that a given sound will seem nearer and more intense at the side than in front or in the back because it is relatively so much higher above the threshold. In other words, the nearer a sound is to the aural axis the stronger and clearer it seems, and this apparent change of intensity with change in direction is a potent factor in the localization processes.

5. Pitch discrimination is decidedly poorer at the side than in front or in the back.

6. Discrimination for intensities of a sound is about uniform for all directions.

7. The richer and more complex a sound the more accurately it can be localized.

8. A pure tone, so far as it has been approximated, can be localized, although with much less accuracy than a complex tone.

9. Angular differences between directions are overestimated in the frontal region, and underestimated in the lateral region.

10. Monaural localization is considerably inferior to binaural localization. The most accurate region is opposite the intact ear, and the most inaccurate region is opposite the deaf ear. The nearer a sound is to the active ear the stronger and clearer it seems.

Summary of the Factors in Localization.

The experimental evidences gained in this entire investigation warrant, I believe, a division of all the perceptual data involved in the localization of sound into two classes, namely, intensity and quality.

I. INTENSITY.

The intensity factors are again of two kinds:

1. The Binaural Ratio of Intensities.

The localization of sound depends to a marked extent upon the relation between the intensities with which a sound strikes the two ears. (See Part I, p. 15.) Some of the experimental evidences for this are:

a. The accuracy of localization is greatest where slight changes in this ratio are most readily perceived, that is, in front and in the back.

b. Localization is poorest where changes in the ratio are not so easily perceived, that is, on the sides, in the region of the aural axis.

c. The presence of confusion points or directions in symmetrical positions for which the ratio is the same.

d. The difficulty of median plane localization, which is really a particular case of confusion points. The ratio is the same for all directions in the median plane.

e. The inferiority of monaural localization in which the ratio is entirely absent.

2. *The Monaural Ratio of Intensity.*

There are systematic differences in the intensity of a sound when it comes from different directions. Evidences for this are:

a. The threshold measurements (Part II, Series I), showing that the threshold is lowest in the region of the aural axis and highest in front and in the back, consequently a sound at the side seems stronger than in front or in the back.

b. The "distance" tests (Part I, p. 21), showing that a sound is estimated to be nearest in the region of the aural axis.

c. The introspections of the observers in all the localization experiments, and especially in Tables I to III and V to VII in Part I.

d. Monaural localization depends to quite an extent on the relation of the intensities for different directions, which is brought out distinctly by the introspections of the observers, and by the tests in which stimuli of different intensities were used, resulting in poorer localization.

e. In the artificially monaural tests the uniform reversing of the responses at 45°lb indicates the force of the variation of intensity with direction in the perceptive process.

II. QUALITY.

The localization of sound depends to a considerable extent upon the quality and complexity of the sound. The evidences for this are:

a. Complex sounds, such as the human voice, noises and the telephone sounds, can be localized much more accurately than

(relatively) pure tones, such as the singing flame and resonator tones. (Part II, Series III).

b. The introspective accounts of the observers testify to the importance of qualitative changes and signs for different directions. (See Tables I to III and V to VII in Part I.)

c. The fact that median plane localization is possible to some extent.

d. In monaural localization the elimination of the intensity factor still left a considerable accuracy in localization, which must have been due to the qualitative data.

The Bearing of the Results on the Theories of Localization.

Of the four or five theories[1] which have been advanced at various times, the intensity theory has no doubt received the most support. It attempts to explain the localization of sound essentially by the binaural ratio of intensities. In the light of present results it is evident that this ratio can not alone account for the perception of direction. In fact it plays a relatively small, though significant, part in the complete process.

The quality and complexity of sound are real, potent factors in the localization process.

Intensity itself has been demonstrated to be effective, not merely in the binaural ratio, but in the characteristic changes and differences in different directions.

If localization depended entirely upon the binaural ratio, monaural localization would be an impossibility; and binaural localization itself would be considerably inferior. We would be surprisingly deceived by the systems of confusion points. Theoretically there are numerous planes parallel to the median plane in which the ratio is the same for all points in the same circumference in a given plane. But the other intensity characteristics and the qualitative factors come in to render accurate localization possible.

The traditional intensity theory is in the main correct, but it is quite inadequate. We must add to it the qualitative ele-

[1] Pierce, "Studies in Space Perception." p. 52.

ments and the monaural quantitative elements. These two have coördinate value with the binaural ratio, in the auditory perception of direction. With these additions, the theory would be more appropriately called the "intensity-quality theory."

ON THE TRANSFERENCE OF TRAINING IN MEMORY.

George Cutler Fracker, A.M., Ph.D.

CONTENTS.

Researches in the transference of training, or "spread of ability," seek to determine the influence of practice in one activity upon our abilities in other activities. There are, in general, two methods by which this problem has been attacked—the method of correlation, and the method of direct experiment. The method of experiment was chosen for this study. The usual means of conducting research by the experimental method in the transference of training is to employ two sets or series of experiments; one set to be given before and after training; the other a training set, in which one may be practiced for some time. The experiments given before and after training may be called the test series, and the other set, the training series. The test series is made up of several experiments, some of which are like the training series, while others differ.

The object of these two sets of experiments is to discover what effect practice in the training set has upon the test set, in

which the observer is not trained. This effect is measured by the difference in the results between the test series given before the training, and the test series given after the training. In order to measure the amount of training in the test series itself, two sets of observers are used: one set who take both the test and training experiments and another set who take the test experiments only. The difference in the gain between those trained and those untrained indicates the influence of the training experiments. The observers composing these two sets are selected on the basis of similarity in age and ability.

The present research is devoted to a study of certain aspects of memory. The training series consisted in practice in memory for the order of four tones. The experiments of the training series and the manner of conducting them will be explained under "Training Series." The test series consisted of eight experiments, as follows:

1. Memory for poetry.
2. Memory for the order of four shades of gray.
3. Memory for the order of nine tones.
4. Memory for the order of nine shades of gray.
5. Memory for the order of four tones.
6. Memory for the order of nine geometrical figures.
7. Memory for the order of nine numbers.
8. Memory for the extent of arm movement.

The relation of the respective test series to the training series, in the above experiments, was as follows:

The four grays; different in content, same in method.

The nine tones: same content, different in method.

All the other tests: different in content and method.

This relation between the training and the test experiments was planned in order that the elements concerned in transference might be determined by analysis of the final results. In order to aid still further in this analysis, each observer was asked to write a careful introspection at the close of each day's training, after each experiment of the test series, and a general introspection at the close of the experiments giving his observa-

tions and conclusions concerning the essential elements in improvement and transference. The object of the introspection was to discover the method or lack of method of each observer. Every effort was made to guard against giving the observer any hints as to how to perform the memory part of the experiments. At the beginning of each test, observers were given written instructions describing just what they were expected to do in response to the stimuli, but conveying no information as to how to do it.

The test experiments present the following important advantages: (1) Each is a task sufficiently difficult to demand intense application while it lasts. (2) Each test is so brief that it affords a minimum amount of training within itself. (3) The time between the first and second tests is fairly long. (4) The tests similar to the training series were taken in double fatigue order. (This order is explained under "General Experimental Conditions.") (5) The test material is of such nature that the second test could be made exactly equivalent to the first test without being a repetition of it.

DESCRIPTION OF THE TEST SERIES OF EXPERIMENTS

Experiment I. Memory for Poetry. Two stanzas of "Eve of St Agnes." The observer memorized two stanzas of this poem so that he could repeat each aloud to the experimenter without error. A record was kept of the time taken for each stanza, and of the errors made.

Experiment II. Memory for the order of four shades of gray. This experiment consisted in exposing before the observer, numbers two, seven, thirty and forty-five of the Hering Grays, by means of the psychergograph. The psychergograph consists of two main parts, the stimulator and the recorder. As the recorder was not used in these experiments, the description of it is omitted here. The following partial description of the stimulator is quoted from the original description by Seashore, Univ. of Iowa, Studies in Psychology, III, p. 5.

"The stimulator is a plain case, 40 cm. square, with a slanting cover. Near the front edge of the cover is a signal window, 8 millimeters wide by 20 milli-

meters long, through which the signals are seen. One hundred signals are pasted or printed on a paper disk, 38 cm. in diameter, so that when the disk revolves they are seen singly in succession right back of the signal window. The paper disk is clamped on a metal wheel which has fifty teeth on its edge. This wheel is energized by a strong clock spring which revolves it and the disk one one-hundredth of a revolution, thus exposing a new signal every time the detent which holds it is released. This detent is in the form of a lever escapement and is operated by electro-magnets." * * *

"A circle of the revolving disk is seen through the cover. On this there is a cross line which passes before the circular scale of a hundred units and indicates to the experimenter which signal is in view. This indicator serves at once as a counter of the number of acts performed and as a guide for the beginning and ending of the series. The order of the signals is determined in the making of the series, either by chance or by some suitable system. The experimenter, therefore, knows the actual sequence of the signals in every series, but the observer has no means of knowing at any time what signal shall appear."

The stimulator was arranged to expose each of the four gray disks for one-half second, an interval of one-half second being allowed between each exposure. Each disk was seven mm. in diameter. After the four grays were exposed, a blank remained before the observer for four seconds, then another arrangement of the four grays was given and another blank exposed. When the second blank appeared, the observer responded, giving aloud the order of the first group of four grays. After the third group had been exposed, he responded to the order of the second group, and so on through the series. The series consisted of forty groups taken in double fatigue order. In responding to the order of the four grays, the observer called the darkest gray, 4; the next lighter, 3; the next lighter, 2; and the lightest, 1. The grays were given in twenty-two different mutations, but the orders 1, 2, 3, 4, and 4, 3, 2, 1 were not used.

Experiment III. Memory for the order of nine tones. The third set of stimuli used consisted of four tones, varying in intensity, and delivered to the observer through a telephone at the rate of one tone per second, each stimulus sounding one-half second. The four sounds were produced by placing a telephone receiver in circuit with a 100 v.d. electro-magnetic fork and branching the circuit through four lines of resistance so adjusted as to produce four readily distinguishable tones when

the four keys of these branches were closed in turn. The e. m. f. was kept constant. These four tones were arranged so as to form a group of nine tones. After the delivery of the stimulus group, an interval of nine seconds was interposed, during which the observer responded aloud to the order in which the nine tones had been given. The tones were called 1, 2, 3, 4, in the order of loudness, 4 being the loudest. The 1, 2, 3, 4 and the 4, 3, 2, 1 orders were avoided. Twenty groups of nine tones each were given in double fatigue order.

Experiment IV. Memory for the order of nine grays. In this experiment, the four grays of Experiment II, were so arranged as to form a group of nine grays. These were exposed on the stimulator at the rate of one per second; exposure, one-half second. After each group of nine grays had been exposed, there followed an interval of nine seconds, during which the observer gave aloud the order of the grays in the group just given. Twenty groups of the nine grays were given in double fatigue order.

Experiment V. Memory for the order of four tones. The four tones were those composing the major chord on the piano. Instead of responding by number, the observer responded by the names Do, Me, Sol, Do-2. The tones were produced in the same order, with the same duration, at the same rate and with the same response intervals as the grays in Experiment II.

Experiment VI. Memory for the order of nine geometrical figures. The nine figures drawn upon a card are described as follows:

> "Each figure is composed of three lines; the lines are all straight; two lines are equally long, and the third is half as long as these; the two long lines always adjoin each other; the lines join either at the end or in the middle; no line is crossed; no two figures are alike; the angles are right angles." [1]

The stimulus card was exposed ten seconds, and the observer was given one minute in which to reproduce all the figures he could remember, drawing them in the same relative positions and proportions as on the card. Five records were taken.

Experiment VII. Memory for the order of nine numbers.

[1] Seashore, "Elementary Experiments in Psychology."

The stimulus for this experiment was nine numbers of two figures each, read aloud at the rate of one pair each second and a half. After nine pairs had been read, the observer was given fifteen seconds in which to write as many of the pairs as he could remember. Ten sets of nine pairs made up the test.

Experiment VIII. Memory for the extent of arm movement. The apparatus for this experiment consisted of a glass rod mounted one-half inch above a metric scale. A hard rubber cylinder, about one inch in diameter, was fastened firmly at one end of the glass rod so that its edge tallied with the zero point of the metric scale. Upon this glass rod was a second hard rubber cylinder freely adjustable. The observer, with eyes closed, moved his finger with a free arm movement along the glass rod, from the fastened cylinder to the adjustable cylinder, held at a standard point by the experimenter. The observer was allowed to move the finger out and back twice. The experimenter then moved the adjustable cylinder away from the standard position, and the observer moved his finger along the rod until he thought he was reproducing the standard distance. Three standards were used; viz: fifteen, twenty, and twenty-five cm., and ten trials were taken in varied order for each standard.

DESCRIPTION OF THE TRAINING SERIES OF EXPERIMENTS

The apparatus for the training series consisted of that described in test Experiment III, the nine tones. The four intensities of tone of the fork were arranged in all possible combinations except the 1, 2, 3, 4, and the 4, 3, 2, 1, order. Enough of these combinations were used and repeated to make a series of seventy-five groups. The observer, seated comfortably at a table with a telephone receiver carefully adjusted to his ear, listened to the four intensities of sound. The experiment was carried on in exactly the same way as the four grays of the test series except that tones were used instead of grays and the number of groups extended to seventy-five, four sets of which made up a day's practice. It was possible to give seventy-five groups in about ten minutes. At the end of each ten minutes, a rest of one minute was taken.

For three observers, Tuesdays, Thursdays and Saturdays of each week were the practice days; for five, Tuesdays and Thursdays, or Wednesdays and Fridays, of each week.

METHOD OF RECORDING AND OF ESTIMATING RESULTS.

The observer's responses were kept by a recorder who used mimeographed sheets corresponding to the sheets used by the experimenters as a guide in giving the stimulus. These mimeographed sheets consisted of columns of numbers corresponding to the order of the numbers of the grays or tones used in the test and training series. The order in which the groupings were given was readily changed by beginning at different parts of the sheet. If the observer omitted to respond to any stimulus group, a line was drawn through that number upon the record sheet. If he responded incorrectly, his reply was written above the corresponding number on the record sheet.

Results in both the test and the training series are estimated on the basis of the per cent of correct responses. Training curves are plotted to show the per cent of correct responses in each of the four sets of seventy-five groups. Test results are shown by lines drawn across the charts.

GENERAL EXPERIMENTAL CONDITIONS.

Every observer was allowed a short preliminary practice at the beginning of each test or training period with the grays and the tones in order to secure adaptation. This seldom took more than a few trials.

No observer knew of the results of his records until after the experiments were entirely completed, with the single exception of G. C. F., during his second training series.

Every effort was made to preserve uniform conditions, especially for the two test series. A record was kept of the hour of the day when each experiment was taken; and the same day of the week, and the same hour of the day, were kept for each observer. Great care was used to keep the temperature, light, and sound conditions of the room as constant as possible. The

experiments were carried on in a room where the apparatus remained in the same position, and all the above elements could be easily controlled.

In order to determine the amount of improvement due to training, the observers were divided into two classes: (1) Those who took both the test and training experiments, and (2) those who took the test experiments only. Of the first class there were eight, and of the second class, four.

The following order was maintained in giving the experiments in the test series:

1. Poetry, two stanzas.
2. Four Grays, twenty groups.
3. Nine Tones, ten groups.
4. Nine Grays, ten groups.
5. Four Tones, twenty groups.
6. Geometrical Figures, five trials.
7. Nine Numbers, ten columns.
8. Arm Movement, ten trials for each of three standards, 15, 20, and 25 cm.
9. Four Tones, twenty groups.
10. Nine Grays, ten groups.
11. Nine Tones, ten groups.
12. Four Grays, twenty groups.

This arrangement gives a double fatigue order for the four experiments most closely resembling the training series.

The observers. G. C. F. is a teacher of psychology and has carried on experiments in this subject on two former occasions. With Professor Seashore, he devised the experiments of this series, set up the apparatus for the tests, worked out the scheme for the mimeographed record blanks, and served as experimenter and recorder for several of the observers. He is considerably older than the other observers. D. S. was also a trained observer and an instructor in psychology; thoroughly familiar with the material and method of this experiment. Nearly two years previously, he had been trained in experiments almost identical with this series[1] as regards stimuli and

apparatus, but the responses had been made by signals on four keys, instead of speaking. F. S. was a graduate student in psychology, trained in many forms of psychological experiment. E. M. C., H. C. E., A. R. F., M. M. M., and M. L., were college juniors with some experience in psychological observation. These observers took both the training and the test experiments.

J. W., M. C., M. D. F. and D. D. W. took only the test experiments; J. W. was a senior in college and somewhat older than the others. M. C., M. D. F., and D. D. W. were college juniors and all were familiar with psychological experiment and observation.

RESULTS OF THE TRAINING SERIES.

The practice curves and the curves of improvement in the test series are shown on Charts I to IX inclusive. All the observers except one were trained for four weeks, two or three days per week. G. C. F. was trained for eight weeks three times per week. Two charts, I and II, show the results for this observer.

Each point in the heavy curves represents the average for seventy-five groups of tones. Four such sets, of seventy-five each, represent a day's work. The curves are plotted on the basis of the per cent of correct responses. Each day's average is shown by a heavy horizontal line.

These curves show the progressive gain by practice in the training series and the relative ability in the end tests before and after the training.

G. C. F. thinks that the advantage he has had in his experience as experimenter is offset by the development of automatism which he was forced to break when he became observer. Before beginning the training, he had served as experimenter for three other observers, and had thus made about 10,000 reactions from the same stimuli but with attention upon the beat of the metronome, the delivery of the stimulus, etc., instead of upon the sounds of the stimulus. At the beginning of the training,

[1] Seashore and Kent, "Periodicity and progressive change," Univ. of Iowa, Studies in Psychology, IV, p. 82 ff. (see practice curve opposite p. 85).

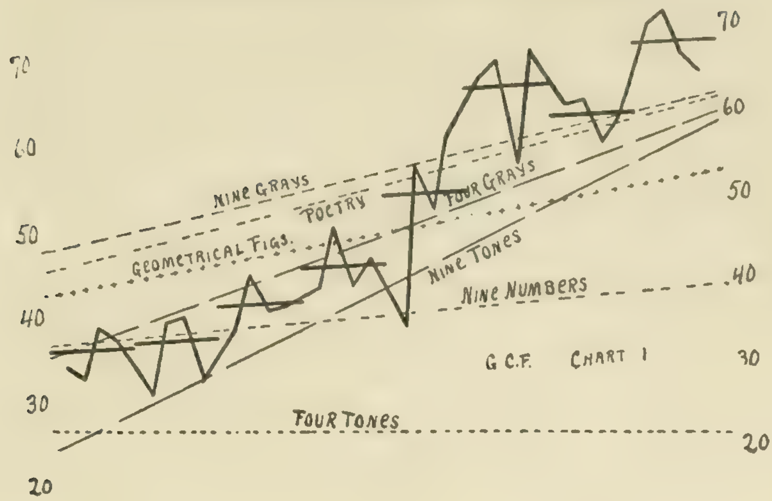


FIG. 1

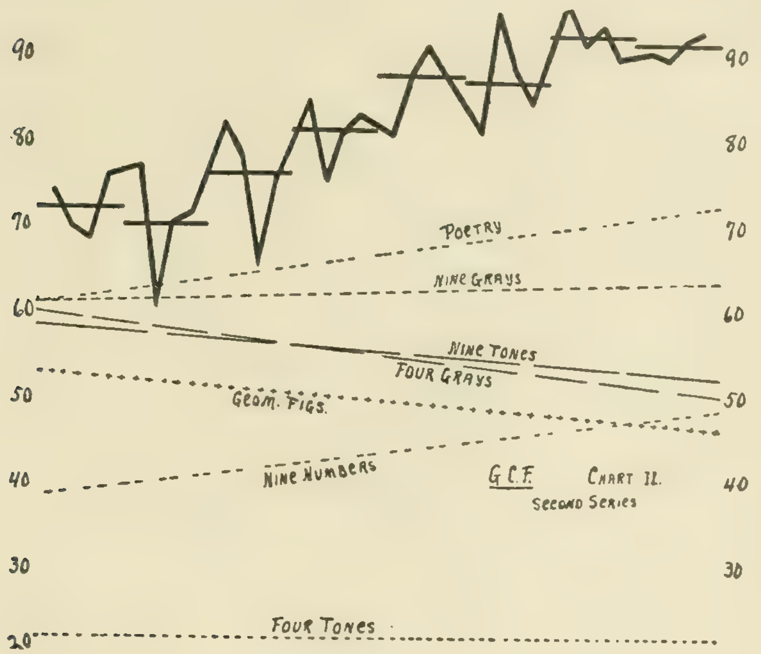


FIG. 2

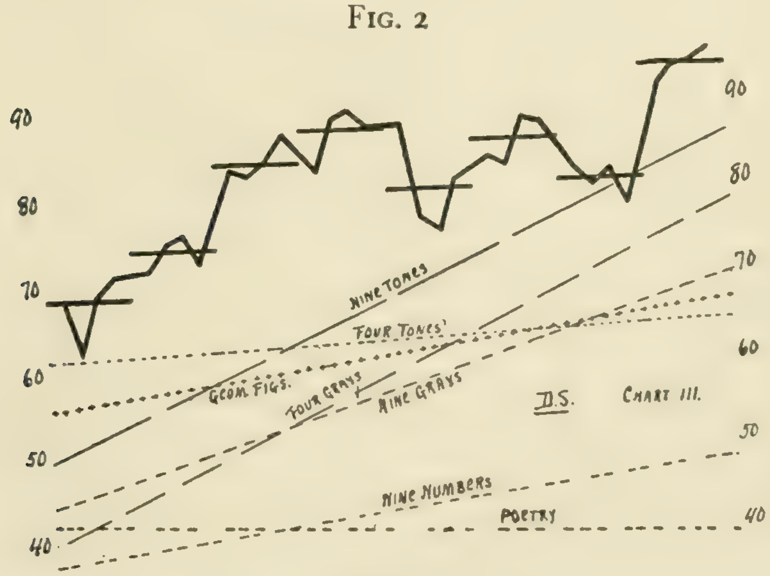


FIG. 3

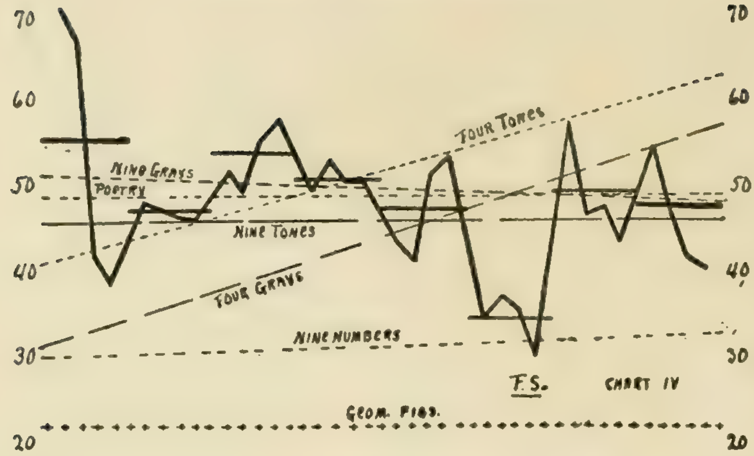


FIG. 4

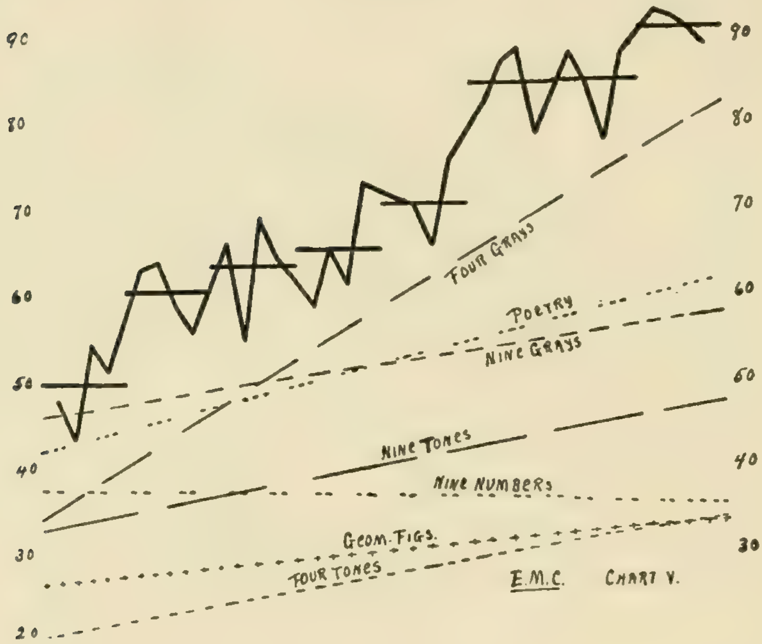


FIG. 5

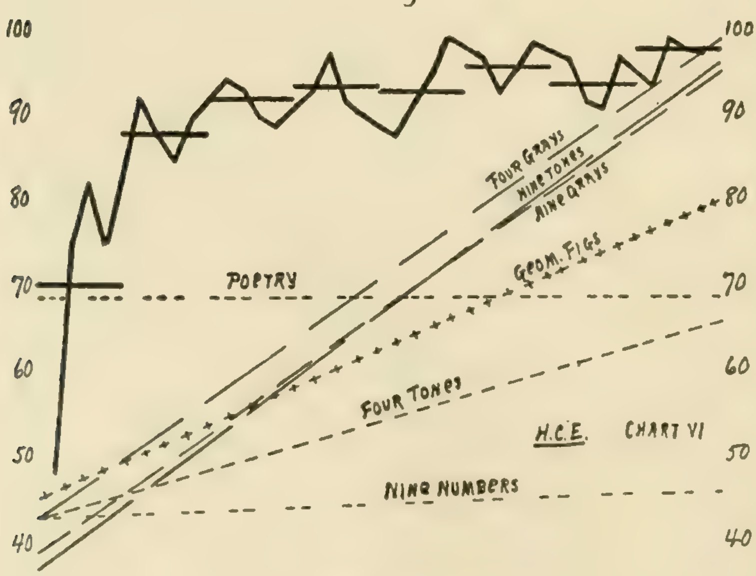


FIG. 6

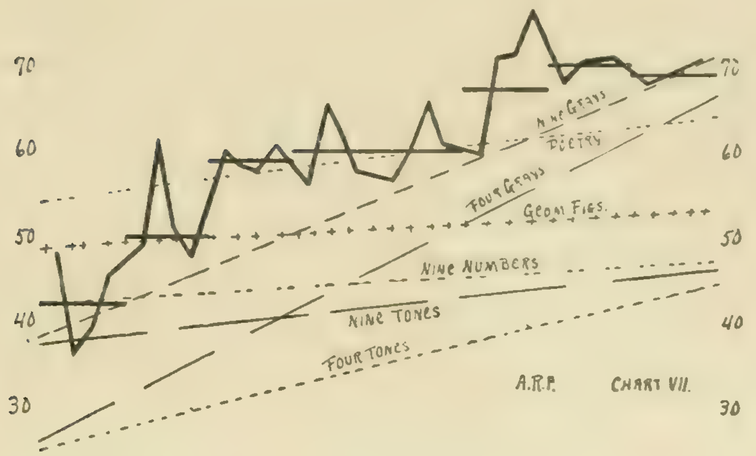


Fig. 7

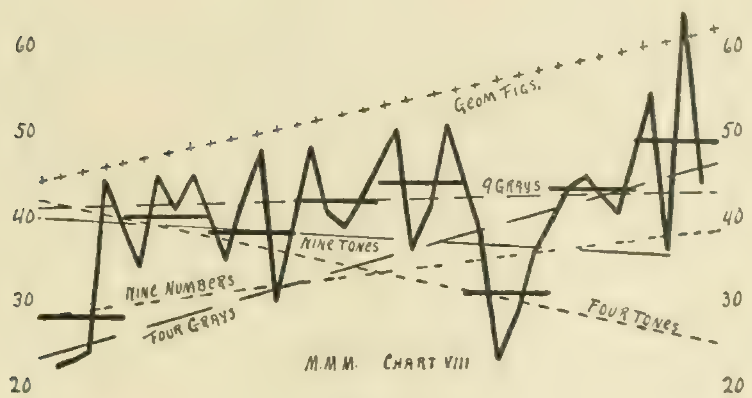


Fig. 8

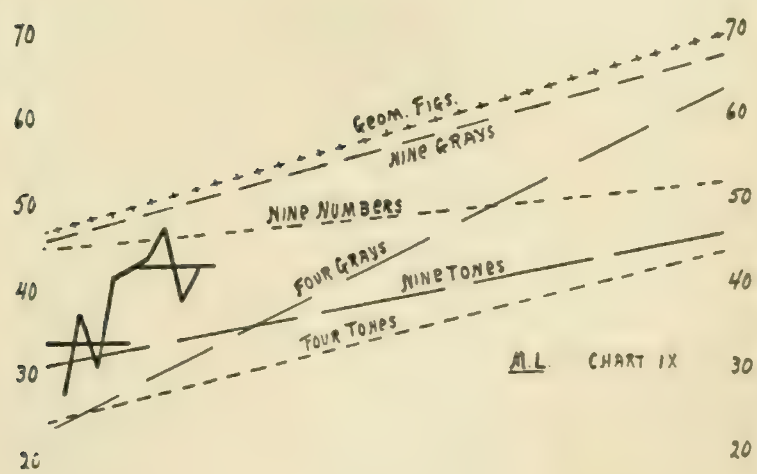


Fig. 9

this kind of response had to be broken up and the attitude of the experimenter interfered with that of the observer. Lack of familiarity with conditions of experiment and with apparatus is mentioned by other observers as a cause for the low starting point of the curves. Another reason given is the tendency to bring outside associations into the practice. Inability to hold the attention to the new task is also mentioned.

G. C. F. believes that his continued practice as experimenter during the training was a cause of fluctuation in his curve.

The progression in the curves ranges from a loss of 9 per cent to a gain of 48 per cent, or an average gain of 28 per cent. The average point at which the curves begin is at 50 per cent, and the average summit of the curves is 73 per cent. The range of the points of beginning is from 32 to 70 per cent, and the range of the summits is from 46 to 98 per cent. The rapidity of the rise is so closely connected with method that it will be discussed under that head.

The explanations offered for the fluctuations in the curves are mainly inability to hold attention, disconcerting associations, guessing at results, and lack of a method to which to adhere.

The introspections reveal the methods used by different observers. At the beginning of practice these introspections show that every observer did more or less guessing in responding. A number of devices were tried during the first days of training. All observers began by repeating as many times as possible, before the response, the order of the stimulus group. Several attempted to divide each group of four sounds into groups of two sounds each. Some tried to locate the loudest and the faintest sound in the group and remember the order of the other sounds by these. There were difficulties with all these methods. In the attempt to repeat the first group as often and as rapidly as possible observers found that, when the second group came, the repetitions had to cease while attention was given to the new stimulus. As a result the first group was lost in the attempt to fix the second, or in responding to the first the second was forgotten. Under such stress there is a strong tendency to guess, or to become confused. Both these things happened frequently. Several observers attempted to separate the stim-

ulus from the response. With G. C. F. this became a factor in his method. With F. S. there seemed to be a tendency to try new methods throughout the training. In guessing, some times the whole group is guessed at, other times one or two of the numbers of the group. All observers said that speaking in response disturbed the retention of the first group. After trying several schemes observers settled down to a definite way of retaining the groups and of making the responses. The individual methods adopted can be shown best by quoting from the introspections.

G. C. F: "The principal features of my method are: first, an imagery of position in space for the four tones. Number four is right at the ear, three is about four or six inches away from the ear, two is several feet away, and one is a faint sound a long distance off. The exact position of two and one is not clear, but the position of three and four is definite. Second, there is a separation in attention between the stimulus and the response, that is, attention is given to the first group, which is fixed by the imagery above described, and placed upon the tip of the tongue to be spoken immediately after the second group is heard. When the first group is thus delegated to the motor side, it is dismissed from active attention, which is then focused upon the second group. The second group is fixed almost while the first group is being automatically responded to."

With D. S. the method used goes back to his previous training. In the introspection following the first day's practice he said that many "names" used formerly came back during the hour and helped materially in the responses. The names referred to are the forms of imagery used by D. S. to identify different arrangements of the four sounds. On the second day of practice the system of "names came back completely," and he attributed practically all of his improvement to the recovery of his system. From thence he improved steadily.[1]

F. S. tried to remember the tones in groups of fours and to hold them by rapid repetition. After the second group was given he responded to the first group, and then immediately repeated the second group as often as possible before the next stimulus was given. Later he tried to remember the first two numbers of a group and guessed at the other two. When he missed a

[1] For full account of the method used by D. S. in his former training series see Seashore and Kent, op. cit. pp. 87–90.

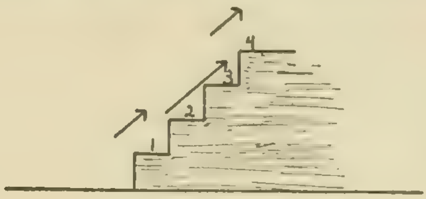
group altogether he guessed at it. Sometimes he grasped the first two numbers of a group with the last number and supplied the third. He found great difficulty in retaining one group while securing the next. Speaking in the response disturbed the retention of the second group. As practice proceeded he noticed no new method but seemed able to take things more quietly and to respond with less effort. He noticed especially a tendency to listen to the loud tone and to locate it in the group. This tendency results in a species of auditory imagery, but an imagery not definitely recognized. In the last introspections, the observer said that there was a tendency to locate the loudest and faintest tones in a group.

H. C. E: "In this training I used two methods of remembering. The first four numbers I repeated several times during the four seconds interval, and continued repeating them while the next four were being given; then I would give them aloud without giving any actual attention to them. While the second group was being given, I turned my active attention to it, first getting a visual image of their position and at the same time repeating the first four. In the four intervening seconds, I repeated aloud the first four members without any active attention to them, keeping my active attention on the visual image. Just as soon as the first four were repeated aloud and gotten out of my mind, I would immediately turn the visual image of the second group into a repeating image and wait for the next group of four. This same process was kept up throughout the experiment. The imagery is a vertical arrangement of the numbers of the four tones, in appearance somewhat like keys. The lowest number or key was 1, and the highest, 4. The keys were apparently numbered as though printed. Some groups, however, such as 4, 1, 2, 3, were divided into two, 4 being in a group by itself, and the 1, 2, 3 combined. The visual image is not an image of the numbers. They are arranged in vertical order and I pick them out vertically but do not see the numbers themselves. The one highest up or number four, is the largest, and number one is very small, with numbers three and two correspondingly between. As near as I can tell, they are round or disk-shaped and appear thus:

As practice continued the visual image became more familiar and found myself more and more able to remember the groups without giving much attention to them."

A. R. F.: "After repeating the numbers of the first group, I would form a sort of mental picture of how they would look on a scale, and this would help me in remembering them. When I got confused, I just stopped and did not

try to recall, but went on with the next group. The figure I find myself using after the third or fourth day of training is like the following:



There was a flight of four steps, each numbered in ascending order. This figure illustrates the group 2, 3, 1, 4; 2 and 3 being consecutive were represented by a long arrow and, because they came first, the arrow was close to the stairs. 1 came next and was a little farther away, and was a short arrow, being alone. 4, the last one, was still farther out, and was also a short arrow. I recalled the group by the position and direction of the arrows in the mental image. After listening to the second group, I go back and, from the picture, get the numbers in the order given. The mistakes I make are caused by taking too long to form the picture or to recall it."

M. M. M. developed a method of very vivid imagery rapidly. He says:

"In remembering these groups I thought of them as being in a position like four keys on a piano, such as C, D, E, F. I remembered them as 1, 2, 3, 4, and while one groupwas being given I tried to keep a picture of the order the keys took in the preceding group. It was much easier to remember the group when the numbers came together, such as 1, 3, 2, 4; in this case, I thought of 3, 2, as being between 1 and 4. I could see the keys just as if they were pressed down. In trying to remember a group, I sometimes hung to it too long and became confused in getting the next group. As practice continued this method became more reflex, and it was easier to remember the groups."

E. M. C. began the training by repeating to himself, about twice during the four seconds interval, the first group. Later the repeating became automatic, so also did the response; and he adds:

"I find that if I miss a group, that breaks into the rhythm and it is hard to get into the swing again. Sometimes I give a group and guess just to keep up the rhythm. During these tests I have been convinced that the subconscious does a great deal for us."

It seems that E. M. C. did not recognize a particular form of imagery as did the other observers. It is not clear from his introspections that he had an imagery form, nor is it clear that he did not have one. Throughout the introspections for the second test experiments, however, E. M. C. speaks of the aid

he received from the training series method in securing results in the test experiments. There seem some indications, therefore, that he had a method or some particular form with which he had become familiar during the training and which he used in the final test. This method must have been something more than that of repetition which he used in the first test experiments.

In the case of M. L. the training was carried on for two days only, and the introspections contain no definite statement concerning the specializing of method in the form of imagery. But under her introspection for the nine grays it is fully described. During the final test series she says that there was a decided help from the first test and the method of the training series. She says:

"The loudest tone seemed to serve as a sort of station around which the others grouped themselves. This scheme helped very much in securing correct results. Sometimes the lowest tone served as a station. Such combinations as 3, 2, 1, 4, or 2, 3, 4, 1, were much easier, because of my way of remembering."

In the matter of recognition of a method, or what seems to be the same thing, the recognition of an individual imagery, the observers did not all recognize a particular method to such an extent as to describe it in introspections. From what has been quoted it is apparent that all but two, F. S. and E. M. C., recognize a peculiar imagery; and the evidence shows that they also used imagery but did not recognize it as such. The relation which the recognition of the method or imagery bears to improvement is significant. Without a conscious recognition of the imagery an observer may improve rapidly, or slowly, but a steady improvement seems to follow if an imagery exists, and is consistently used. E. M. C. seems to illustrate this. F. S. illustrates an observer who had imagery but who failed to use it consistently. With observers who recognized imagery the rate of the improvement seems to coincide with its recognition. This is reasonable for, as observers say, the recognition of a method gives one confidence in his ability to do the work. With G. C. F. imagery was recognized about the fourth or fifth day, and the rise in the curve is most rapid immediately after.

With D. S., imagery was recovered the first and second days and the rise in the curve is most rapid during the first, second and third days. With H. C. E., imagery was recognized the first day and the gain is very rapid the first two days. With A. R. F., imagery was described on the fifth and sixth days and the gain is greatest on the sixth day, although the gain is great on the second day also. In the case of M. M. M., whose curve shows only a slight gain during practice, yet who recognized imagery on the first day, the rise is great on the first day but is not great thereafter until the seventh and eighth days when he began to recover from an attack of the grip, from which he suffered severely on the sixth day.

With A. R. F., whose imagery seems somewhat intricate, the relation between complexity and rate of improvement is shown. This observer speaks of losing many groups because he had not time enough to adjust his imagery to the group.

In the case of untrained observers, a recognition of imagery is not alone sufficient to give this confidence of improvement; for many say that they are not sure that they can use the imagery in other tests. A certain familiarity with imagery would seem, therefore, essential. This familiarity the training series gives.

The fact then seems to be that steady improvement may take place because of the use of an imagery without a conscious recognition of its presence. An imagery may even be recognized without adding essentially to the speed of improvement, but a recognition of it adds confidence in one's ability and reliance upon one's method, which is pretty sure to result in rapid improvement.

The essentials of method in training as brought out by these experiments are; first, familiarity with conditions of training, such as the room, the light, heat, furniture, apparatus, the experimenters, the adjustment of the observer to the apparatus, and learning what is expected of the observer; second, the use of rapid and frequent repetition in order to retain; third, the sorting out of things essential to the performance of the act from those things that are non-essential;[1] fourth, the selection,

[1] See Coover and Angell, "General practice effect of special exercise," Am. Jour. Psychology, XVIII, pp. 328–341.

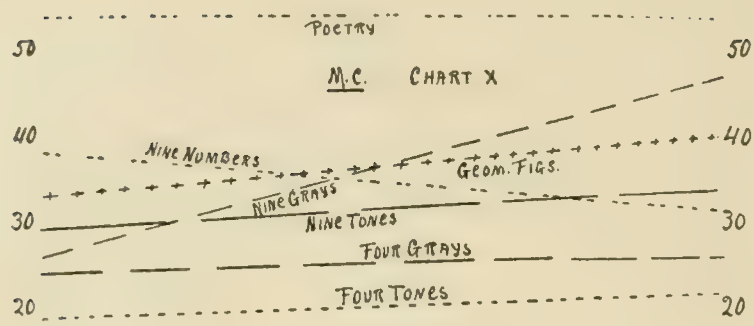


Fig. 10

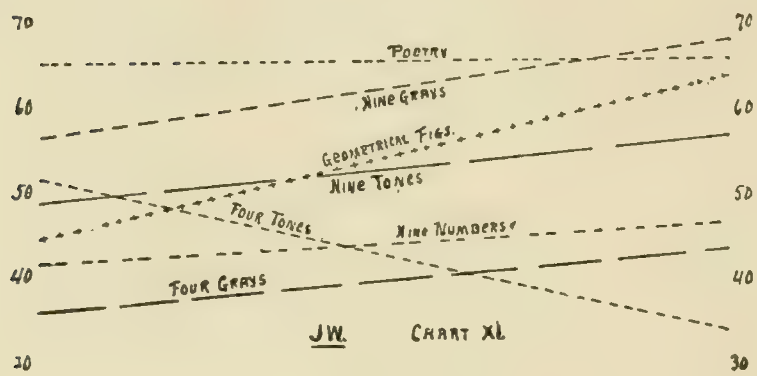


Fig. 11

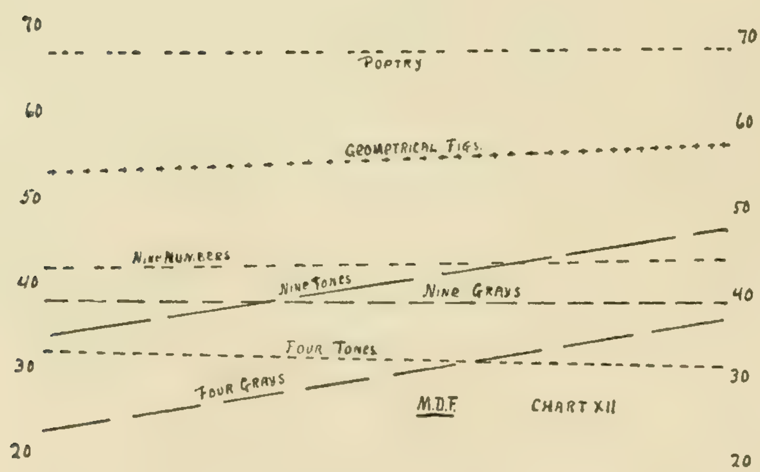


Fig. 12

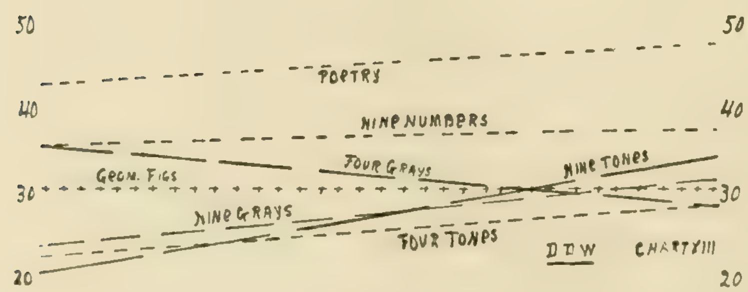


Fig. 13

TABLE I.

Results of Test Series for Trained Observers.

	G. C. F. 1″	2″	3″	2″—1″	3″—2″	3″—1″	D. S. 1″	2″	2″—1″	F. S. 1″	2″	2″—1″
Poetry												
1″ Stanza	43	61	67				36	39		44	45	
2″ "	49	62	77	+15	+11	+26	49	39	−3	55	54	0
Av.	46	61	72				42	39		49	49	
4 Grays												
1″ Trial	29	51	41				43	95		25	59	
2″ "	45	68	61	+22	−8	+13	39	61	+37	38	56	+26
Av.	37	59	51				41	78		31	57	
9 Tones												
1″ Trial	13	61	58				49	82		54	49	
2″ "	37	56	48	+34	−6	+27	51	91	+35	38	46	+1
Av.	25	59	53				50	85		46	47	
9 Grays												
1″ Trial	54	59	71				47	72		44	50	
2″ "	63	63	57	+2	+3	+6	42	68	+26	59	48	−2
Av.	59	61	64				44	70		51	49	
4 Tones												
1″ Trial	36	24	31				69	53		45	58	
2″ "	18	18	11	−6	0	−6	54	76	+3	38	69	+22
Av.	27	21	21				61	64		41	63	
Geom. Figs.												
5 Trials	44	53	47	+9	−6	−3	56	67	+11	22	22	0
9 Numbers	37	39	49	+2	+10	+12	37	48	+11	30	34	+4
Movement												
15 cm.	96	95	95	−1	0	−1	95	94	−1	94	96	+2
20 cm.	97	97	98	0	−1	+1	98	95	−3	94	93	−1
25 cm.	98	97	98	−1	+1	0	97	94	−3	94	95	+1

1″, 2″, and 3″ = First, second and third tests.
2″—1″ = Average of First test minus average of the second test.
\+ = Gain.
— = Loss.

Table 1 (Continued)

	E. M. C.			H. C. E.			A. R. F.			M. M. M.			M. L.		
	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"
Poetry															
1" Stanza	41	56		54	65		61	70					89	97	
2" "	44	66	+19	84	73	00	48	59	+10				92	100	+8
Av.	42	61		69	69		54	64					90	98	
4 Grays															
1" Trial	10	83		24	98		25	73		15	48		19	69	
2" "	59	83	+48	63	100	+56	29	60	+39	33	46	+23	30	56	+39
Av.	34	82		43	99		27	66		24	47		24	63	
9 Tones															
1" Trial	27	47		23	94		31	43		30	36		28	38	
2" "	40	50	+15	52	99	+59	44	48	+8	50	34	+5	33	54	+15
Av.	38	48		37	96		37	45		40	35		31	46	
9 Grays															
1 Trial	39	64		26	93		53	70		42	31		40	68	
2" "	54	52	+12	53	98	564	23	71	+32	40	56	+2	52	67	+21
Av.	46	58		39	95		38	70		41	43		46	67	
4 Tones															
1" Trial	20	40		33	63		30	44		41	32		29	43	
2" "	19	29	+15	54	70	+23	20	46	+20	44	19	−16	23	45	+19
Av.	19	34		43	66		25	45		42	26		25	44	
Geom. Figs. 5 Trials	27	33	+6	45	80	+35	49	54	+4	44	62	+18	47	69	+22
9 Numbers	38	36	−2	43	46	+3	43	47	+4	29	38	+9	45	52	+7
Movement															
15 cm.	96	95	−1	98	96	−2	96	95	−1	97	95	−2	88	94	+6
20 cm.	98	96	−2	98	98	−0	95	96	+1	97	97	0	89	97	+8
25 cm.	97	97	0	97	96	−1	97	96	−1	97	93	−4	90	93	+3

TABLE II.

Results of First and Second Tests for Untrained Observers.

	M. C.			J. W.			M. D. F.			D. D. W.		
	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″
Poetry*	48	34	00	65	65	0	70	63	+2	40	51	+5
	61	74					64	75		47	45	
	54	54					67	69		43	48	
Four Grays	21	37	+2	32	41	+7	25	28	+15	36	23	−7
	28	14		40	46		20	46		36	35	
	24	26		36	43		22	37		36	29	
Nine Tones	30	30	+5	33	55	+9	28	43	+14	24	28	+14
	29	38		63	60		39	51		18	42	
	29	34		48	57		33	47		21	35	
Nine Grays	19	43	+21	48	64	+11	40	44	0	22	36	+8
	33	51		65	72		38	34		25	30	
	26	47		57	68		39	39		24	32	
Four Tones	20	21	+2	37	31	−17	29	28	0	18	28	+6
	18	21		66	37		36	36		28	30	
	19	21		51	34		32	32		23	29	
Geom. Figs.	33	40	+7	44	64	+20	53	58	+5	31	31	00
Nine Numbers	39	31	−8	41	47	+4	42	44	+2	36	38	+2
Movement	94	94	0	88	97	+9	97	92	−5	96	96	0
	97	96	−1	92	97	−5	98	95	−4	95	96	+1
	98	95	−3	95	97	−2	97	95	−2	96	97	+1

* Notation same as in Table I.

consciously or subconsciously, of an individual way of picturing the stimuli, sometimes also including the response, and which consists of a particular form of imagery; fifth, the use of this imagery until it becomes reflex; sixth, the appearance of rhythms in the ability to hold the image-changes in attention; and seventh the formation of associations between the giving of the stimulus and the response, after the use of the imagery becomes reflex.

RESULTS OF THE TEST SERIES.

The results of the test series for all observers are shown on Charts I to XIII inclusive by the broken lines which run directly across the chart. The results are also shown numerically in Tables I to III. The ability in each of these tests is expressed in the per cent of correct responses, as in the training records. The tests are discussed in the order in which they were taken.

Trained Observers.

Poetry. The gain in the tests for poetry was not very great in the case of any observer. Various methods were used. A large number read the stanza over first. Some divided it into parts of two lines each, some of four lines each, some into two parts. More than half of the observers speak of using imagery in remembering. Several say that they pictured the lines on the page in relation to each other. Several divided the stanza into parts according to the pictures it contained. Many memorized by these images in the stanza, and then combined the images into a whole picture. Two observers say that the training series may have helped in securing improvement. One says that the training may have helped by emphasizing imagery, and the other says that it may have helped in dividing the stanza into parts.

The Four Grays. The gain in the four grays is often greater than the gain in the training; it is usually as great, seldom less. In the first test the methods used were—to catch alternate groups, to divide groups into two groups of two figures each, and to remember each group. F. S., especially, noticed a tendency to image the groups. His first impulse was to remember the

groups by a picture of the numbers of the grays or the grays themselves, but the time was too short to work out the picture for each group. Often observers tried two or three ways of remembering during the first test for the four grays. In the second test every trained observer but one says that the method developed in the training helped in securing a better record. Six of the trained observers say that they used the same imagery in the final test series that they used in the training series. There was more or less hesitancy in using the same imagery because of the difference in the stimuli of the training series and this test. The difficulty seems to depend upon the imagery. With G. C. F., whose imagery was visual-auditory for the tones of the training, there was great difficulty in using it with the grays. With D. S., whose imagery was visual, there was but a slight hesitancy in fitting the imagery into the responses for the grays. This was true also of H. C. E., A. R. F., M. M. M. and probably of E. M. C., for he says that the training helped him decidedly in the final tests, though he does not record a specific type of imagery. The same is true of M. L. F. S. says, in the introspection for the second test, that he remembered the groups by visual imagery with which he had no difficulty. He did not repeat the numbers as in the first test and as in the training series, but saw them in two groups of two in each group.

The Nine Tones. Four observers gained more in the nine tones than in the training series. Two made the greatest gain in the nine tones of any of the test experiments. The influence of the training therefore seems to be very strong. G. C. F. says that with him visual imagery is usually the strongest. Yet he made the greatest gain in the nine tones. His training imagery is auditory-visual, and he thinks that most of the gain in the second test for the nine tones is due to the influence of the training imagery. D. S. says that during the second test he was able to transfer his imagery system directly to this test by grouping the nine tones into fours. All the trained observers say that the training series helped in the second test. All except M. M. M. used the training imagery in the second test with the nine tones. M. M. M. divided the nine into numbers of three figures each, as 421, 343, 124. All had a different

way during the first test, and all found that the immediate succession of the second group of four tones after the first group led to confusion.

The Nine Grays. Three observers made a greater gain in the nine grays than in the training. One observer made his greatest gain in the nine grays. The introspections show the same characteristics regarding the influence of the training in the second test over the first. Two observers, G. C. F. and D. S., say that they find it easier to use their imagery with the tones than with the grays. But there is the same difficulty experienced in the immediate succession of the second group after the first, and the same change of method from the first to the second test. Under her introspection for this test M. L. descr bes her imagery:

"Toward the last of this series I thought of a new method of getting these by fours. It was by sort of picturing them with braces connecting them, with a top brace being the first one thus:

1, 2, 3, 4,   1, 2, 3, 4,   1, 2, 3, 4,

This would represent the combinations 1, 3, 2, 4, 2, 4, 1, 3, and 4, 2, 3, 1. This method helped me most when the grouping was something of the skipping order, and not 1, 2, 4, 3, where the numbers were right next to each other."

The Four Tones. One observer only, F. S., gained more in the four tones than in the training series, and no observer made the greatest gain in this test. As the name implies this test is most like the training series and therefore suggests that it should show the greatest gain if training is to influence tests similar to it. G. C. F. says that the failure to improve is due to the different method of response. Instead of responding by numbers the observers were instructed to respond by the syllables—Do, Me, Sol, Do-2. All observers say that there was a distinct tendency during the second test to use the methods and imagery of the training, but in the effort to make the syllable response, the tendencies developed in the training were broken up. Nearly all think that they could have made a better record if they could have responded by the use of numbers. The similarity to the

training series made the interference the more effective. M. M. M. says "the practice series was a hindrance here, because the numbers were so drilled into me that it was hard to change."

Geometrical Figures. Several observers think that the experience of the first test is sure to suggest methods for the second test. Motor imagery was strong with many observers for they moved the pencils over the paper while trying to retain the figures in memory between the trials of the test. The method most used was to remember the figures because of their similarity to letters of the alphabet. Some observers tried to remember the entire nine figures after a ten second exposure but most observers adopted the method of remembering two or three at each exposure. H. C. E. thinks that the training series helped here because he was able to group things together and think of two groups at once. M. M. M. knows of nothing in the training series that helped except that he was better able to concentrate attention on what he was doing. The other observers saw no connection between this test and the training series.

The Nine Numbers. F. S. made his greatest gain in this test. The common method here was to remember two or three of the first pairs, and to hold two or three of the last pairs because of their recency. No one recognized any way by which the training helped here.

Movement. The method employed by most observers is indicated by the introspection of D. S.: "The movements of the finger along the glass rod were always accompanied by an eye movement and a visual image of the distance traversed. It is evident that the estimate was made both by muscular and visual imagery." Not all observers recognized these factors but nearly all speak of visual and motor imagery.

Final Introspections. The opinion of the trained observers in regard to the factors that make for improvement and the relation of the training to the tests is shown by the following quotations from introspections written after the experiments were completed.

D. S.: "The following are the most important effects of the training series upon the test series:

1. The system of 'names.' The most important and effective factor.

2. Imagery was very prominent in the training and seemed to be more prominent in the second test than in the first.

3. Greater economy in mental effort and attention.

4. Development of ingenuity in devising methods. The method of 'names' was a very essential part of the improvement in the training series. Without it, I believe that I could not have reached the proficiency I did. In the last test, I felt that somehow I could better master the situation."

F. S.: "If I made any improvement in the last test, I think it was due to the following causes:

The idea that I was going to improve.

I found it easier to hold my attention on the work during the second test.

I felt a distinct sense of improvement in only one test, namely, the four grays. This was due to a change of method. I also have a feeling that I improved in the four tones. This may have been due to a general familiarity with the test. I was able to recall these tones with much less difficulty than the telephone tones. The gain in the four tones is due to the fact that I used the same method as in the training series. I had formed a certain habit of imagery which served me in this test."

E. M. C.: "There is a marked relation between the training series and the four grays, the nine tones, the nine grays, and the four tones, but apparently not much relation between the other experiments and the training series. Practically all the gain shown in the second test is due to the influence of the method used in the training series."

H. C. E.: "I think the thing that accounts for the improvement in the second test over the first is the system or method which was developed from the training series. In the last test series, I used the same method as in the training series, except that the system of imagery was changed slightly for the grays; that is, the grays appear in a vertical row just as for the tones, but instead of each being represented by a disk of different size they are now the same size and have the respective brightnesses of the four grays. I think it would make no difference what sort of tests I might be given where these four numbers were used; I could do as well as with the tones and grays."

Untrained Observers.

The methods used by the untrained observers will not be described unless they differ, in particular tests, from the methods used by the trained observers.

The Four Grays. Three of the four observers note a tendency to visualize the four grays. M. C. says, "The visual impressions seemed rather strong during the first test." In the second test, however, she abandoned the attempt to image. M. D. F. speaks of the use of an imagery he had adopted in some of the first tests. In the second test D. D. W. says, "I

tried to remember the grays as a row of figures; also to get a mental picture of them but the time was too short.''

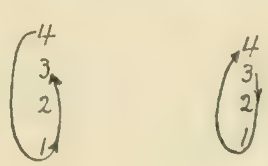
The Nine Tones. M. C. says that she saw the figures in a "group imagery" and retained them in that way. M. D. F. tried to repeat the numbers as tones, that is, high tones to low tones in each group, or low tones to high in each group of four. The four tones gave him an image of a board with four keys but he is not able to state the form of the keys or of the board, but tried to remember how the place or point suggested by each tone would skip around. In the introspection for the second test, he says:

"I can see fairly distinctly before me a key-board, and the tones go up and down. Four is at the top, and one is at the bottom, and I simply let the tone suggest the position, and when it comes time to respond, the image of the key-board returns. This test seems easier for me than the other experiments and also easier than the first test of this experiment, that is, it appears that the key-board helps me to remember."

With D. D. W. and J. W., a number of methods were tried during the first test. Both adopted the method of grouping by fours during the second test.

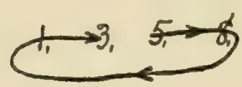
The Nine Grays. In the first test, M. C. tried to picture the numbers as they were given and also to note how often one of any kind was repeated. In the second test he tried to remember the nine grays in groups of fours, but this time they appeared not in imagery of figures, but as a picture of the dots. This aided materially in retaining the groups. J. W. says that he tried visualizing, but it was a complete failure because of lack of time to recall the picture in detail. M. D. F. says:

"Each color as it appeared suggested a number, and I had no trouble in classifying the colors. Nine, however, was too great a number for me to retain at once. Some groups I found much easier than others, for instance, 4, 1, 2, 3, and 3, 2, 1, 4, etc., appeared arranged in a vertical column, and attention skips from the four, which is always at the top, to the other numbers in the order in which they were given, somewhat as follows:"



M. D. F. used the same method in the second test. D. D. W. said that he tried to remember all the numbers of the group, but the nine seconds interval was too short, to form a distinct image of either the grays or the numbers they represent.

The Four Tones. M. C. thinks that the imagery which was used in the nine grays assisted here, but says that the different response interfered. In the first test J. W. tried to form an auditory image of the four tones but failed. He complains of an annoying fusion of the tones, a sort of harmony, which prevented him from remembering. With M. D. F. each note suggested a position on the piano, as 1, 3, 5, 8, and he remembered the order of the tones in that position. For instance, 5, 8, 1, 3, would be like this:



As long as he kept his mind concentrated on the above form he could remember, but it was very easy to allow the second group to confuse the one already received.

The final introspection for M. C. describes the form of imagery used and therefore it is quoted entire:

"I felt that I did better in the last end series than in the first, because I understood what I was supposed to do and was not confused. The only thing I know that helped me was the picture of the four dots, the dots appearing in a horizontal line,—

It seemed to me that if I could close my eyes during the test for the four and the nine grays, and not have the other group follow so closely, I could get them all correct. I used this imagery in my last tests for the four groups, but it seemed much easier with the grays than with the four or nine tones. This method did not help me in the other four tests. They seemed independent, and concentration in the first test did not help me in any of these four tests—geometrical figures, nine numbers, poetry, and movement. My greatest hindrance was that I often felt that I was not getting them right, and that confused me so that I could not concentrate my attention."

COMPARISON OF RESULTS.

Definition of Transference. It is first necessary for us to seek an understanding of what is meant by transference. We may mean by transference, the ability to use in one act the elements involved in another act. If we mean by transference that the training one receives in using a number of elements in one act is transferred to another act in which these elements do not occur, then the phrase "spread of training" would describe our meaning more accurately. In the sense of "spread of training," we can hardly say that there is "transference." A technical meaning of transference is answered only by the first definition. Both conceptions are involved in the present experiments.

If, as in the first definition, transference means simply the use of identical elements in different tasks, then the analysis of the conditions must be somewhat as follows: Let us suppose that any act is composed of the elements a, b, c, d, e; another act of the elements l, m, n, o, p, The two acts would be represented by two sets of elements, totally different, in which case there could be no such thing as transference. If, however, we examine an act composed of the elements a, b, c, d, e, and a second act composed of the elements e, f, g, h, i, where the element e appears in both acts, we would have the possibility of transference as far as the element "e" is concerned. If we had an act composed of the elements a, b, c, d, e, and another composed of the elements b, c, d, e, we would have a much stronger possibility of transference since all the elements except a in the first act are identical.

But, in the case of "spread of training" if we had an act composed of the elements a, b, c, d, e, and another act composed of the elements f, g, h, i, j, then the influence of training in the first act upon the elements composing the second act, will be measured by the amount of relationship between the elements of the two acts, that is, it will be measured by the amount of subconscious and actively conscious connection made between the elements by the observer. The application of the principle of "spread of training" can be made only to such cases where the elements concerned in the two acts are not identical, but related.

Of course, it were to beg the question if we used the phrase "natural connection," or the phrase "a natural relationship." The experimental difficulty involved is to determine which elements of experience are identical, which are related and which unrelated. The answer differs with individuals. Things related in one mind are not necessarily related in another mind. The relation between the things in experience depends, therefore, upon the relation which each person establishes in his own life—whether the frequent occurrence together of elements has built them into a purely automatic relation, or a connection is worked out by logical consciousness. At any stage of life an individual has a lot of relations which are automatic and another lot in which he has traced out a conscious relationship. We are often surprised by the discovery of a relation where we thought none existed. On the other hand we must guard against the conception that any mental components of a complex act enter into another complex act without being modified.

Relation of the Gain by the Trained to the Gain by the Untrained.

The following lists show the order of the tests arranged according to the greatest improvement for the trained and untrained observers:

Trained Observers.	*Untrained Observers.*
Four Grays	Nine Tones
Nine Tones	Nine Grays
Nine Grays	Geometrical Figures
Geometrical Figures	Four Grays
Four Tones	Poetry
Poetry	Nine Numbers
Nine Numbers	Movement
Movement	Four Tones

The rank of these tests, as shown by the greatest gain foı each observer, is indicated in Table III. The four grays, with seven observers, show the greatest gain, and second greatest gain with two observers. The nine tones received first rank

with three observers, and second rank with five observers. The nine grays received first rank with two observers and second with two. The four tones received no first or second rank.

TABLE III.

Comparison of Results by Gain or Loss.

	G. C. F.	D. S.	F. S.	E. M. C.	H. C. E.	A. R. F.	M. M. M.	M. L.	M. C.	J. W.	M. B. F.	D. D. W.	Average Trained	Average Untrained	Difference between Trained and Untrained.
Training Series	32 (24)	25	−9	41	27	29	17	[9]					21		
Four Grays	22	*37*	*26*	*48*	56	*39*	*23*	*39*	2	7	*15*	−7	36	4	*32*
Nine Tones	*34*	35	1	15	*59*	8	5	15	5	9	14	*14*	22	11	10
Nine Grays	2	26	−2	12	56	32	2	21	*21*	*11*	0	8	19	10	9
Four Tones	−6	3	22	15	23	20	−16	19	2	−17	0	6	10	−2	12
Average	13	25	12	23	49	25	4	24	8	11	15	5	22	6	16
Difference between training gain and test of greatest gain	2	12	35	7	32	10	6	30							
Geom. Figs.	9	*11*	0	6	*35*	4	*18*	*22*	*7*	*20*	*5*	0	13	8	5
Nine Numbers	2	*11*	*4*	−2	3	4	9	7	−8	4	2	2	4	0	4
Movement	−1	−2	1	−1	−1	−1	−3	6	−2	1	−4	1	0	−1	−1
Poetry	*15*	−3	0	*19*	0	*10*		8	0	0	2	*5*	7	2	5
Average	6	4	1	6	10	5	6	11	0	6	1	2	6	3	3

All figures represent differences between first and last tests.
The record of the test which shows the greatest gain is printed in italics.
Minus sign means loss.

In Table III, the tests are ranked by similarity and by dissimilarity to training series. In the ranking for first and second places geometrical figures received first rank with six observers and second with three. The nine numbers received first rank with two observers, and poetry received first rank with four observers. Movement did not receive first, second or third rank with any observer.

The amount of gain of the trained over the untrained is shown by the amounts recorded in the last column of Table III. This column shows that for the four grays there was a difference between the results for the trained and the untrained of 32 per cent, or a gain nine times greater in the trained than in the untrained; for the nine tones, a difference of 10 per cent, or a gain twice as great for the trained as for the untrained; for the nine grays a difference of 9 per cent or a gain twice as great; for the four tones, a difference of 12 per cent; for geometrical figures, a difference of 5 per cent; for the nine numbers, a gain of 4 per cent; for the movement a difference of – 1 per cent; and for poetry a difference of 5 per cent.

The difference of the gain of the trained over the untrained in tests intentionally similar to the training series is 16 per cent. The corresponding difference between the trained and the untrained for tests intentionally dissimilar is 3 per cent. The gain of the trained in tests similar to the training is three and one-half times as great as their gain in tests dissimilar. The gain of the untrained in tests similar to the training is twice as great as their gain in tests dissimilar.

Let us now analyze, if possible, the influence of the training series upon each of these tests.

The four grays differed from the training series in but one factor, that is, grays instead of tones were used for stimuli. A reference to the table shows that the average gain for all observers in the four grays over the training series, is about 15 per cent. This is true of every observer except one.

The test showing the second greatest gain was the nine tones. Here the test differed from the training series in method, but was identical in content. In estimating the influence of the training upon test experiments in tones the first test may be regarded as the first practice series. This would tend to lift the recorded results for the first training above the first record for the tests in tones. This, in turn, would tend to increase the gain of the test over that of the training series. On the part of five of the observers there was a gain in the nine tones greater than the gain in training. The average shows a difference of 1 per cent in favor of the nine tones.

The nine grays show the third greatest gain. In the case of five observers the gain is greater in the nine grays than in the training. The difference between the averages of the two for all observers is greater by 2 per cent for the training than for the nine grays. The nine grays differ from the training series in both content and method.

The four tones differ from the training series in the use of pitch differences, instead of differences of intensity of the same tone, and in the method of response. The results show a gain in the training series greater than in the four tones, except for two observers. In reality there is a gain in the tones greater than in the training in the case of one observer only, for M. L. trained but two days. F. S., the other observer, made no gain in his training, but there was a gain of 22 per cent in the four tones. It seems that of those who gained in training and trained the full time, there was in no case a gain in the four tones equal to the gain in the training. In two cases there was a loss in the four tones. For all observers the average shows that the gain in the training was twice as great as in the tones. It is to be pointed out that this test is very similar to the training series. The different response required was planned because it was thought that it would be more familiar to the observers than the number response. As already mentioned, however, the introspections show that it was the different nature of the response that accounted for the failure to gain more in the four tones; several observers speak of this as a hindrance to correct responses.

Table III, shows also the difference between the gain in the training series and the gain in the test making the greatest gain. Every observer made a greater gain in these latter than in the training series.

The test showing the greatest gain for trained observers in the class of dissimilar tests was geometrical figures. In the case of two observers (including the observer trained for two days only) there was a greater gain in this test than in the training series for the same observers. This is the test among those unlike the training series in which the untrained observers make the greatest gain. This test also shows a greater gain in the

case of trained observers than does the test for the four tones. But for the other dissimilar tests there is so small an improvement that the trained observers gain on the average three and one-half times as much in the similar tests as in the dissimilar ones.

Table III indicates great variations in the amount of improvement made by different observers in the different tests. It might be expected, judging from the way the experiments were planned, that it would be easy to subtract the amount of improvement in the several tests from the improvement in the training series, and thus arrive at a direct estimate of the influence of the training upon that particular test. This, however, is not an easy matter. Individual differences and factors beyond those brought out in the numerical results enter to complicate the estimates. It is only possible to make such an analysis when the introspective evidence is sufficiently full and accurate to enumerate and define all the factors involved.

Three significant features have thus been noted in the above table; first, the difference between the improvement of the trained over the untrained; second, the difference of improvement in the tests similar to the training series in trained observers over their improvement in tests dissimilar; and third, the greater amount of improvement in the tests than in the training.

The Factors in Improvement and Transference.

The things most commonly mentioned by observers as contributing to improvement and transference have already been enumerated. It remains to point out some of the considerations bearing upon the interpretation of these factors.

Imagery. No suggestions were given observers regarding imagery. Indeed, it was not until the experiments were completed and work on results began that the uniformity of the testimony of observers concerning it was realized.

Eight of the twelve observers, all but two of the eight trained and two of the untrained, record a specific type of imagery. The two trained observers, F. S. and E. M. C., who did not do so, show by the language of their introspections that they

used imagery, but did not recognize a particular form. Of the untrained observers, two recorded the development of a complete imagery system, while with the other two there was a strong tendency to develop an imagery. As is shown by their introspections, in the case of observers who took only the test series, it is evident that the brevity of the tests together with the rapidity of change, would not permit the development of imagery with such facility as in the training series. Nevertheless two of the untrained observers, M. C., and M. D. F., developed a specific, well recognized imagery.

The fact that the stimulus of the training series was sound would lead us to expect auditory imagery. However, reference to the introspections already quoted will sufficiently indicate that there are three main types of imagery represented among the observers. G. C. F.'s may be called a visual-auditory type; that of D. S. and M. C., a purely visual type, and that of M. M. M., A. R. F., H. C. E., M. L., and M. D. F., a visual-motor type.

Everyone who does any act like this memorizing has a characteristic method. The evidence derived from these experiments indicates that the essential element in method is imagery.

Having once selected, consciously or unconsciously, an imagery, improvement seems to depend upon the fidelity of the observer to that imagery. Improvement depends also upon the fitness or adequacy of the imagery to do the thing for which it was adopted.

Whether each has an imagery for each separate act, whether each has a great many forms of imagery, corresponding perhaps to the customary things of life, or whether we have a few forms of imagery which we use for many different things, are interesting questions. If we do not have an imagery for each act, then the question of the use of imagery in different acts is just the one we are seeking to answer in regard to "transference" or "spread of training" by these experiments.

If an imagery is selected which is complicated, such as that of A. R. F., the observer is doing no other than selecting a complex method, which requires longer use to secure accuracy and speed. Or, if one selects an imagery which is not adequate to

the task as a whole, but serves for part only, such as an imagery for certain groups of sounds in the practice series, illustrated possibly by the imagery of D. S., then the observer must adopt a double or even a manifold system of imagery, and improvement in speed and accuracy would seem slower. Also, if one should change his type of imagery, it would lead to a lack of improvement or to fluctuation in improvement. This is a possible explanation of the failure of F. S. to improve.

For a short practice series, it would seem better to adopt a method or type of imagery as soon as possible, and, even though it is found to be cumbersome, remain faithful to it; for tasks long continued or to be oft repeated, the sooner one selects the best imagery, the better for the final outcome. Native ability finds its field in the readiness with which one selects an imagery adequate to secure the accuracy and speed demanded by a skillful performance of the task.

The prominence thus given to imagery as the essential characteristic of method has been pointed out before. Binet, in his "Psychology of Reasoning," insists upon imagery as the essential factor in all mental operations. Nearly every research in imagery since then has indicated something of the large place which imagery occupies in mental life. Coover and Angell have shown the value of the "careful elaboration of the plan of work, the actual working out of the method in the form of detailed introspections, and the searching and thorough analysis of results in experiments of this kind." Their research, however, not only fails to bring out the fact of an individual imagery, but even seems to seek to eliminate imagery altogether as a factor in improvement in training. Its presence, however, seems to be indicated in some of the introspections quoted.

"Am able to abstract from visual imagery of the apparatus entirely, and yet refer sounds to external stimuli. This seems to take the least effort, and is most satisfactory,"

They say—"The introspections indicate that the discrimination processes were accompanied by much imagery from other domains of sense, which in some cases determined the judgment. This imagery was largely kinesthetic and visual." "One reagent seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, e. g., the 'flash of a bicycle lamp.'"

Still they say explicitly—"Many introspections * * * near the end of training were, 'No imagery.'"[1]

The relation of type of imagery to "transference" or "spread of training" is indicated in part by the results. In the case of G. C. F., the visual-auditory imagery used was that of a localization in space of the four tones. When in the test experiments, the four tones were changed to grays, there was a strong tendency to remember the grays in the same manner as the tones were remembered, because the stimulus rhythm and the method of response were the same. But the grays refused to take the position in space that had been customary in the case of the tones. An improvement was made between the tests for grays, but this tendency to use the practice imagery had to be overcome. The more thoroughly he was trained in the use of his imagery the less able was he to make a good record in the tests where he found a tendency to use it, but to which it seemed inapplicable. He gained about 20 per cent during his second practice period, but the results of the third test series shows not only no gain over the second test results but an actual loss of 8 per cent in the four grays, and of 6 per cent in the nine tones. These are, however, the tests in which he had made the greatest gain between the first and second tests. He gained but 3 per cent in the nine grays in the third test, and he made no gain in the four tones. His tendency to gain is shown by the gains of 11 per cent in poetry, 10 per cent in nine numbers, and 6 per cent in geometrical figures.

Nearly every observer, especially those who developed a clear imagery, was troubled with the same difficulty in the case of the four tones, for here the change in the response interfered with the use of the practice imagery.

On the other hand with D. S., H. C. E., A. R. F., M. M. M., M. L., M. C., and M. D. F., the type of imagery developed was as easily used with the grays as with the tones. These observers illustrate the benefits of making an imagery capable of being used in several acts thoroughly automatic as quickly as possible. With these observers, the longer

[1] Coover and Angell, *op. cit.*

they were trained, the easier it became to use the automatic imagery in the tests. So strong did this connection seem to H. C. E., that he said: "I think it would make no difference what sort of test I might be given where these four numbers were used; I could do equally well as with the tones and grays." The longer the observer was trained, therefore, the more non-essentials were cast aside, while the few essentials became habitual. When attention was long confined to the essentials, each element among them became welded into the imagery system.

Now, if a task differing in one essential only, from the trained one is given, the whole system feels the shock of the change in a vital part, until the adjustment is made. If the new task differed in two or three points, the shock is still greater. If the task was so different that the observer recognized no similarity, that is, if for him there was no way of applying his system of imagery, or if the imagery did not apply itself, then a new system of imagery was built up for the new task. It would seem, then, that the best time to make transfers of training in tasks which we recognize as dissimilar, is in the moments of beginning a new task, because the non-essentials which we use at first may be the essential ones in the second task. Thus, there may be advantages in learning several acts at about the same time.

Cases in which the amount of improvement in the test is greater than the amount of improvement in the training, are explained in part by the nature of the imagery used by the observers; the imagery used by the majority of observers was more readily applied to the tests than to the training. Such imagery as that of H. C. E., A. R. F., M. M. M., M. L., M. C., and M. D. F., supports such a view. The question of transference, then, becomes in very large part, a question of the nature of the imagery employed in the practiced task.

The significance of practice in the first test must be estimated here. Tables I and II show the difference between the first and second trial of each test, for both the first and second tests. It will be noticed that the gain between the first and second trials of the first test is often greater than the gain between the two tests. This is in accord with the well known fact that practice shows the greatest gain at the beginning of a training series.

The influence of one end test upon another is, therefore, the more serious in the "before" tests; and the effect of these tests upon the beginning of the training series may, in some cases, amount to more than the effect of a day of training in the training series.

The relation of improvement to one's ideas of improvement has often been raised as an experimental question in psychology. Many experiments have shown how often results differ from the feeling regarding improvement. It seems probable, from this series of experiments, that the feeling of improvement or the lack of it, is more or less closely connected with familiarity or lack of familiarity with imagery. All observers in this series of experiments were kept ignorant of results, but were asked to note in their introspections their own feelings regarding improvement. It often occurred that the feeling and the fact coincided. This seems to be more uniformly true in the case of those who developed a recognized form of imagery. It seems to be more often true in the case of those who did not recognize an imagery, and of those who had not yet recognized their imagery, that the fact did not correspond to the feeling.

The factor of attention and its control seems to be an important one in improvement and transference. In the opinion of observers it ranks next after imagery. Introspections at the beginning of the tests, and early in the training, show that observers recognize attention or the lack of its control as an important element in selecting the essentials from the non-essentials. Many speak of the rapid fluctuations of attention at this time. Observers who had a vivid imagery, speak of the fluctuations of attention in the use of the imagery; later in training, when the use of the imagery has become automatic, they say that control of attention seems to be the chief factor in rapid improvement, and the lack of it, the cause of error. Nearly every observer who seemed to approach the limit of his ability in training, testifies that the slightest fluctuation of attention produces a change in the results. In early training, therefore, attention seems to be drawn easily to the new conditions of work, i. e., to non-essentials. In improvement during practice, attention is more and more given to the central element concerned,

i. e., to the imagery which the observer uses. Toward the limits of training, attention may be permitted to run off on associations for the automatism of the imagery permits extra time between the stimulus and the response. When observers are making every effort to miss no stimulus or response, slight disturbances, such as slight changes in the stimulus, or noises from without, break the rhythm and produce rapid changes in attention. Practice curves of these observers, if plotted with regard to the grouping of the fours, where there is no change of method, are an excellent representation of the normal fluctuations of attention.

Association is another factor in training and transference. Most observers say that at the beginning of training and in the first test there is no time to form other associations than those among the elements concerned, but toward the close of training, nearly all speak of lapses of attention, due to associations with outside things formed in the interval between the stimulus and the response, or between the response and the stimulus. The relation between the training series and the test series may be called association, but it is better defined from the standpoint of imagery.

Automatisms have already been mentioned several times in connection with training and transference. It is inevitable that they should be formed in any process of training. The rapidity with which they are produced depends directly upon the fidelity of the observer to the imagery adopted, and upon the simplicity or complexity of the imagery to the observer. For example, H. C. E. adopted an imagery the first day of training and used it throughout training. His imagery was to him easy and readily used, and became automatic very quickly and thoroughly. A. R. F. did not recognize an imagery early and, when he did recognize it, it seemed to him complex and difficult of use. His imagery became automatic slowly, and before it became very thoroughly so, the training was over.

The relation of automatisms to the final tests is one of assistance, or of interference. The more automatic an act becomes, the less likely are its elements to be transferred to unlike elements. If it can be used, all goes smoothly. But if the task

is sufficiently different in content or method, for the observer, to awaken conscious efforts to use it in the new tasks, then automatism becomes a hindrance. In improvement in training, therefore, the more quickly automatisms may be cultivated the better. In transference, the cultivation of automatisms may be either a help or a hindrance according to the nature of the imagery of the observer.

GENERAL CONCLUSIONS.

The original research by Professor James (Psych., Vol. 1, p. 667), which served as the starting point for the investigations, contains this sentence, "All improvement of memory consists in the improvement of one's habitual methods of recording facts." Several experimenters have interpreted their facts for or against James' conclusions as seemed evident to them. The fact is, however, that many researches interpreted adversely are capable of interpretation to support his contention. A research which the writer carried on with Professor Gilbert, published in the University of Iowa Studies in Psychology, Vol. I, on "Practice in Reaction and Discrimination" left a distinct impression in the writer's mind that Professor James was wrong. The evidence of that same research seems now to be capable of an interpretation in support of Professor James as otherwise. Among the researches which have been interpreted as against James' conclusions are those of Judd (Psy. Rev., Vol. IX, pp. 27 to 39); several researches on cross-education, such as those of Scripture, Smith and Brown (Yale Studies Vol. II), Davis (Yale Studies, Vols. VI and VIII), Ebert and Meumann (Arch. f. d. ges. Psy., Bd. 4). The researches which take the ground apparently in support of Professor James are those of Thorndike and Woodworth (Psy. Rev., Vol. VIII), Bair (Psy. Rev., Mono. Sup. No. 19), Coover and Angell (Am. Jour. of Psy., XVIII, p. 328). A distinct effort to analyze the elements concerned in improvement in practice and in transference has characterized the later researches. As typical of this tendency, we may quote the researches of Thorndike and Woodworth, and of Coover and Angell. Thorndike and Woodworth say that after

practicing with rectangles 10 to 100 sq. cm., observers learn that one has a tendency to over-estimate all areas and consciously make a discount for this tendency, no matter how different other sizes or shapes of surfaces used in tests may be; also to look for the variations or the exceptional occurrences among the elements involved in training and in tests; third, learning to estimate in comparison with a mental standard, rather than an objective standard. This analysis of factors involved has a bearing only upon the tests carried on by these experimenters. They simply point out what seem to them to be the elements in their set of experiments.

Coover and Angell give a more translatable list of elements that seem to them concerned in improvement and transference:

> "We find, therefore, causes of the tranference of facility to be: (a) the formation of a habit of reacting directly to a stimulus without useless kinesthetic, acoustic, and motor accompaniments of recognition, which results in (b) an equitable distribution of attention to the various possible reactions so as to be about equally prepared for all; and (c) the consequent power of concentrating the attention throughout the whole series without distraction."

The elements that appear on the surface in our experiments are, while in the main in support of the analysis given by Thorndike and Woodworth, and Coover and Angell, contain elements both somewhat at variance with, and in addition to, those discovered in these researches. If, in the following from Coover and Angell: "Improvement seems to consist of divesting the essential process of the unessential factors, freeing judgments from illusions, to which the unnecessary and often fantastic imagery gives rise, and of obtaining a uniform state of attention which is less than a maximum," and "useless kinesthetic, acoustic and motor accompaniments of recognition," by "fantastic imagery" is meant such imagery as appears in our experiments or if it means such imagery as one of Coover and Angell's observers mentions, when he "seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, e. g., the "flash of a bicycle lamp," then we must regard our results as distinctly divergent. Such imagery is an essential factor, if not the most essential factor in training and transference. With Coover and Angell's

general conclusion regarding the factors common in cases of training of dissimilar stimuli; i. e., "the habit of stripping the essential process of unnecessary and complicating accessories," we are in agreement.

In regard to the experiments of Thorndike and Woodworth, the difference between their conclusions and the conclusions of this series may be pointed out as follows: "After one gets mental standards of the areas, he judges more accurately, if he pays no attention whatever to objective standards." If Thorndike and Woodworth mean by this the same condition of imagery as developed in our experiments, which we imagine is possible, that is one point of agreement.

"Improvement in any single mental function need not improve the ability in functions commonly called by the same name. It may injure it." With this our conclusions also agree. Some definition, however, as Thorndike admits, needs to be made of the phrase "single mental function."

"Improvement in any single mental function rarely brings about equal improvement in any other function, no matter how similar, for the working of every mental function-group is conditioned by the nature of the data in each particular case." The results of our experiments do not support the statement contained in this sentence, especially in the first half of it. Improvement in many cases is absolutely greater in amount in the tests than in the training. The truth of the latter part of the quotation is verified in our experiments if the word "imagery" may be substituted for the word "data."

"The very slight amount of variation in the nature of the data necessary to affect the efficiency of a function-group makes it fair to infer that no change in the data, however slight, is without effect on the function." This our results corroborate.

"The loss in the efficiency of a function trained with certain data, as we pass to data more and more unlike the first, makes it fair to infer that there is always a point where the loss is complete, a point beyond which the influence of the training has not extended." Again our results corroborate.

"The rapidity of this loss, that is, its amount in the case of

data very similar to the data on which the function was trained, makes it fair to infer that this point is nearer than has been supposed. Again our results corroborate.

In the light of results here secured, we would change the following statement: "The general consideration of the cases of retention or of loss of practice effect seems to make it likely that spread of training occurs only where identical elements are concerned in the influencing and influenced function," to read—spread of practice occurs only where an imagery develops capable of being used by the individual observer in both training and test fields.

Our results do not corroborate the following statement from Coover and Angell, p. 339, as far as the freeing from any system is concerned:

> "At the beginning of training, they matched the color of the cards with the labels on the compartments; then to increase speed a system of mnemonics is employed, designed to form associations in the mind between a compartment and its color; this system then goes through a process of mutation,—becomes abbreviated, changed in part, supplemented, or is superseded by another; finally, through repetition, reactions to particular compartments become coordinated with their respective colors and are made directly—free from any 'system' except in rare cases."

The evidence from the introspections of all of our observers shows that there is no tendency to do away with the imagery or to free from the imagery system. Such cases as D. S., who had been trained by a long series of reactions practically identical with those in which he is trained here, and of G. C. F., who was trained for two months in the practice reactions used in this experiment, reactions which were selected for the intensity of application required in improvement and because of a possibility of reaching the limit of training for different observers within the practice period—such cases do not show any tendency to abandon the system. In this, therefore, our results do not agree with those of Coover and Angell.

With the statement of Professor James our results are in accord inasmuch as all the factors we have discovered have to do with methods.

There are two factors then, which we are seeking to analyze; first, to determine the factors that make for improvement; and

second, to determine the factors that make for spread of training or transference of training. If the problem were attacked from the standpoint of numerical results only, the analysis into elements would be most confusing.

SUMMARY OF CONCLUSIONS.

Some elements concerned in *improvement* and *transference* have been enumerated. Of these the central or most essential element is individual imagery.

Improvement seems to depend upon the consistent use of some form of imagery, whether it is the most advantageous form or not.

Imagery may be sub-consciously developed, but if it comes to be consciously recognized the improvement is more rapid. The rate of improvement seems to depend directly upon the conscious recognition of the imagery, and upon attention to its use.

A change of imagery during practice increases the rapidity of the improvement if a better form is adopted and adhered to. It may prevent improvement if a change of imagery is frequent, or if a less adequate form is adopted.

Individual differences are clearly shown in different types of imagery by the rapidity with which the imagery develops, and by the clearness or definiteness of the imagery.

The habit of guessing interferes with the formation of imagery and therefore, results in lack of improvement.

Transference may be divided into two kinds. It is either the use of identical elements in different tasks, or it is of the nature of "spread of training." The evidence of these experiments is in favor of the use of identical elements, or at least in favor of a limited spread of training. We are able to say that transference depends upon the nature of the imagery employed in practice, rather than upon any other factor. Whenever the training has become automatic and the difference between the training and the test consists of a few elements, these different elements serve as a hindrance only. We have then something of the nature of spread of training. If the difference is so very

slight that the elements are practically identical, as between the four tones of the training series and the four grays, there is little difference between the gain of the training and the test series. We have here something of the nature of transference, though transference as we have defined it, demands a complete identity between the elements of the acts. When the acts are made up of quite different elements, there is a distinct breaking up of the habit of responding, by the intrusion of the different elements, which raises the whole act into active consciousness so that the transfer of elements from one act to another act, other than the identical ones, is a conscious transference. It seems, therefore, that a conscious effort to use the elements of training in a different task, assists in making the transfer.

Factors that lead to improvement in the training do not necessarily lead to improvement in the tests; they may hinder it. The nature of the imagery, and the training in it seems to determine this. If, in the mind of the observer, the imagery is capable of adjustment to different tasks, it can be used in both improvement and transference, for the elements of the training act are thereby made the same as those of the test act. If it is adapted, in the mind of the observer, to the training task only, it may assist in improvement but it may interfere with transference.

Native ability appears to have abundant opportunity in the recognition of similarity or difference in the capability of the imagery for use in various tasks.[1]

[1] "Images, along with sensations, constitute the material of all intellectual operations: memory, reasoning, imagination, are acts which consist, in an ultimate analysis, of grouping and co-ordinating images, in apprehending the relations already formed between them, and in reuniting them into new relations." Binet, Psychology of Reasoning.

"Just as the body is a polypus of cells, the mind is a polypus of images." Taine, "On Intelligence."

THE EFFECT OF PRACTICE ON NORMAL ILLUSIONS

BY

C. E. SEASHORE, EDWARD A. CARTER, EVA CRANE FARNUM, AND RAYMOND W. SIES.

The following experiments are an outgrowth from experiments made by one of the writers in 1894 on the persistence of the size-weight illusion.[1] At the time that those experiments were made illusions of the kind were comparatively unknown and it so happened that the four observers experimented upon were all completely ignorant of the illusion. They had, however, been selected as the most intelligent and cautious observers among the advanced students. The experiments ran through twenty days, a half hour each day, with each observer. All exhibited a strong normal illusion, somewhat larger than that found as the average for ten other observers of the same type. And all came out alike in showing the surprising conclusion that the twenty days of practice revealed no tendency to decrease the illusion. At the conclusion of these experiments the results and the significance of the illusion were explained in detail to each observer and another test was made to determine the effect of knowledge of the illusion. Again the results were alike for all the observers showing that the knowledge of the illusion immediately decreased it by nearly one-half of its original force.

The measurements on the persistence of the illusion of the vertical made by Dr. Williams in 1901 also belong to this series of studies.[2] She had three observers and trained them for ten days each making one hundred trials each day for each observer.

[1] Seashore, "Measurements of Illusions and Hallucinations in Normal Life," Yale Studies in Psychology, III, 5–9.

[2] Williams, "Normal Illusions in Representative Geometrical Forms," Univ. of Iowa, Studies in Psychology, III, 108–116.

The first of these observers had no knowledge of the illusion, the second had partial knowledge, and the third (C. E. S. of the present series) had full knowledge of it. The first two observers exhibited an abnormally large illusion, both averaging 21%; the third averaged 6%. The following conclusions were drawn:

> "The illusion fluctuates in strength from day to day, especially for the observers who are aware of its existence.
>
> The practice gained in one thousand trials does not decrease the force of the illusion of the vertical for the line: this is equally true for those observers who know of the illusion and those who do not know of it.
>
> For one observer, who has had extensive experience in the observation of this illusion for years, the illusion still has a normal force."

The results of the investigation first mentioned were remarkable in that they demonstrated the normal persistence of the illusion so long as the observer has no knowledge of it. The second investigation resulted in another surprise in that the illusion persisted also in those observers who had knowledge of it. In the meantime Judd[1] reported experiments on the Mueller-Lyer illusion showing that the illusion disappeared with practice, and without leaving any conscious trace of the process of correction. This became a further stimulus to a search into the conditions which determine the effect of practice, especially with reference to different types of illusion, different degrees of knowledge, different capacities in critical attitudes, duration of practice, and the consciousness of gain.

To contribute toward the solution of such problems the following four studies have been undertaken in coöperation, each devoted to one type of illusion. The first named writer is responsible for the general plan and supervision of the experiments and has written this article as a synopsis of the four independent reports written by the respective experimenters.

[1] Judd, "Practice and its Effects upon the Perception of Illusions," Psychological Review, IX, 27–39.

I. THE ILLUSIONS IN THE LENGTH OF A CYLINDER.

Measurements by Edward A. Carter.

A cylinder looks to be longer than it really is. This overestimation has been analyzed by Williams[1] into several constituent illusions. Thus, the dimensions of a surface are overestimated when compared with a line; the surface of a solid is overestimated when compared with a plane surface, and, there is some peculiarity about the cylinder which leads to a further overestimation of its length. These three errors Williams has called respectively the area illusion, the volume illusion, and the illusion of cylinder length. When a cylinder is placed in a vertical position, the illusion of the vertical also enters.

Four observers went through similar training series on this illusion in its gross form, without any attempt to isolate the constituent elements.

As illusion objects we used three black metal cylinders, each 114 mm. in diameter and, respectively, 109 mm., 114 mm., and 119 mm. in length. The object in using more than one cylinder was to prevent the forming of any absolute standard of the length. The practice consisted in repeated measurements which were made in terms of a straight line by the method of production with the apparatus described by Williams.

This apparatus consisted essentially of a frame one meter square placed in an erect position and covered with manilla cardboard near the center of which a 2 mm. watch spring protruded through a slit and lay flush against the surface. The length of the exposed part of this spring was regulated by cords in the hands of the observer and a permanent millimeter scale on the back enabled the experimenter to record each setting. A similar frame having an inconspicuous wire support for the cylinder near its center was placed by the side of this. The frames were placed edge to edge and so turned that their centers would be on a level with the eyes of the observer and at right angles to the line of regard of the observer when seated at a distance of one meter.

[1] Williams, M. C., "Normal Illusions in Representative Geometrical Forms," Univ. of Iowa, Studies in Psychology, III, 38–139.

FIG. 1. Horizontal Position.

Observations were made on both the vertical and the horizontal positions of the cylinders. The vertical length was measured in terms of a vertical line and the horizontal length in terms of a horizontal line. Repeating this on the three cylinders made six independent sets of observations. Ten settings were made for each of

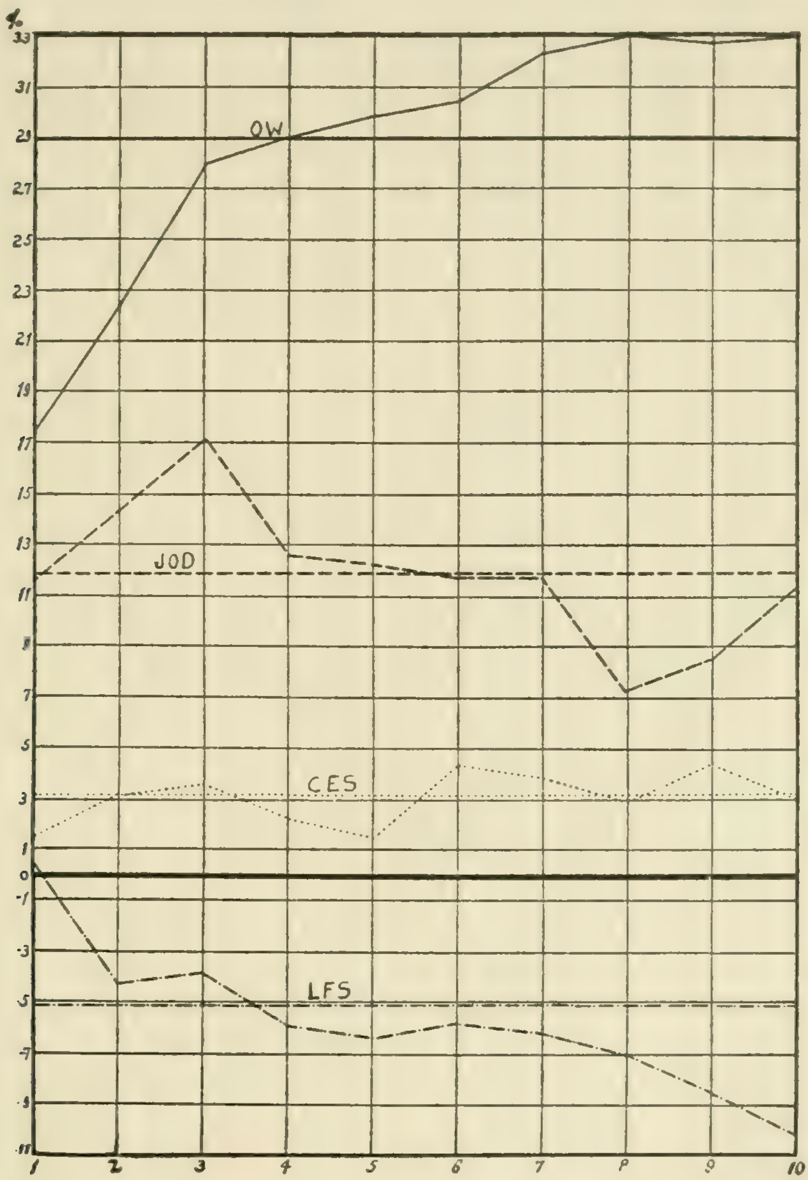


FIG. 2. Vertical Position.

these in each training period, which usually lasted an hour. The trials were made in the double fatigue order. The observers were allowed to look back and forth from the cylinder to the line as often as they wished, but were required to turn the head and not merely the eyes. All conditions of the experiments were

kept as nearly constant as possible. The experiments were made at the same time of the day and, as nearly as possible, on ten consecutive days. The observers were kept in complete ignorance about their results throughout the entire series. The task before the observer was quite simple—merely to represent, with the spring, a line which he was satisfied looked equal to the length of the cylinder. Unavoidable changes of attitude and development of theories were noted in introspective notes. The experiments were made during the winter, 1903–04.

The results of the experiments upon the four observers are contained in Tables I to IV, the average amount of the illusion and the mean variation for each day being given in percentages for the ten successive days.

Since the 109 mm. and the 119 mm. cylinders were used only for 'confusion' and the results for these differ in no essential way from the 114 mm. cylinder, the results of the three are combined and thrown into practice curves, Fig. 1 and 2. The percentage of illusion, figured on 114 mm. as a base, is indicated on the ordinates and the periods of practice on the abscissae. The average illusion for the ten days is indicated for each curve by a horizontal line of the same type as the curve.

A glance at the curves reveals the fact that the observers react to the practice and conditions of training in different ways. C. E. S. has a small illusion which remains approximately constant. L. F. S. unconsciously reacts to his knowledge of the illusion and reverses it. J. O. D. has an average illusion and gives no certain evidence of lowering it by practice. O. W. starts out with a strong illusion which rapidly increases throughout the whole training. We must consider each of these in some detail.

C. E. S. had full knowledge of the nature and force of the illusion, as well as of all the conditions of the experiment. He had had varied and extensive experience in the analysis and measurement of all the illusions involved. He expected the series to start with an illusion of about 10% for the vertical position, but did not have so definite expectation in regard to the horizontal position. Steadily avoiding to 'think it out,'

TABLE I. (C. E. S.).

A. Horizontal position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	5.5	1.8	5.3	1.8	8.4	2.3	6.4
2	7.3	1.8	6.1	1.7	6.7	1.7	6.7
3	7.3	1.8	6.1	1.8	6.7	2.3	6.7
4	7.3	1.8	7.0	0.9	7.6	2.3	7.3
5	3.7	0.9	5.3	2.7	3.7	2.3	4.2
6	6.4	1.8	4.4	1.8	7.6	1.6	6.1
7	7.3	1.8	7.0	0.9	7.6	1.7	7.3
8	5.5	2.6	6.1	1.7	7.6	2.3	6.4
9	9.2	1.8	7.9	1.8	9.2	1.6	8.8
10	7.3	1.8	5.3	1.8	8.4	1.7	7.0
Ave.	6.7	1.8	6.1	1.7	7.2	2.0	6.7

B. Vertical position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	2.7	1.8	1.8	1.8	0.0	2.3	1.5
2	3.7	2.7	3.5	1.7	2.3	1.6	3.2
3	2.8	1.8	4.4	2.6	3.4	2.3	3.5
4	1.8	1.8	2.6	1.8	2.3	1.7	2.1
5	1.8	1.8	2.6	1.7	0.0	1.6	1.5
6	3.7	1.8	5.3	2.6	4.1	1.7	4.4
7	4.6	1.8	3.5	1.8	3.4	1.6	3.8
8	3.7	1.7	2.6	2.6	2.3	1.6	2.9
9	3.7	2.7	5.3	1.7	4.1	0.8	4.4
10	3.7	2.7	2.6	1.8	3.4	1.6	3.2
Ave.	3.2	2.1	3.4	2.0	2.5	1.7	3.0

TABLE II. (L. F. S.).

A. Horizontal position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	13.8	2.7	12.3	5.3	12.6	2.3	12.9
2	12.8	2.7	14.0	1.7	10.0	1.6	12.3
3	13.8	0.9	11.4	3.5	9.2	1.6	11.1
4	14.7	2.7	12.3	2.6	9.2	2.3	12.2
5	12.8	1.8	11.4	1.7	10.9	2.3	11.7
6	15.6	1.8	14.9	1.7	13.4	1.6	14.6
7	17.4	1.8	15.8	1.7	14.3	1.6	15.8
8	12.8	1.8	13.2	2.6	16.0	0.8	14.0
9	9.2	1.8	7.0	0.9	7.6	1.6	7.9
10	5.5	2.6	6.1	0.8	5.0	1.6	5.5
Ave.	12.8	2.1	11.8	2.2	10.8	1.7	11.8

B. Vertical position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	3.7	0.9	− 0.9	1.7	− 1.6	1.6	0.4
2	− 3.7	1.8	− 2.6	1.7	− 6.7	0.8	− 4.3
3	− 2.7	1.8	− 3.5	1.7	− 5.0	2.3	− 3.7
4	− 3.7	3.7	− 6.1	1.8	− 7.6	1.6	− 5.8
5	− 3.7	1.8	− 7.9	1.7	− 7.6	3.4	− 7.1
6	− 4.6	2.7	− 6.1	1.8	− 6.7	1.6	− 5.8
7	− 4.6	1.8	− 6.1	1.8	− 7.6	2.3	− 6.1
8	− 5.5	1.8	− 7.0	1.7	− 8.4	1.6	− 7.0
9	− 9.2	1.8	− 8.8	1.8	− 9.2	1.7	− 9.1
10	−11.0	1.8	− 8.8	1.7	−10.9	1.6	−10.2
Ave.	− 4.5	2.0	− 5.8	1.7	− 7.1	1.8	− 5.8

TABLE III. (J. O. D.).

A. Horizontal position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	7.8	0.9	6.1	2.6	6.7	1.6	6.7
2	7.3	2.7	6.1	3.5	5.9	2.3	6.4
3	12.8	2.7	14.9	2.6	13.4	2.3	13.7
4	12.8	2.7	10.5	1.7	10.9	1.6	11.4
5	9.2	1.8	12.3	1.7	9.2	2.3	10.2
6	6.4	2.7	10.5	1.7	10.0	2.3	9.0
7	11.0	2.7	11.4	2.6	8.4	1.6	10.2
8	7.3	2.7	7.9	1.7	7.6	2.3	7.6
9	5.5	1.8	7.9	2.6	8.4	2.3	7.3
10	6.4	1.8	11.4	2.6	13.5	1.6	10.4
Ave.	8.6		9.9		9.4		9.3

B. Vertical position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	11.9	1.8	12.3	1.7	10.9	1.6	11.7
2	16.5	1.8	14.9	2.6	12.6	2.3	14.7
3	18.3	1.8	17.5	1.7	16.0	1.6	17.3
4	13.8	1.8	12.3	2.6	11.8	2.3	12.6
5	11.0	1.8	13.2	0.9	12.6	3.4	12.0
6	11.0	1.8	12.3	1.7	11.8	2.3	11.7
7	11.9	1.8	12.3	1.7	10.9	1.6	11.7
8	7.3	3.7	6.1	4.4	8.4	3.4	7.1
9	7.3	2.7	9.6	3.5	8.4	2.3	8.4
10	11.9	1.8	11.9	1.8	10.9	2.3	11.6
Ave.	12.1	2.1	12.2	2.2	11.4	2.3	11.9

Table IV. (o.w.).

A. Horizontal position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	22.9	5.5	27.2	4.4	26.9	3.4	25.7
2	32.1	3.7	36.0	3.5	32.8	2.3	33.6
3	40.4	3.4	38.6	3.5	36.1	3.4	38.4
4	46.8	3.8	44.7	2.6	45.4	4.1	45.6
5	49.5	6.4	49.1	3.5	47.9	4.1	48.8
6	52.3	3.7	51.7	2.6	46.2	4.1	50.1
7	50.0	3.7	39.5	3.5	46.2	2.3	45.2
8	51.4	2.7	47.4	3.5	47.1	5.0	48.6
9	53.2	3.7	54.4	3.5	47.9	3.4	51.8
10	56.9	3.7	53.5	2.6	51.3	2.3	53.9
Ave.	45.6	4.0	44.2	3.3	42.8	4.0	44.2

B. Vertical position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	17.4	3.7	19.2	3.4	16.0	3.4	17.5
2	27.5	3.7	21.9	2.6	18.5	3.4	22.6
3	26.6	3.7	24.6	4.4	32.8	5.0	28.0
4	29.4	5.6	28.9	7.0	28.6	6.8	29.0
5	33.9	4.6	24.6	5.3	30.2	2.3	29.6
6	32.1	3.7	32.4	4.4	26.9	2.8	30.5
7	35.8	2.8	30.5	3.6	31.1	2.3	32.5
8	35.8	3.7	33.3	2.6	30.3	3.4	33.1
9	32.1	3.7	36.0	3.5	30.3	2.3	32.8
10	33.9	4.6	34.3	4.4	31.1	3.4	33.1
Ave.	30.5	4.0	28.6	4.1	27.6	3.7	28.9

he continued throughout to feel that the illusion for the horizontal position would be less than for the vertical.

The average illusion in the horizontal position amounts to 6.7%; and in the vertical to 3%. There is no evidence of any constant tendency to change the force of the illusion with practice, and there is no appreciable progressive change in the mean variation. Both the per cent of illusion and the per cent of mean variation come out approximately equal for the three cylinders.

Among the introspective observations of C. E. S. the following are noteworthy:

"There is a much greater temptation to correct for the illusion of the vertical in the line than in the cylinder. It is more difficult to disregard a relatively simple illusion which readily intrudes in consciousness than one which is more complex and therefore becomes focal in consciousness only with effort. This may account for the difference in the force of the illusion in the vertical and the horizontal positions.

It takes greater effort to see the length of a cylinder than the length of a line. Looking beyond the near side of the cylinder gives distinct sensations of effort through the eye muscles. This peculiarity of effort is particularly noticeable in a visual comparison of the height of a cube or a square with the height of the cylinder. This suggests an explanation of the illusion of cylinder length which Williams isolated and determined quantitatively but was unable to explain on the data in hand at that time.[1]

There seem to be amœboid movements in the line; as one tries to size it up, there seems to be a creeping lengthening and shortening. This is traceable to change in the mode of regard; when one tries to get the line as a whole, there is a tendency to converge the eyes for a point beyond the plane of the line and, consequently, the line seems shorter—the retinal image being of a given size but the line being judged to lie farther away than it really is. This suggests an explanation for the illusion of filled space in a line which is simply bisected: the bisection favors the effort to regard the line as a whole and, therefore, the tendency to converge for a point behind the plane of the line.

It was soon noticed that these creeping, amœboid movements were invariably referred to one end of the line, the movable one. (Bear in mind that the line was represented by a spring which protruded through the background and that its length was changed by shoving the spring in or out.) The method employed afforded especially favorable conditions for the apperception of this effect. Ordinarily we are so sure that a line does not change in length while we look at it that we inhibit the actual sensory process. Here the line was being adjusted so frequently that I became particularly appreciative of movements, both real and apparent.

[1] Williams, op. cit.

There was a tendency to remember the length of the horizontal line and to be influenced by this absolute standard in the perception of the vertical. (For this observer, who was aware of the differences of the cylinders, this tendency may have contributed toward the difference in the illusion for the figures in the horizontal and the vertical positions.)

There is a tendency to turn the eyes instead of turning the head. This may account for the error often found in comparing the length of two objects some considerable distance apart. The one to which the eyes are turned without turning the head tends to be overestimated."

In order to determine whether the illusion is likely to be less when one feels particularly satisfied with a measurement, C. E. S. followed the plan of recording cases in which he felt especially sure and satisfied that the adjustment of the line was right. There are in all sixty-three such cases distributed as follows: Twenty-seven give a smaller illusion than the average for the day by an average of 2.0%; twenty-five give a larger illusion than the average for the day by 1.6%; eleven cases coincide with the average for the day. Taking the three groups together, they give an average mean variation of 1.5%. The average mean variation for the series is 1.9%; the 'sure' cases are, therefore, somewhat more reliable; and the number of these cases above the average illusion is about equal to the number below but those below are 0.4% farther away from the average than those above. Hence there is no prominent tendency except in the direction of a smaller mean variation.

L. F. S. was a graduate student with a keen and brilliant mind. He was familiar with the illusion of the vertical. He also knew that the cylinder looked longer than it really was, and this knowledge, though undifferentiated, was of a sort of bogey order because he had seen the illusion demonstrated in some extreme forms. He was not informed about the dimensions of the cylinders or the order in which they were presented.

His average illusion in the horizontal position is 11.8%. While there is a complete wave in the latter part of the curve, it cannot be said that there is any constant progressive change with the practice. His recognition of the area and the volume illusions on the ninth day, however, may account for the lowering of the curves on those days.

The average illusion for the vertical position is negative, aver-

aging – 5.8%, and in this there is a uniform increase throughout the practice. He starts with no illusion and ends with a minus record of – 10.2%. The mean variations remain approximately constant for both positions throughout the ten days.

In the introspective account written after the tenth day, but before knowing the results, the following items are particularly relevant:

> "Feel that I have not improved during the course of the observations. If there is any change, so far as I know, it must be due to practice."
>
> "Knew of the illusion of the vertical though I did not attempt to use this knowledge. The same is true of what I thought to be a 14% illusion of cylinder (length)."
>
> "Of the three cylinders, I think that the longest two are equal in length and that, in one of these, the length is equal to the diameter and, in the other, the length is greater than the diameter. The length and the diameter of the short cylinder are also equal."
>
> "I think that the illusion would be more pronounced in the vertical cylinder than in the horizontal. My records for the horizontal cylinders are more accurate than for the vertical. Occasionally I was aware of a tendency to make my judgment of the length of the vertical line by comparing it with what I remembered to be the length of the horizontal line for the corresponding cylinder. If then I would make the vertical seem equal to the horizontal, I would make it too short, on account of the illusion of the vertical."
>
> "Given a cylinder in the vertical position and a line in the vertical position, I think there would be a tendency to make the line too short. I became more aware of this tendency during the latter part of my observations."

On the ninth day he records that, instead of regarding the whole cylinders, he imagined a line drawn from end to end on the near surface of the cylinder. This is important because it is a condition which would tend to eliminate the illusions in the cylinder. (See notes by C. E. S. above.) The same day he records that he had not before thought distinctly of the effect of the area and the volume illusions. These two changes in attitude easily account for the downward turn in the curve in Fig. 1 on the ninth and the tenth days.

The explanation, then, of the results for the training for L. F. S. are essentially these. For the horizontal position the illusion is normal to a person 'with knowledge;' the lessening of the illusion on the last two days is accounted for by the two changes in attitudes, named above. This lowering is, however, not

greater than the immediately preceding rise, for which we have no explanation.

The reaction in the vertical position is partly accounted for by (1) the tendency to correct for the illusion of the vertical in simple and not in complex forms, as noted in the case of the first observer; (2) the bogey character of the gross illusion of length in the cylinder in the vertical position. (This observer did not know the force of the illusion for the horizontal position, and his estimate of 14% for the vertical position was a conservative estimate with reference to his own critical and discriminative attitude which excluded such force of the illusion as may be due to indiscriminate estimates); (3) the fact that he thought that the illusion would be more pronounced in the vertical than in the horizontal cylinder; and, (4) the fact that he considered the record for the horizontal cylinder most accurate, there being a feeling of unrest with reference to the vertical. The extreme negative results for the last two days have been accounted for above.

J. O. D., a liberal arts junior, was a distinguished athlete, slow in all his movements but a keen observer. He had heard an elementary lecture on the type of illusions involved and knew that the experiment involved these, but he did not have specific data clear in mind as a basis for correction although he was able to name some of the illusions. He had had no practice on this or any other illusion. The general effect of his information was to put him particularly on guard against all possible sources of inaccuracy. He accepted, without much questioning, the plain directions to make every possible effort to improve in accuracy.

The average illusion for the horizontal position is 9.3%; and, for the vertical, 11.9%. In neither case is there any constant tendency toward progressive change with the practice.

On the third day the observer recorded that he had been making the lines too long this day and attributed it to excessive fatigue. This explanation can, however, not be accepted without taking into consideration that he reported the same kind of fatigue and dullness on the fifth and the ninth days, and the illusion was almost average on the fifth day and below average

on the ninth. On the fourth day he reported a tendency to allow for the volume illusion 'by making the line a little longer.' This allowance was, of course, in the wrong direction; its effect is seen especially in the curve for the horizontal position.

He also noted the change corresponding to the fall in the curve on the eighth day and rightly attributed it to a scheme of imagining a plain line on the face of the cylinder, just as L. F. S. had done on the ninth day.

This observer noticed the "amoeboid" movements of the line on the first day.

O. W., a bright and painstaking liberal arts freshman, was a naïve and unprejudiced observer. He had not studied psychology and knew practically nothing about illusions. Special care was taken to keep him from getting any suspicion of the existence of illusions during the experiment. As a good student, he took the instructions in good faith and worked most diligently in the daily effort to cultivate accuracy in the use of his eyes.

His average illusion for the horizontal position is 44.2%; and, for the vertical, 28.9%. In both there is an unmistakable progressive increase in the illusion.

The observer being untrained, and the experiment being conducted with the effort to maintain a naïve state of mind, no introspective account was obtained. When he was shown the results at the conclusion of the series he was shocked, simply surrendered in a sort of despair and had no explanation to offer.

Our experiments resulted in no simple law of the effect of practice, but they enrich our insight into the actual complexity of the process. The four observers each represent an individual type of practice effect.

In the eight curves there are three cases of marked progressive change—two of increase in the illusion, and one of increase in the over-correction for it. The other five curves indicate no progressive change. To one who knows the observers and the conditions under which they worked these results seem 'strangely' natural. The first three observers had knowledge of the illusion and this probably reduced its force by as much as one-half. C. E. S. had gone through so much general training in illusions as to be free from disturbing motives which are due to

lack of a true point of view or lack of a discriminative attitude. J. O. D. was objective minded and naturally maintained the sensory-discriminative attitude in which the more rigid motives for the illusion gained uniform expression. L. F. S., who had not had sufficient training to guard him against the danger of being influenced by a partial knowledge of the situation, gave way to his analytical tendencies and made semi-conscious corrections, progressively increasing, in the vertical position. O. W. started with the strong illusion characteristic for hose who have no knowledge of it and, finding the task increasingly perplexing, probably changed mode of regard, etc., but for the worse and the illusion gradually increased.

If the drop in the curve for L. F. S. had taken place in the presence of the normal illusion there would have been danger of interpreting it as a clear case of gain through practice, but here it is clearly shown to be merely an expression of prejudice. This case is therefore particularly noteworthy.

The rise of the curve for O. W. is also noteworthy because it takes place for a person who already has a very strong illusion.

There is no distinct progressive tendency to increase or decrease the force of the illusion during the individual sittings, except in the three curves which show the progressive change for the whole series. There are many temporary fluctuations in the curves which may be accounted for by changes in method, etc., which one cannot fully preclude, but we have here discussed these experiments only from the point of view of progressive change.

II. THE T-ILLUSION.

Measurements by Eva Crane Farnum.

If two straight lines are joined in the shape of a plain capital T the one which is bisected seems to be shorter than the other. This is true though in different degrees, in all positions of the figure, when the illusion of the vertical has been eliminated.

This illusion was selected as representing a type which is probably due to lack of discriminative observation. It is usually very strong for one who is not aware of it. It was thought that mere practice, without information, would lead quickly to

discriminative apperception which would eradicate the illusion.

The illusion has never been fully explained. An analysis of the figure reveals several motives. First, as the two lines are at right angles, the illusion of the vertical enters. When the bisecting line is in the vertical position, the illusion of the vertical coöperates with the T-illusion; but, when the bisecting line is in the horizontal position, the illusion of the vertical counteracts the T-illusion. This motive may be fairly eliminated by studying the figure in both vertical and horizontal positions.

The T-illusion proper may be reduced to several component factors, and it is not unlikely that different motives operate in different methods of judging.

(1) The single division of a straight line is one constant factor. It is well known that, while filled space is usually overestimated, there is a paradoxical exception[1] in the fact that a single interruption, such as a bisection, leads to underestimation. The cause of this is a small but rigid motive about which there are several well-known theories. (2) Contrast enters in that a short line is compared with a long line when, as is often the case, half of the whole line is compared with one end of the bisected line. This has been demonstrated in three forms, namely: (a) the double square, (b) the two sides of the double square in the shape of L, and (c) two plain horizontal lines, one twice as long as the other.[2] (3) Confusion of whole and half of the bisected line, impossible though it may seem, is probably the main motive for the illusion when it appears very strong, as in children or adults who lack power of keen discrimination. There is a sort of subconscious tendency to select a variable line that is shorter than the whole bisected line because there is a vague craving for comparison with the one-half of it.

Three observers engaged in a practice series taking a minimum of a half hour a day for twenty days in the most intensive form of practice under the given conditions. The days were

[1] Wundt, "Geometrische optische Täuschungen," 82.

[2] Seashore and Williams, "An Illusion of Length," Psychological Review, VII, 592.

nearly consecutive. Two of the observers were selected with the hope of obtaining naïve results and training without theory or knowledge of the records during the practice series. The third man was a psychologist familiar with the illusion.

The measurements were made by the method of selection. The T-figure was drawn on a series of fifteen white cards, 36 cm. square, in black ink with lines five-eighths of a millimeter in width. The bisected line was equal in all the figures, 114 mm. The other line was varied by five-millimeter steps from 79 mm. to 149 mm. The cards were so frequently changed as to prevent identification. These experiments were made in the fall of 1905.

The cards were exposed one at a time against a neutral background at a distance of one meter from the eye and at right angles to the line of regard. The observer was required to state at each exposure whether the undivided line seemed longer, equal to, or shorter than the bisected line. The experimenter followed a definite plan in presenting the cards, as follows: The experiment might begin with any card, but after that, the observer's reply determined which card should be presented next and the plan was so arranged that the cards were selected in a continuous zigzag crossing the region of equality. The procedure may be represented in the following scheme, in which the numbers denote the length of the variable lines and the letters denote the three respective judgments Longer, Shorter and Equal:

79	84	89	94	99	104	109	114	119	124	129	134	139	144	146
			S	S	E	E	L	L		106.5				
		S	S	E	E	E	L			104.0				

This method is very effective and enables one to work economically and without fear of bias or knowledge of the results.

Four series were made by placing the figures successively in each of the four cardinal directions. In speaking of the directions, we shall refer to the direction of the variable line as well as to the number of the positions, thus: (1) ⊥ vertical-up; (2) ⊣ horizontal-left; (3) ⊤ vertical-down; and (4) ⊢ horizontal-right.)

The practice was distributed equally among the four series and, on the average, about 130 judgments were made in each sitting.

Among the methods, as learned from the introspections, the following three were frequently used and will be referred to by numbers, as follows: (1) The judgment was based, upon the general impression of the figure as a whole without any separation into parts. (2) The standard or bisected line was superposed upon the variable or undivided line. (3) One end of the standard, or bisected, line was rotated through 90°, using the point of bisection as a center of rotation.

The results are represented in Tables V, VI and VII and are represented graphically in the corresponding curves, Figs. 3, 4 and 5.

Since the four positions of the figure naturally divide themselves into two pairs, the vertical and the horizontal, the results for the four sets are grouped into two sets, namely, 1–3 and 2–4. For our present purpose such combination offers no objection. The results are expressed in terms of percentages, based upon the standard, 114 mm. Each record shows the average per cent of illusion for the day, with the per cent of mean variation.

The records are the gross results, without the elimination of the illusion of the vertical or any analysis. They simply mean that, under a given condition, the variable line was selected so much too short, or too long as the case may be. The minus sign denotes that the variable was selected longer than the standard.

D. H. was a freshman student in the university. She was a careful and painstaking observer of more than average intelligence. She had been a pupil of the experimenter in the preparatory school. This made her feel at home with the experimenter and favored a natural and docile attitude without any feeling of restraint. She did not know that an illusion was being measured nor did she have any specific knowledge about illusions. She regarded the experiment as an opportunity for accurate sense training and expected to acquire skill.

As the aim was to determine what mere persistent effort, without information, would accomplish, no introspections could

TABLE V. (D. H.)

	⊥ and ⊤		⊣ and ⊢	
Day	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
1	22.8	3.1	11.1	3.2
2	25.3	2.1	8.9	2.1
3	22.7	1.4	7.5	2.4
4	21.5	2.7	6.4	2.0
5	19.7	0.4	7.8	1.6
6	22.7	1.6	9.9	1.7
7	22.3	1.5	10.4	1.5
8	21.9	2.4	8.9	2.2
9	22.5	2.4	3.6	2.0
10	18.8	1.3	7.7	1.7
11	20.3	1.9	7.9	1.4
12	21.3	1.9	4.7	2.5
13	22.3	1.2	7.1	1.7
14	22.6	2.3	6.8	2.2
15	22.9	1.6	6.8	2.1
16	23.6	1.1	4.4	1.6
17	23.0	1.2	4.7	1.0
18	23.2	1.3	5.7	1.3
19	24.5	1.6	5.8	0.9
20	26.3	0.0	4.9	1.2
Ave.	22.5	1.6	7.0	1.8

TABLE VI. (T. S.)

	⊥ and ⊤		⊣ and ⊢	
Day	*%Il.*	*%m.v.*	*%Il.*	*%m.v*
1	22.8	3.1	0.1	3.8
2	21.2	1.8	− 2.6	1.8
3	18.8	1.6	− 4.8	1.8
4	19.2	1.8	− 5.7	2.2
5	14.6	1.3	− 7.1	1.4
6	15.4	1.3	− 6.6	1.6
7	14.3	1.7	− 4.8	1.6
8	13.7	1.8	− 6.0	1.8
9	13.2	1.5	− 5.1	1.5
10	11.5	1.1	− 7.7	1.5
11	9.5	0.7	− 9.3	1.5
12	9.9	1.5	− 7.1	1.1
13	9.9	1.3	−10.1	2.2
14	9.9	0.7	−12.2	1.1
15	10.2	1.3	−11.5	1.8
16	10.2	1.3	− 9.3	1.5
17	11.5	0.8	−11.3	1.6
18	11.7	1.3	−15.0	1.3
19	13.3	1.3	−14.1	1.7
20	16.8	1.9	−14.4	1.7
Ave.	13.9	1.5	− 8.2	1.7

TABLE VII. (C. E. S.)

	⊥ and ⊤		⊣ and ⊢	
Day	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
1	8.5	1.8	− 6.0	2.7
2	6.6	2.2	− 9.6	2.1
3	5.8	1.6	− 7.6	1.9
4	1.1	1.6	−11.5	1.2
5	1.1	1.7	−10.8	1.7
6	− 0.4	2.3	− 9.3	1.3
7	1.7	2.0	− 7.3	1.7
8	6.2	1.9	− 6.0	1.3
9	5.3	1.1	− 6.4	1.6
10	7.1	1.2	− 4.2	1.2
11	8.0	1.0	− 4.6	1.8
12	7.1	1.2	− 7.9	1.1
13	4.7	0.6	−11.0	1.4
14	4.4	0.7	−10.8	1.9
15	3.8	2.0	−11.3	1.2
16	1.3	1.1	−14.1	1.0
17	1.1	1.5	−12.1	1.3
18	0.7	1.0	−14.8	1.1
19	0.9	0.9	−15.0	0.9
20	1.3	1.3	−13.9	1.3
Ave.	3.8	1.4	− 9.7	1.5

be asked for until the series had been completed and but little was volunteered.

Reference to Table V and Fig. 3 shows that for the vertical positions of the variable her average gross illusion on the first

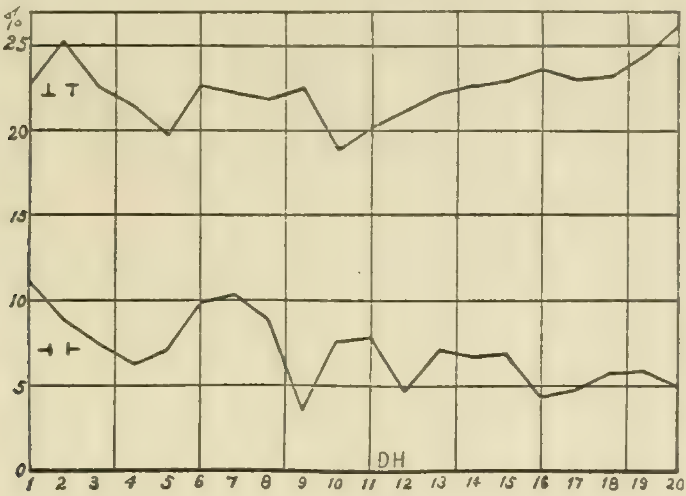


FIG. 3

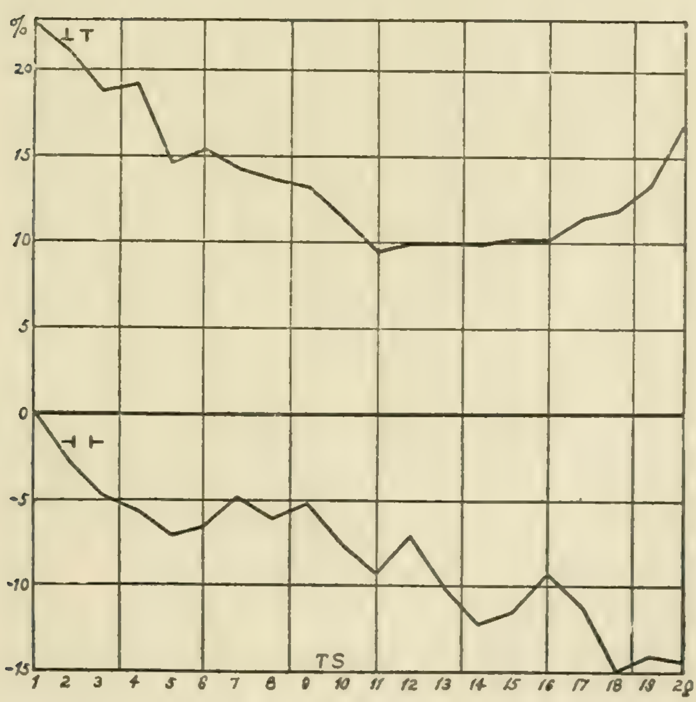


FIG. 4

day was 22.8% and, on the last day, 26.3%, there being a slight decline for the first ten days and a somewhat larger rise during

the second ten days. The average illusion is 22.5%, the minimum being 18.8% (tenth day) and the maximum 26.3% (last day). The table shows that the mean variation is small and regular.

For the horizontal position, the general average for the twenty days is 7%, the maximum, 11.0%, being on the first day and the minimum, 3.6%, on the ninth day. On the whole there is a slight general tendency in the direction of decline of the illusion. This decline but little more than offsets the increase shown for the vertical position.

To obtain a measure of her normal capacity in visual perception of space when no illusion was involved, she was tested

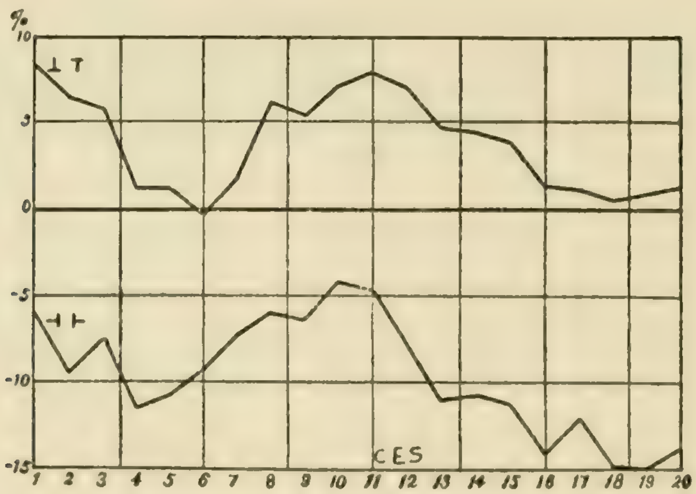


Fig. 5

on making one plain line equal to another, the two lines being in the same direction and end to end. With a line 114 mm. long, thirty trials resulted in an average error of 0.6% with a mean variation of 1.4%, which is a good record.

At the conclusion of the series, her illusion of the vertical was also measured with plain lines. Here the method of production was employed. The illusion of the vertical, when the variable line was in the horizontal position, averaged 16%; but, when the variable line was in the vertical position, she showed a strong tendency to correct for the illusion of the vertical. This tendency has been demonstrated before and is

due to the fact that the effort to adjust the vertical line makes the conditions for the illusion focal in consciousness, which is not the case in the other position. We may therefore take the record for the horizontal position as the truer record of her illusion of the vertical. This procedure is the more justified in view of the fact that the measurement was made after the training had been completed.

The attempt to eliminate the illusion of the vertical in cases like this is fraught with many dangers of error; there are a number of known, and doubtless also unknown, factors which should enter into our calculations, but we may make a gross elimination and bear in mind that the result is subject to minor corrections. Making the illusion, in round numbers, 15%, we find the data for the two positions self-consistent; for 22% minus 15% leaves a residual of 7% as the measure of the T-illusion in the vertical position; and, 15% minus 7% leaves a residual of 8%, which is the average record for the horizontal position. We must guard against being misled by the difference in the level of curves in the charts. There is danger of assuming from a glance at the chart that the T-illusion is greater in the vertical position than in the horizontal.

For this observer, then, the T-illusion amounts to about 7%; in one position of the figure, there is a small increase and in the other, an equal decrease in the gross illusion. Therefore we may say roughly that the illusion is comparatively small and is uninfluenced by training without knowledge.

Observer D. H., according to remarks during the experiment and careful examination afterward, maintained a naïve attitude throughout the whole training. She followed her natural intuitive tendencies and reported her impression of the figure at each exposure without knowledge of danger or serious suspicion of sources of error. She trusted her eyes. Indeed she used different methods and commented on the reasons for change but spoke of them merely as "difficulties," giving the impression that they spurred her on to use her eyes carefully. She thought that she was learning how to see the figure most effectively, her confidence in her judgments increased and she felt that she was improving with the practice

The data in hand would seem to justify the following interpretation of her case: She had a large illusion of the vertical which probably was not allowed for, or affected by the practice; she had a small T-illusion, probably due to the small and constant motives, which also remained practically constant throughout the practice. The practice resulted in a slight decrease in her mean variations for the daily records.

T. S. was a freshman, not exceptionally bright, but a faithful student who always secured good records in his class work. He knew nothing of psychology and approached the subject in the same naïve attitude as the foregoing observer, except that he was fully aware of the illusion of the vertical. He was asked not to make any allowance for this but simply to trust his eyes regardless of any theories he might have.

He started with a gross illusion of 22.8% which rapidly decreased to 9.5%, on the eleventh day, and then gradually increased to 16.8%, on the last day, making an average of 13.9%. In the horizontal position, his record starts with 0.1% and falls gradually to −14.4% on the last day, the average being −8.2%.

Tests for accuracy in space perception when no illusion is involved, as, e. g., in the above test on Observer D. H., show that in this respect he has greater ability than the average male university student. He made an average error of .2%.

Tests for the illusion of the vertical with plain lines, at the end of the series, revealed a strong illusion of the vertical—about 12%. As this record was obtained after the training series, it is safe to assume that the illusion of the vertical had not decreased much in the training. If we assume the conservative estimate of 10% as a constant illusion of the vertical, the T-illusion began with a force of 12.8% and fell off rapidly and steadily, during the first ten days, until it was eradicated; it remained absent for about six days and then gradually returned during the last four days.

Making the same allowance of 10% for the illusion of the vertical in the other position, we find that there is a very consistent parallel in the force of the T-illusion for the first sixteen days. During the last four days there is an end to this correspondence.

The general conclusion, then, would be that, this observer starts with a strong T-illusion which gradually passes away but returns with about half its original force in the vertical figure and is overcorrected at the same time in the horizontal figure.

Observer T. S. revealed by his remarks during the experiment and by examination after the experiment that he took an attitude of speculation and developed several theories which undoubtedly influenced him, although he tried to follow the instructions to make no allowance for any theory of sources of error. Thus, he learned by comparing different methods of judging the lines, that the bisection had the effect of shortening the line and he estimated this error to be possibly 20%. As a matter of fact, that motive does not amount to more than three or four per cent. This then, was a discovery of one of the true motives for the illusion, but it was overestimated and this overestimation led to the speedy decline in the record. Then, he developed a theory that the horizontal variable looks shorter than it really is, which is contrary to the T-illusion motive. He admitted a temptation to select a longer line on this theory, which would again tend to reduction of the illusion. This theory was, of course, based upon his knowledge of the illusion of the vertical and should be properly interpreted as resulting in a correction for the illusion of the vertical rather than an overcorrection for the T-illusion. This observer was aware that he got different results by different methods but the large changes in the curve are not due to any one method, because he changed methods frequently.

C. E. S. knew the details of the theory and the conditions of the experiment. He made an effort not to make any allowance for any of the known motives of illusion. From a former training series in the illusion of the vertical, he expected that to be about 6%. He expected the T-illusion to be considerably larger than that.

The gross illusion in the vertical position starts at 8.5% and falls rapidly, reaching zero on the sixth day; then it rises at about the same rate and reaches its original force on the eleventh day, and then falls again, reaching close to zero on the last five days.

The direction of the curve for the horizontal position is

fairly parallel to the curve for the vertical position. But the signs are minus, i. e., the horizontal line was made too long and the numbers in the low portions of the curve are too large to be expressions of the illusion of the vertical. This gives a clue to the interpretation of the large waves in both curves; namely, placing the normal illusion of the vertical constant at 6%, the drop below that number in either curve, regardless of sign, represents overcorrection for the T-illusion. This observer not only started with the T-illusion practically eliminated, but over-reacted against it unconsciously.

He represents the type of observer for whom the T-illusion is practically absent on account of training in accuracy of observation. But he started the training not knowing this, supposing that his records would show a decided T-illusion. It is well known that a conviction of that sort is almost sure to show itself subconsciously in some way. Here it resulted in periodic tendencies to overcorrect.

Although full notes are at hand, it is not easy to account in detail for these fluctuations. Of course, the observer did not know that there were any fluctuations until after the series was completed. From comparison of methods, chiefly the three mentioned, he concluded that the first (unanalyzed impression) would lead to the largest illusion, and the third (turning one half of the bisected line upon half of the other) was the most exact. In difficulties he used all three methods although he gave most weight to the third. But there is no traceable connection between the development of method and the notes on changes in method to correspond to the large waves in the curve.

After the nineteenth trial, he states:

"I cannot get myself to accept the third method 'straight' because it differs so much from the second. Still, at the present time, I approach the third method requirement more nearly than the second. I really believe that if I followed the third method rigidly there would be no error. I feel that there must be an illusion due to the bisection of the variable line which counteracts the T-illusion."

In the final introspection he says:

"I have continually struggled to remain in a naïve state of perception and to avoid making allowances. Thus, e. g., I have not figured out what the balance of all the illusions ought to be when the vertical line is in the horizontal position.

Still I have continually been conscious of a sort of allowance for the illusion of the vertical and the T-illusion, not in the sense of correcting fully for them, but in the sense of bearing in mind that there is a deceptive appearance. The judgment always impressed me as being very complicated and it has been difficult to keep the various factors constant. About the middle of the series I was particularly pleased with the 'Horizontal-left' figure. I felt that my judgment must be about right and thought that the two illusions probably cancelled. But the last few days I have grown more helpless and feel the greatest difficulty in this position."

An interesting index to this observer's orientation is obtained from a series of notes made from time to time saying which figures seemed to him to look exactly right and estimating what he thought might be right after corrections. On the fourth day he thought that the line which was -13% was probably the 'equal' figure in the vertical position. His record for that day was -11.5%. On the fifth day he selected the -13% figure again as the 'equal,' *to his eye*, but estimated that, owing to the presence of the illusions, it was probably actually -4%. On the fourteenth day he was sure that the -9% figure was the true 'equal' figure, after due allowances for the illusions. We see in all three of these cases that the observer was under the impression that he had not corrected for the motives of the T-illusion, the error of his estimates being 13% in the first case, 9% in the second case, and 9% in the third.

To bring together the conclusions obtained from this inspection of the three records, we may say: One observer had a medium T-illusion which remained unaffected by practice; another observer started with a strong T-illusion which rapidly passed away, but returned in part for one position and resulted in a decided overcorrection in the other; the third observer had no T-illusion at the beginning but periodically over-reacted against the motive.

A general review of these facts would lead us to conclude (1) that the T-illusion may be due to either or both of two types of motives: first, failure to take a discriminative attitude toward the figure and, second, the presence of such more rigid motives as those which condition contrast and the underestimation of a bisected line. Observer D. H. was probably influenced only by the second type of motives, Observer T. S. by both, and

Observer C. E. S. only by the second. But C. E. S. labored under an exaggerated estimate of the force of these motives of the second type. (2) Where the motives of the second type are not known, the T-illusion is not likely to be affected by practice. (3) Where the motives of the first type are present at the beginning, they rapidly disappear with practice. (4) Suspicion of a large motive for illusion leads to unconscious correction.

It is fairly certain then, also, that wherever the T-illusion appears in very great force on first trial, it will decline with practice whether the observer proceeds with or without knowledge in the practice. Or, perhaps more to the point, the molives of the first type disappear the moment the observer takes that serious discriminative attitude which he naturally assumes in entering upon a training series.

The difference between the results for the members of each pair of figures may be noted in passing. In the horizontal position the 'left' line is made approximately one per cent shorter than the 'right.' This is in accord with the known tendency for equal distances to appear greater when at the left than at the right. In the vertical position the difference is greater. The 'up' line is made fully three per cent shorter than the 'down.' This is also in accord with the known tendency to overestimate the lower portions of a figure. In both positions the point of union of the two lines is naturally taken as the center of gravity of the figure.

III THE MUELLER-LYER ILLUSION.

Measurements by Eva Crane Farnum.

The conventional double fledged Mueller-Lyer figure was used in this study of practice-effect. Two series of training were run parallel, one by the method of production and the other by the method of selection.

In the method of production, Judd's Mueller-Lyer apparatus as described in The Yale Studies, N. S., I, 68–9, was used. This apparatus enables the observer to vary one of the base

lines by both coarse and fine settings without otherwise disturbing the figure. The records are made automatically.

In the method of selection, the complete figure was drawn on a series of large cards, the base line in one section being varied by three-millimeter-steps in the series of cards. The mode of procedure was identical with that described for the T-illusion above. A fresh start was made after every two complete determinations, the object being to prevent the observer from inferring what relation the present judgment would hold to a foregoing one.

The constant base line was 100 mm. long, the angle lines 30 mm., and the angles of these with the base line 45° or its complement. The section with converging angle lines was kept constant and always at the left by both methods.

The two methods were used together in order to make the practice as free as possible from the contingencies of method. The production method is rapid but is always subject to dangers of accessory parts in the figure and disturbing tendencies in movement. The only real dangers in the method of selection lie in the possibility of suggestion by the order of the presentation of cards and the possibility of identifying cards. The latter danger was guarded against by continually changing cards, and the former was eliminated by the order of procedure mentioned above. It is absolutely essential, in a test of this sort, that the observer shall have no means of determining objectively or by inference what the actual proportions are.

A day's practice consisted in twenty settings by the method of production and twenty complete determinations by the special method of selection. As each complete determination by this latter method involved from four to seven separate judgments, it afforded the largest practice, about one hundred and thirty judgments a day. After the first day or two, all this could be accomplished in thirty or forty minutes, if there was no interruption or rest.

Four observers engaged in the training; two for twenty-four, one for twelve, and one for thirty-five days. These experiments were made in 1906.

The results are condensed into Tables VIII, IX, X, and XI,

which give the record for each method, with mean variation, for the successive days. The method of production is designated by *P* and the method of selection by *S*. The figures are averages for the day and are expressed in percentages.

TABLE VIII. (C. E. S.)

Date	S %Il.	S %m.v.	P %Il.	P %m.v.
Feb.				
23	9.1	1.1	12.1	1.1
24	10.0	1.0	10.5	1.4
26	10.7	1.5	8.7	1.5
27	10.0	0.6	8.9	1.3
28	8.7	0.9	7.1	2.0
Mar.				
1	7.3	1.1	5.6	1.2
2	5.7	0.3	5.1	1.0
3	4.5	0.9	3.2	1.2
5	3.2	1.1	2.9	1.8
6	3.1	0.7	2.6	1.4
7	3.5	0.7	4.1	1.5
8	3.4	0.8	3.4	1.2
9	2.0	0.9	3.2	1.1
10	1.4	1.0	0.5	1.2
12	1.4	1.2	− 0.7	1.2
13	1.2	0.7	− 1.4	1.4
14	0.4	0.5	− 1.1	1.0
15	1.2	1.1	− 1.1	1.1
16	− 1.4	1.0	− 2.9	1.6
17	0.6	0.9	− 3.8	1.2
19	0.1	0.4	− 2.1	1.1
20	0.1	0.5	− 4.5	1.2
21	0.5	0.8	− 3.6	1.4
22	0.4	1.2	− 3.5	1.7

TABLE IX. (D. S.)

Date	S %Il.	S %m.v.	P %Il.	P %m.v.
Feb.				
26	11.9	1.3	12.9	3.3
27	10.7	0.9	14.0	1.9
28	12.6	1.8	14.3	2.0
Mar.				
1	10.3	1.3	9.8	1.4
2	7.2	1.4	8.6	1.8
3	6.6	1.1	7.0	2.0
5	2.2	1.4	3.7	1.1
6	4.7	1.9	6.5	1.4
7	4.5	0.6	5.2	1.3
8	4.2	0.8	3.8	1.0
9	1.2	1.0	4.4	1.1
10	1.0	1.7	5.2	1.7
12	0.0	0.5	4.1	1.2
13	− 0.1	0.4	7.3	1.0
14	0.4	0.5	6.7	1.0
15	− 1.2	1.7	3.8	2.3
16	− 2.8	0.4	0.8	0.7
17	1.1	0.8	2.6	0.8
19	0.3	0.6	0.1	1.0
20	0.2	0.3	1.1	1.2
21	− 1.1	1.2	1.9	1.2
22	− 3.3	0.9	1.0	0.9
23	− 2.3	0.9	2.7	1.0
26	− 1.1	1.0	1.7	1.5
27	− 0.7	0.9	3.1	1.1
28	− 1.9	1.1	3.0	0.9
29	− 1.9	0.7	2.2	1.2
30	− 2.3	1.1	2.1	0.6
31	− 4.3	0.9	1.4	0.8
Apr.				
2	− 3.3	0.8	1.8	1.1
3	− 4.1	0.7	1.2	0.5
4	− 3.5	1.0	3.0	1.3
5	− 2.7	1.2	2.0	0.5
6	− 2.9	1.1	2.2	0.5
7	− 2.9	1.0	1.9	0.6

TABLE X. (T. P.)

	S		P	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Mar.				
9	15.7	2.1	21.5	2.8
10	18.6	1.2	16.8	0.9
12	20.4	1.1	16.5	0.8
13	19.5	1.0	19.2	0.5
15	21.3	1.4	19.7	0.5
16	24.7	1.2	20.4	0.8
17	21.9	1.2	20.1	0.4
19	21.9	1.0	21.9	0.7
20	22.0	1.1	21.0	0.6
21	22.0	1.2	21.7	0.8
22	22.8	1.0	21.3	0.7
23	22.0	0.9	21.8	0.7
24	21.7	0.9	21.7	0.7
27	21.5	1.2	20.9	0.4
28	21.0	0.4	20.8	0.4
29	20.1	1.1	20.5	0.5
30	19.6	1.1	20.9	0.9
31	19.5	0.9	18.4	0.8
Apr.				
2	19.9	1.0	19.8	0.6
3	19.6	1.0	19.7	0.4
4	19.3	1.0	19.9	0.6
5	19.2	1.1	19.9	0.6
6	19.3	1.0	19.6	0.8
7	19.0	1.0	19.7	0.7

TABLE XI. (J. A. M.)

	S		P	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Mar.				
28	25.5	1.3	26.7	1.5
29	22.4	1.2	26.6	2.8
30	14.5	2.3	26.0	2.2
Apr.				
2	11.7	1.4	24.2	1.7
3	10.4	1.4	19.0	1.5
5	6.2	1.9	19.0	1.7
7	5.7	1.7	16.8	1.7
9	3.2	0.2	17.2	1.6
10	2.1	0.2	15.1	3.0
11	2.9	0.2	16.1	1.8
12	2.9	0.1	11.9	2.9
13	3.4	0.4	11.9	2.1

A minus sign indicates that the illusion was reversed. The same results are represented graphically in the curves. Figs. 6, 7, 8, and 9.

C. E. S. who was familiar with all the details of the experiment, started with a fairly strong normal illusion and this was gradually eradicated by the practice. The illusion disappeared on the fifteenth day by the method of production, and on the nineteenth day by the method of selection. The records for the two methods run fairly parallel until the last one-third of the series, when a progressive deviation begins. The records for the method of selection remain on a level, near zero for the last third of the series of days; but the other record continues to fall and is increasingly negative from the fifteenth day on.

The result was astonishing to the observer when he saw it after the series had been completed. From many years of experience with this illusion, and from practice on other illusions, he had been led to the conviction that no improvement would take place without conscious change in the perceptual

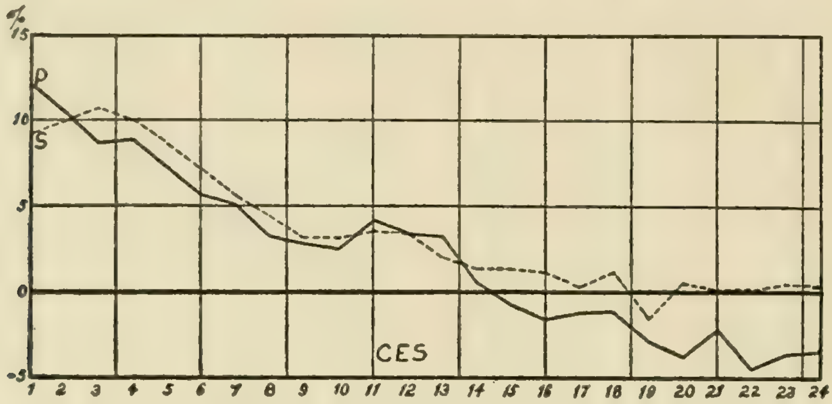


Fig. 6

attitude; and here he had remained in the same perceptual attitude toward the motives of the illusion, so far as it could be analyzed by introspection, from the second day to the end. He was under the impression that his normal illusion would amount to about 6 % and that it would remain constant throughout.

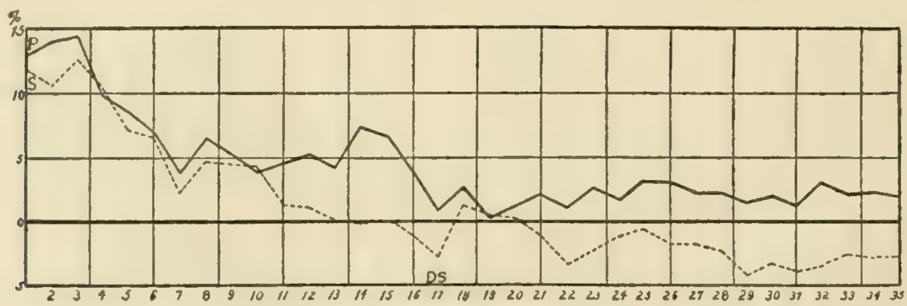


Fig. 7

From one point of view, this conviction was favorable to the reduction of the illusion for it kept the observer in that complacent attitude in which the perceptual process might adapt itself to the confronting difficulty without rousing consciousness of adaptation.

His method of judging was to fixate the middle joint and judge the two sections with the eyes at rest on this point, but

the act usually resulted in sweeping eye movements in both directions after the first fixation.

The difference between the *P* and the *S*-records in the latter part of the series was predicted by the observer during

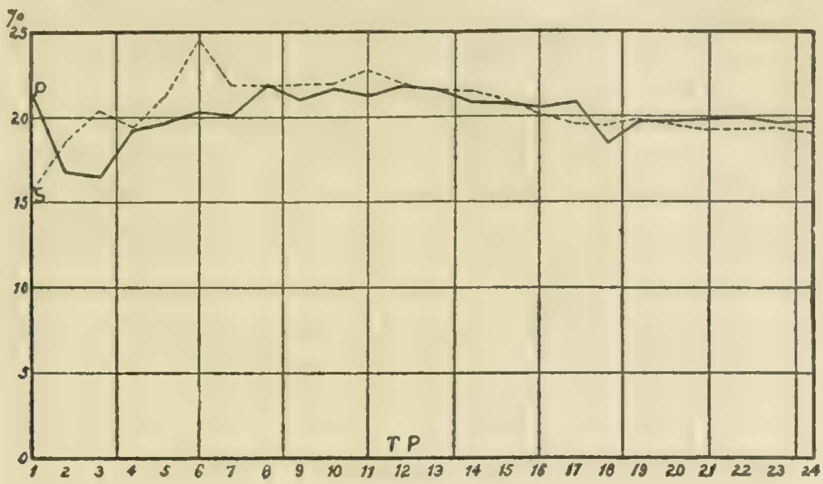


FIG. 8

those experiments. He recorded that the line of overlapping in the cards in the method of production was a source of confusion and he estimated that this might lead to the production

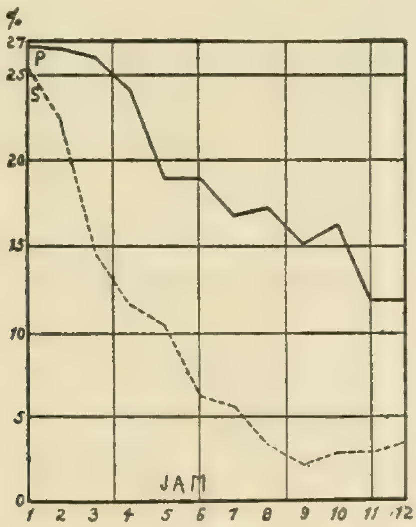


FIG. 9

of a longer line because a single interruption of a line leads to underestimation of that line. It is however difficult to see why the same principle should not have operated in the earlier part of the series when he was not aware of it as disturbing.

D. S. was also a trained observer thoroughly familiar with all the conditions of the experiment. So far as the effect of training is concerned, his records resemble those of the foregoing observer. The parallel is particularly close in the *S*-records. There is a large divergence in the results for the two methods, and it is in the opposite direction to that found for the former observer. Taking a mean between the two records, we may say that the illusion falls off gradually during the first half of the series and remains practically eliminated during the second half.

The main drop in this curve is explained by the introspective notes on method. For the first three days he recorded that he did not try to follow any particular method or contrivance but naturally took a glance at the figure as a whole and then sought a general impression as to the balance of the two parts to be compared. But on the fourth day he noticed that, "if I try to disregard the end-lines and center my attention upon the horizontal line, the left part has to be considerably longer than by the previous method in order to appear equal to the right." From the fourth day to the seventh, inclusive, he oscillated between these two methods but considered the latter the more satisfactory. On the eighth day, he fully adopted the second method and continued it to the end. During the transition period, he stated that the change of method must have resulted in a decrease in the illusion.

This record again illustrates the danger of constant errors, peculiar to the method of experimenting, entering into the record. Nothing in the introspections, in the critical review of conditions by the observer at the close of the experiment, or in the known conditions of the technique explains the divergence between the two records. We have no means of knowing which if either is the true one, further than the fact that the *P*-method is more complex and therefore more liable to error than the other.

The observer did not think that he had been influenced in his judgments by knowledge of the illusion. The force of the illusion is, then, accounted for in part by his method, or lack of method, for the first three days. The rapid decline following

is accounted for by the change of method. The further l wering is probably due to developed capacity for disregarding the accessory lines. The fact that the observer felt sure that the illusion had not been eliminated by the practice may have something to do with the overcorrection shown by the *S*-method.

T. P. was entirely naïve and uninformed so far as the psychology of illusions is concerned, and remained so throughout the experiment. He supposed that the accessory lines were there merely for the purpose of making the task of training more difficult, and never suspected the presence of any illusion. He was fully aware that he was to compare the base-lines only, but he was not aware that the accessory lines would influence his judgments. He therefore proceeded in the same way as a trained observer would in comparing two plain lines. It is easy to understand that it is no small achievement on the part of the experimenter to keep an observer so free from suggestions for twenty-four periods of experiment. This observer was a graduate student, with major in political science, extraordinarily faithful in his task, and confident of great good to result from his efforts.

He started with the strong illusion characteristic of those who are not aware of its existence and, although there are deflections in the curve, we may say in general that it remains at the level of about 20% without any tendency to change as the result of the practice. The mean variation is exceedingly small in comparison with the magnitude of the illusion, and the records by the two methods agree remarkably well.

A. M. was a graduate student in modern languages. His attitude may be expressed by quoting from notes made by him after the experiment had been completed. He says:

"I knew that 'things were not what they seemed,' before I commenced the experiments. But I had no idea as to what caused the illusion. I tried to estimate the length of the lines just as they appeared to me without making any allowance for any 'fake' I might imagine to exist. After a while the figures appeared different (?) without my making any allowance at all for the illusion. I did not attempt to figure out any system and did not think of the figures between experiments. The fact that I knew of the illusion may have unconsciously influenced my judgment, but I tried to guard against that."

The experiment was unfortunately interrupted, but it was carried far enough to reveal his type of reaction clearly. He started with the strong illusion of the uninformed but rapidly decreased this. The interpretation is undoubtedly to be found in the fact of a progressive adaptation of his mode of grasping the figure in a way favorable to isolation of the lines compared, but he made no effort to analyze the process. The difference between the results by the two methods is large.

Here again the different observers reveal different types of practice effect. In general the results may be summed up as follows: When the observer proceeds to the experiment with full knowledge of the conditions and does not expect the illusion to disappear, it does pass away, and without leaving any introspective evidence of the change; when the observer proceeds to the experiment without any knowledge of the illusion, and is not led to suspect any illusion, the force of the illusion remains unchanged throughout long continued practice; and, when the observer suspects the illusion but has no definite knowledge of its cause, it tends to disappear as in the cases of specific knowledge of it.

The difference between the results for the two methods of measurement employed shows the danger of ascribing to practice in general what may be due to peculiarity in method, and how practice may lead to improvement by one method of estimation and not by another.

Two years after the above training series had been completed C. E. S. and D. S. repeated the test to determine the effect of the long interval upon the practice gain. C. E. S., taking twelve complete determinations by the method of selection, revealed, an illusion of 12% with a mean variation of 1.4%. D. S., taking one hundred trials by the method of production, gave an average of 9%. The illusion had therefore returned to the approximately normal force that it had before the training. Unfortunately we have not yet had opportunity to repeat the training to determine to what extent the second training would profit by the first.

IV. THE ILLUSION OF DISTANCE BETWEEN CIRCLES.

Measurements by Raymond W. Sies.

The linear distance between two circles a moderate distance apart is overestimated. This is undoubtedly a form of the Mueller-Lyer illusion but it probably involves other motives than those ordinarily operating in the conventional form, as in the above experiments.

In the following experiments upon the effect of practice on this illusion the method of selection was employed as described in the section above, on the T-illusion. The standard figure consisted of two circles, each 114 mm. in diameter, the space between them being equal to the diameter of a circle. This figure was drawn on a series of cards, the inter-space being varied in successive cards by three-millimeter steps from 75 to 135 mm. The task was to select the card in which the distance between the two circles seemed to be equal to the diameter of a circle. Every effort was made to eliminate suggestion or information in regard to the illusion from the apparatus and method.

Three observers, representing as many different types of preparation, engaged in the test. Each observer made sixteen complete determinations each day for twenty successive days, Sundays excepted. This amounted to about one hundred judgments a day for each observer. The tests were equally distributed for the horizontal and the vertical positions.

Table XII shows the results for the three observers, giving the average illusion with its mean variation of the successive days for each of the three observers. Reversal of the illusion, i. e., underestimation of the distance between the circles is indicated by the minus sign. Fig. 10 represents these records graphically for the vertical position and Fig. 11 for the horizontal position.

C. E. S. is the trained observer who has taken part in the preceding experiments. His training on the illusion of the vertical, taken five years before, had resulted in no evidence of progressive gain from the practice. His training on the illusion of cylinder length had resulted in the same way. His train-

ing on the T-illusion had shown no appreciable progressive gain because the illusion was practically absent at the beginning of the training. The foregoing series of training on the Mueller-Lyer illusion was in progress and had reached the fourteenth day when the present series began, the observer of course being ignorant of what record he was making in the former until that

TABLE XII.

Day	Observer C. E. S.				Observer F. V.				Observer R. M.			
	Vertical		Horizontal		Vertical		Horizontal		Vertical		Horizontal	
	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.	%Il.	%m.v.
1	7.2	5.0	0.8	2.8	2.6	3.7	1.5	1.0	17.8	2.7	5.9	3.1
2	4.1	1.9	−1.7	0.4	2.6	3.7	1.1	0.6	18.9	1.5	7.1	4.3
3	3.8	1.6	0.2	2.2	−0.2	0.3	−1.3	1.8	14.5	6.0	6.6	3.8
4	3.2	1.0	0.4	2.4	−1.1	0.7	−2.0	2.5	16.1	4.3	2.8	.0
5	2.6	0.4	−0.8	1.2	−3.9	3.5	−3.9	4.5	13.5	6.9	3.6	0.8
6	3.2	1.0	−1.5	0.5	−1.1	0.7	−0.4	0.9	14.6	5.8	−0.7	3.5
7	1.7	0.5	−2.8	0.8	−2.1	1.7	−0.4	0.9	14.6	5.8	−1.8	4.6
8	2.6	0.4	−2.3	0.3	0.0	0.4	0.5	0.0	16.6	3.9	−1.7	4.5
9	3.3	1.1	−1.8	0.2	0.4	0.8	0.5	0.0	17.1	3.3	0.0	2.8
10	1.8	0.4	−1.5	0.5	−2.6	2.2	−0.8	1.3	21.2	0.8	0.8	2.0
11	1.8	0.4	−3.0	1.0	−0.5	0.1	1.5	1.0	23.9	3.4	2.3	0.5
12	1.8	0.4	−3.3	1.3	1.0	1.4	0.2	0.4	22.2	1.8	2.3	0.5
13	1.7	0.5	−3.9	1.9	−1.7	1.2	−0.8	1.3	21.1	0.6	1.1	1.7
14	3.0	0.8	−1.1	0.9	−1.0	0.5	−0.2	0.7	21.7	1.3	2.0	0.8
15	2.0	0.2	−2.8	0.8	−1.0	0.5	1.3	0.8	24.8	4.4	2.3	0.5
16	0.2	2.0	−3.0	1.0	0.5	1.0	3.0	2.5	25.7	5.2	3.9	1.1
17	1.1	1.1	−2.5	0.4	0.8	1.2	3.6	3.1	24.8	4.4	4.7	1.9
18	0.4	1.8	−3.8	1.8	0.2	0.6	1.8	1.3	25.4	4.9	4.1	1.3
19	−1.8	4.0	−2.6	0.6	−0.5	0.1	3.3	2.8	27.0	6.5	4.3	1.5
20	−0.5	2.7	−2.6	0.6	−0.5	0.1	2.3	1.8	27.6	7.2	5.9	3.1
Ave.	2.2	1.4	−2.0	1.1	−0.4	1.1	0.5	1.4	20.4	4.0	2.8	2.1

series was completed, which was on the twelfth day of the present series.

The outcome of the training in the first three types of illusion had led us to assume that the overlapping of these two series would not interfere. The records show that this assumption was wrong and unfortunate. C. E. S. supposed that the illu-

sion at the beginning of this series would be about 8% for the vertical position and about 4% for the horizontal position. This estimate was based upon knowledge of the average illusion for students in the laboratory as well as upon measurements

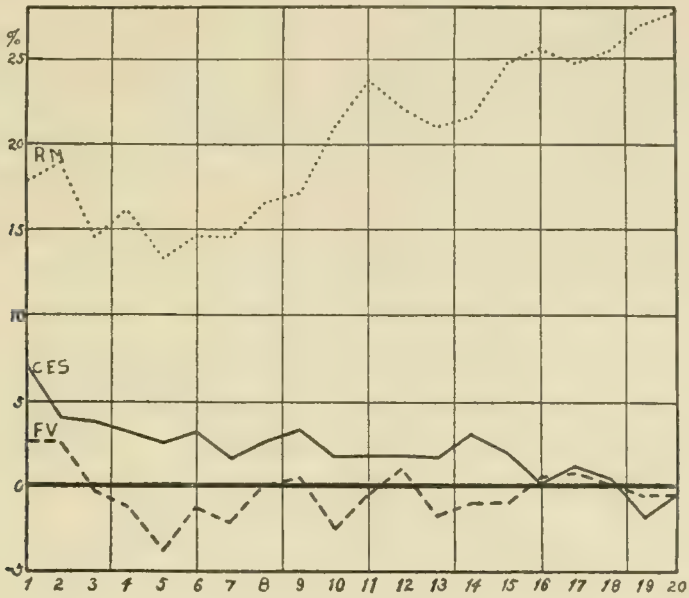


FIG. 10. Vertical.

made upon himself about seven years before. The results were therefore very surprising to him. They were doubly surprising, because after learning on the twelfth day of this series that the foregoing form the Mueller-Lyer illusion had tapered off with practice he had from that time supposed that the same process was going on for the present form of the illusion.

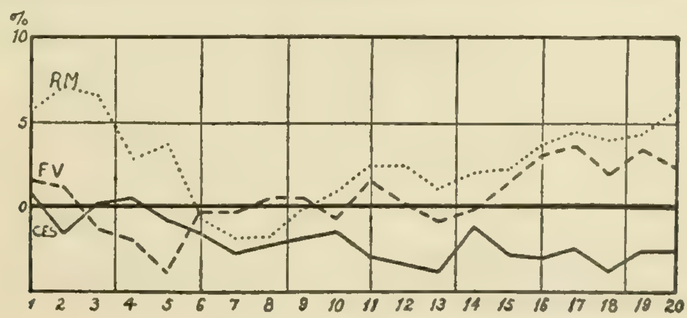


FIG. 11. Horizontal.

It is clear that there is a transference of practice-gain from the conventional form of the Mueller-Lyer illusion to this variant for the illusion is smaller at the beginning of this test than before the training in the former. The amount can not be stated as

the early measurements were made under different conditions. The observer's information of and surprise about, the outcome of the former series does not seem to have resulted in any objective evidence in the results after the day the information was obtained.

There is indeed a considerable illusion for the vertical position and this falls off rapidly during the first three days. The smallness of the error for the vertical position must not, however, be interpreted as evidence of efficiency only but also as the result of unconscious correction for the known motives of the illusion. This is demonstrated by the fact that there is a decided over-correction in the horizontal position. Although the small error in the vertical position remains fairly constant throughout, the series really ends with this also negative, which is further proof of the tendency to correct.

The following introspection was written by C. E. S. after the second day of training:

"The judgment on this figure is very uncertain. I notice three distinct methods of seeing the figure. (1) Seeing the whole figure in one sweep with an attempt to divide it into three parts without superposition of parts. (2) Comparing the diameter with the central space, allowing the limiting arcs of the circle to suggest bands of space about two inches wide, instead of a mere line. (3) Trying to image mere linear distances and superposing these. The third I think is the most effective but it is also the most difficult. The illusion seems to be greatest for the first, then the second is next, and the third is freest from illusion. Wherever the comparison is close I tend to use the third method.

"The circles appear distinctly oblong vertically. The lower circle seems to be larger than the upper. I usually use the lower. The right circle seems to be larger than the left. I tend to use the right.

"I had no basis for estimating the strength of the illusion at the start, if present at all. I am continually conscious of the direction of the illusion and should think that this would lead to a small illusion."

The second observer, F. V., was a sophomore of average intelligence, who knew of the illusion and some of the motives for it; but he was not a trained observer and it was impossible for him to make sharp distinction between what it appeared to be and what he estimated it to be with knowledge of conditions. He started with the illusion practically eliminated and made a very regular and consistent record with practically no illusion present in either vertical or horizontal position.

This record has but little value from the point of view of training. From measurements on variants of the illusion it was demonstrated that this observer was subject to the normal illusion when he was taken unawares. We should not conclude that he was dishonest, nor that he was a poor observer in other respects; but, having satisfied himself about the amount of the illusion before beginning the test, and knowing the tendencies present, he was not able to distinguish the logical estimate from the visual presentation.

The third observer, R. M., was also an undergraduate of strong ability, but he knew nothing of this illusion and remained in a naïve state of mind with reference to it throughout the test. The average illusion for undergraduates under the same conditions in a single test is 15% for the vertical position and 4% for the horizontal. His illusion is therefore 5 per cent above the normal in the vertical position and 1% below in the horizontale To be more specific, with reference to the vertical position he starts with an illusion which is about normal for observers of his class, this remains fairly constant for the first nine days, but in the last ten days, it increases 10%. For the horizontal position, he starts with an illusion above the normal, which falls off gradually to an overcorrection during the first eight days and then gradually returns so that the series of training ends with the same degree of illusion with which it began. The observer was greatly astonished at the results, but he could give no introspective account of changes which would account for these variations in his record.

On the surface the records of C. E. S. and F. V. are similar. C. E. S. undoubtedly reduced his illusion by training on the regular Mueller-Lyer figure but he also made unconscious correction and even over-correction. The first two or three days may also be interpreted as showing decrease with the training in this form. On the other hand F. V. did not have the advantage of practice, was not influenced by any foregoing training but by the knowledge of conditions which resulted in a confessed inability to take the strictly perceptual attitude. R. M. represents a type we have found in every series before—the person who does not know of the illusion and who, as a result, shows

a strong illusion which does not tend to disappear or diminish with practice.

GENERAL CONCLUSIONS.

The essential feature of these experiments lies in the demonstration of a number of factors which determine what effect practice shall have upon these normal illusions. Chief among these are the degree and the kind of knowledge of the illusion, the capacity for maintaining the perceptual attitude, speculative tendencies, the estimate placed upon the known illusion at the beginning of the training, the duration of the training, knowledge of progress, the effect of a long interval after training, and different types of motives for illusion.

The list of the cases in Table XIII is arranged as a partial aid in a review of the results. The Roman numerals refer to the series: I, the illusion of cylinder length; II, the T-illusion; III, the Mueller-Lyer illusion; and IV, the illusion of distance between circles. The observers are designated by their initials; v. and h. denote the vertical and the horizontal positions respectively; s. and p. designate the method of selection and the method of production respectively. 'Strength,' has reference to the strength of the illusion at the beginning of the training; 'Knowledge' has reference to the observer's knowledge of the existence and character of the illusion involved.

The illusion persists with undiminished force so long as the observer has no knowledge of its existence. This is illustrated by Cases I, O. W.; II, D. H.; and III, T. P.—as it was illustrated first in the study on the size-weight illusion and the illusion of the vertical mentioned in the introduction. We have found no exception to this rule.

Among observers who have knowledge of the illusion those who are best capable of maintaining the perceptual attitude (i. e., reporting what they actually perceive as opposed to what they may judge relations to be) are least likely to decrease the illusion by practice. This assertion is based largely upon the introspections and the internal evidence in those cases in which the observers had knowledge. The surest way of aiding the observer in maintaining the perceptual attitude is to find and

keep him ignorant of the existence of the illusion and free from suspicion of it. That was an easy task fifteen years ago, but is exceedingly difficult now in the face of popular knowledge of the illusion.

Of course, if one knows the illusion he can readily learn to make proper correction for it in a judgment. Such correction

TABLE XIII

Case		*Strength*	*Knowledge*	*Practice Effect*
I, C. E. S.,	v.	Small	Full	Not any
	h.	"	"	" "
I, L. F. S.,	v.	Not any	Partial	Overcorrection
	h.	Medium	"	Increase-decrease
I, J. O. D.,	v.	Medium	Partial	Not any
	h.	"	"	" "
I, O. W.,	v.	Very large	Not any	Great increase
	h.	" "	" "	Increase
II, D. H.,	v.	Small	Not any	Slight increase
	h.	"	" "	Slight decrease
II, T. S.,	v.	Medium	Partial, suspicion	Decrease-increase
	h.	Small	" "	Overcorrection
II, C. E. S.,	v.	Very small	Full	"
	h.	Not any	"	"
III, C. E. S.,	s.	Medium	Full	Complete reduction
	p.	"	"	Overcorrection
III, D. S.,	s.	Medium	Full	"
	p.	"	"	Decrease
III, T. P.,	s.	Large	Not any	Not any
	p.	"	" "	" "
III, A. M.,	s.	Large	Partial, suspicion	Decrease
	p.	"	" "	"
IV, C. E. S.,	v.	Small	Full	"
	h.	Not any	"	Overcorrection
IV, F. V.,	v.	Not any	Partial, speculative	Not any
	h.	" "	" "	" "
IV, R. N.,	v.	Large	Not any	Increase
	h.	Small	" "	Decrease-increase

is at first focal in consciousness but soon becomes so automatic that the closest introspection may not trace the correction process involved in the form of an allowance for the illusion. To prove that we can make conscious correction for the illusion would be a waste of energy; to assume that such correction could not be made in a normal individual would be absurd. We

the early measurements were made under different conditions. The observer's information of and surprise about, the outcome of the former series does not seem to have resulted in any objective evidence in the results after the day the information was obtained.

There is indeed a considerable illusion for the vertical position and this falls off rapidly during the first three days. The smallness of the error for the vertical position must not, however, be interpreted as evidence of efficiency only but also as the result of unconscious correction for the known motives of the illusion. This is demonstrated by the fact that there is a decided over-correction in the horizontal position. Although the small error in the vertical position remains fairly constant throughout, the series really ends with this also negative, which is further proof of the tendency to correct.

The following introspection was written by C. E. S. after the second day of training:

"The judgment on this figure is very uncertain. I notice three distinct methods of seeing the figure. (1) Seeing the whole figure in one sweep with an attempt to divide it into three parts without superposition of parts. (2) Comparing the diameter with the central space, allowing the limiting arcs of the circle to suggest bands of space about two inches wide, instead of a mere line. (3) Trying to image mere linear distances and superposing these. The third I think is the most effective but it is also the most difficult. The illusion seems to be greatest for the first, then the second is next, and the third is freest from illusion. Wherever the comparison is close I tend to use the third method.

"The circles appear distinctly oblong vertically. The lower circle seems to be larger than the upper. I usually use the lower. The right circle seems to be larger than the left. I tend to use the right.

"I had no basis for estimating the strength of the illusion at the start, if present at all. I am continually conscious of the direction of the illusion and should think that this would lead to a small illusion."

The second observer, F. V., was a sophomore of average intelligence, who knew of the illusion and some of the motives for it; but he was not a trained observer and it was impossible for him to make sharp distinction between what it appeared to be and what he estimated it to be with knowledge of conditions. He started with the illusion practically eliminated and made a very regular and consistent record with practically no illusion present in either vertical or horizontal position.

This record has but little value from the point of view of training. From measurements on variants of the illusion it was demonstrated that this observer was subject to the normal illusion when he was taken unawares. We should not conclude that he was dishonest, nor that he was a poor observer in other respects; but, having satisfied himself about the amount of the illusion before beginning the test, and knowing the tendencies present, he was not able to distinguish the logical estimate from the visual presentation.

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	p.	"	"	Decrease
III, T. P.,	s.	Large	Not any	Not any
	p.	"	" "	" "
III, A. M.,	s.	Large	Partial, suspicion	Decrease
	p.	"	" "	"
IV, C. E. S.,	v.	Small	Full	"
	h.	Not any	"	Overcorrection
IV, F. V.,	v.	Not any	Partial, speculative	Not any
	h.	" "	" "	" "
IV, R. N.,	v.	Large	Not any	Increase
	h.	Small	" "	Decrease-increase

is at first focal in consciousness but soon becomes so automatic that the closest introspection may not trace the correction process involved in the form of an allowance for the illusion. To prove that we can make conscious correction for the illusion would be a waste of energy; to assume that such correction could not be made in a normal individual would be absurd. We

are therefore interested only in the effect of practice upon actual perception, or what seems to be perception; but the judgment process shades so imperceptibly into the perception process that the task of distinguishing them becomes exceedingly difficult.

Some illusions may be eradicated with practice without leaving any conscious trace of the correction. See, e. g., I, L. F. S. v; III, C. E. S.; III, D. S. These are in accord with Judd's results on the Mueller-Lyer illusion and Cameron and Steele's[1] results with the Poggendorf illusion. The figure looks different at the end from what it did at the beginning of the series. But, so far as the records go, such reduction has taken place only for persons who know of the illusion. From this it is not necessary to assume that the decrease is due to conscious correction; it may be due to the ability to avoid a certain kind of eye movements, attention to accessories, etc.

The illusion may disappear at the very beginning of a training series merely as a result of the elaborate and discriminative adjustment for systematic observation, and without any practice. For all who know of the illusion, there is a tendency to begin the training series with a smaller illusion than would ordinarily be shown in a single test. The cases of small, or not any, illusion in the above list illustrate this.

When the illusion decreases as the result of practice, without the appearance of any change in attitude of the observer, the gain takes the form of the conventional curve of learning; the illusion tapers off gradually during one or two thousand trials, with normal fluctuations.

The gain made by training in a series like this is not retained permanently; it may be wholly or partly lost in two years.

Overestimation of the illusion which is supposed to be involved at the beginning of a training series tends to lead to correction. This is one of the clearest evidences of the failure to maintain the perceptual attitude. This is illustrated in I, L. F S. v; II, C. E. S.; and II, T. S., h.

[1] Cameron and Steele; "The Poggendorff Illusion," Yale Psychological Studies, N. S., I, 83.

In partial knowledge of a given illusion, those motives which are known are affected as in full knowledge of the illusion whereas those which are not known remain unaffected by the practice. See Cases I, L. F. S., h; I, J. O. D.; III, A. M.

Suspicion of the illusion is likely to result in erratic records, depending in part upon the rightness or wrongness and in part upon vacillation in the suspicion. Where the suspicion is specific it operates in the same way as knowledge. The increase in an already large illusion may be accounted for by the presence of an erroneous suspicion; e. g., I, O. W., v; and IV, R. M. Case II, T. S., may illustrate a vacillating suspicion with reference to the vertical position and a true and firm suspicion with reference to the horizontal.

Where the same motives are involved, the gain made by training on one variant is transferred to another variant while the results of the training are fresh. See the effect of Series III upon Series IV in the case of C. E. S.

In all these experiments the observers were kept completely ignorant of their records until the training had been completed. With progressive knowledge of one's records the results would undoubtedly be quite different. It would probably be impossible to maintain the perceptual attitude under such circumstances. The gain would, of course, be more general and more rapid.

The fact that observers who are working extensively with illusions tend to have comparatively small illusions would seem to show that the discriminative attitude of the trained observer, regardless of practice in any particular illusion, weakens some motives for illusion and wholly obliterates others. In many illusions trained observers may choose to regard the object in such a way as to obtain a strong illusion, a small illusion, or no illusion at all, at will.

There are undoubtedly two general types of motives for illusion: those which are due to lack of discriminative apperception of the task in hand, as in the large illusion in the T-figure; and those which are due to deeply ingrained misleading tendencies in the interpretation of sensory data, as in the illusion of the vertical. Such complex illusions as the Mueller-Lyer, the size-

weight, the cylinder length, and the T-illusion probably involve both. The force of the rigid sensory motives is also lessened by a keen discriminative attitude of the observer.

The practical value of experiments of this sort is evident. In these training series we have followed the making and unmaking of habits under conditions partly controlled and have traced the dominance and the vanishing of ideas in developing perception. We feel that the possibility of working out a serviceable system of laws governing the persistence and the elimination of normal illusions which enter into our ordinary sensory experience is as promising as it is urgently needed.

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THE TONOSCOPE

BY

CARL E. SEASHORE

In experimental psychology instruments have as a rule been designed to meet immediate needs, and have usually been described incidentally in reporting the results of psychological investigations. Much research has been wasted because done with untried apparatus. In fact most of our instruments are in a crude condition; and many fields of investigation lie untouched for want of measuring instruments. It is a sign of a higher stage in the science that the most essential psychological instruments are now being subjected to investigation apart from the specific pending psychological use. Only in this way can we properly develop instruments and standardize the technique of manipulation. I therefore take pleasure in presenting the description of an instrument and its use, as an object worth while in itself.

The tonoscope, Fig. 1, works on the principle of stroboscopic vision, the principle of moving pictures. Auditory vibrations of air, caused by voice or musical instrument, are converted directly and instantaneously into visual configurations on a screen, and the vibration frequency which denotes the pitch of the tone may be seen in plain figures on a scale. This enables us to measure the pitch of any tone by direct inspection while singing, speaking, or playing under normal conditions. The ability to do this opens up countless problems in the psychology of tonal expression.

There is a contrivance by which the vibration of the voice mechanically raises and lowers a flame for each sound wave. The oscillation of the flame results in corresponding exposures on the screen which it illuminates. The vibration being rapid, the retinal lag produces the effect of continuous vision, although the duration of the illumination for each vibration is very short in comparison with the corresponding period of non-illumination. In moving pictures it is well known that, if we have successive pictures which are alike thrown on the screen in the same place and in rapid succession they form one continuous picture which stands out clear and still. This is the principle here employed. The revolving screen,

rotating at the rate of one revolution per second, carries rows of dots, regularly spaced but varying in number for each row. When a tone is sounded, the row which has the dot-frequency that corresponds to the vibration-frequency of the tone will stand still and be clear while all other dots move and tend to blur. Each row runs under a number on the scale. The row which stands still, therefore, points to a number which designates the pitch of the tone. The screen contains a sufficient number of rows of dots, varying in number, to correspond directly, or by multiple, to all tones within the range of the voice. To see the pitch of the tone one has therefore only to see the number of the line that stands still.

Earlier models of the tonoscope have been described in a previous volume of these Studies (1); also in the Musician (2). Such radical changes have, however, been made since then that we are now dealing with an instrument very much modified and extended in its usefulness.[1] The present instrument is not the result of the work of one man but of many of whom, aside from those who have developed the principle of stroboscopic vision in physics, I desire to mention particularly Dr. E. W. Scripture who designed the first laboratory exercise using this principle in psychology (3); Dr. C. F. Lorenz (4), to whose ingenuity and most generous coöperation we owe the synchronous motor and the plan of using the selenium cell with the siren; Mr. E. W. Bechly, and Professor E. A. Jenner, who made the first tests with the tonoscope in determining its value for use in the musical conservatory (5); and Dr. Walter R. Miles (6), who has standardized procedure for various problems in the measurement of singers with this instrument.

Instead of giving merely a description of the commercial form of the instrument, I shall attempt to suggest, in a semi-technical way, its various possible forms on the basis of actual experiment from the laboratory point of view. The essential features which must be discussed in turn in the description are the speed regulation, the screen, the dot grouping, the sensitive light and sound transmitter, and the siren.

The synchronous motor.—The validity of stroboscopic frequency measurements depends upon the accuracy with which the movement

[1] Fig. 2, showing the 1902 model, is here reproduced because it shows the bare elements of construction better than they can be seen in the present encased model, as shown in Fig. 1.

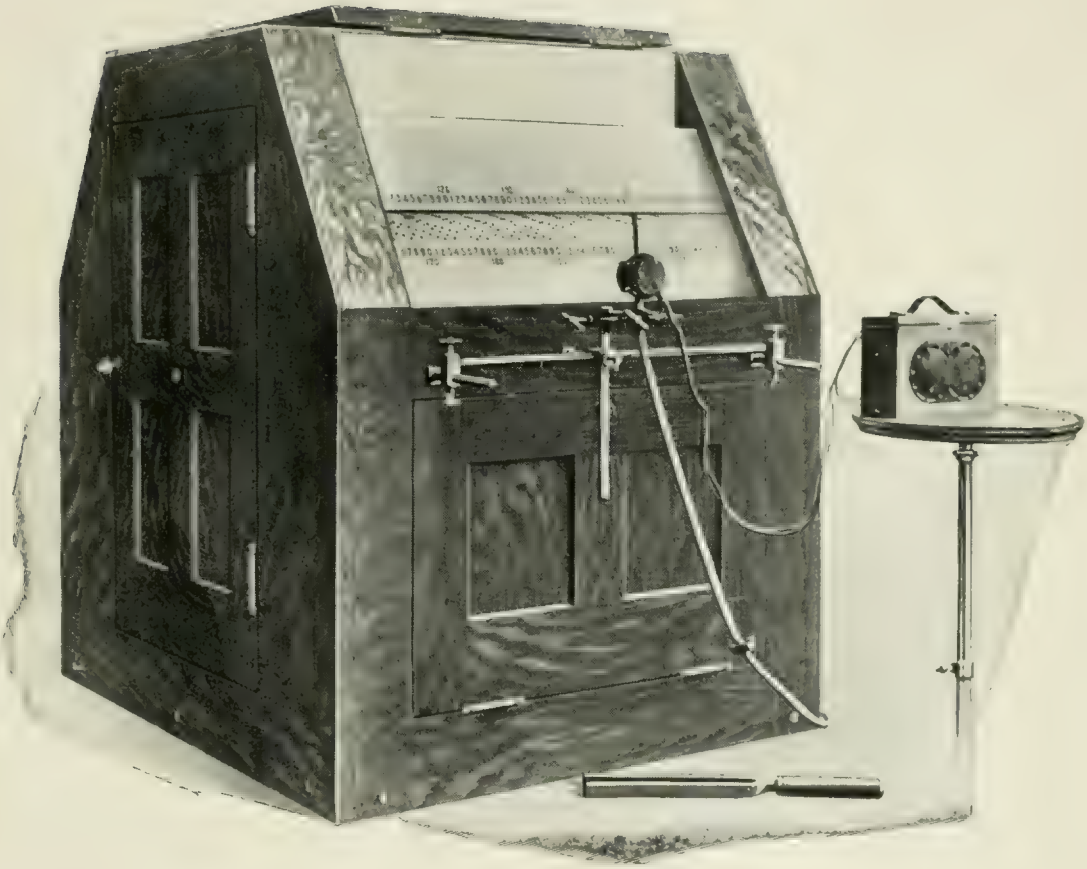

FIG. 1. THE TONOSCOPE

(The instrument seen at the right is the acousticon.)

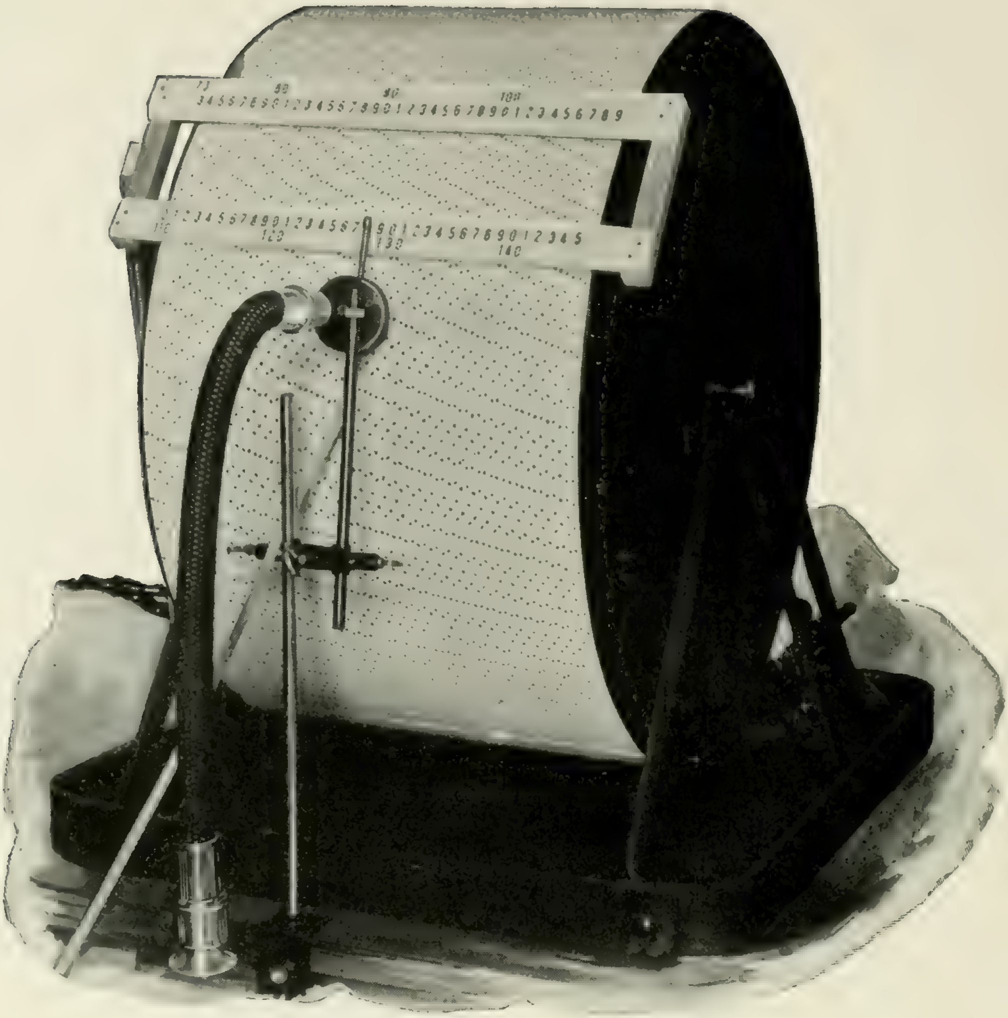

Fig. 2. Early Model of Tonoscope. (1902)

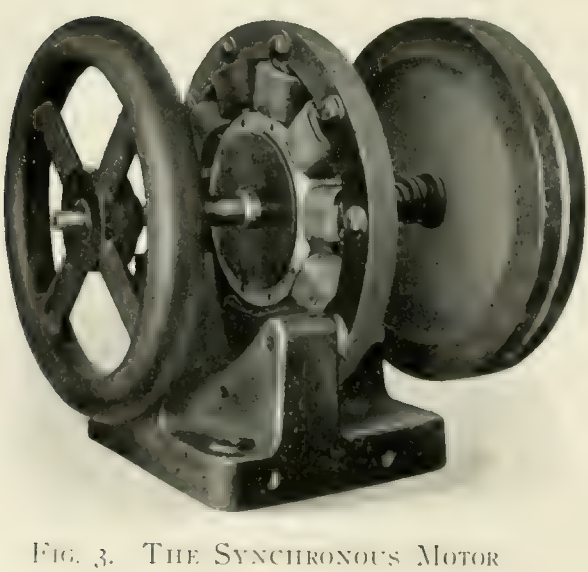

Fig. 3. The Synchronous Motor

of the exposed object is controlled. The method employed in the early models proved entirely too laborious and inconvenient.[2] The final solution was found in the use of a synchronous motor which drives the drum (screen) at a regulated speed. This motor (Fig. 3) works on the principle that, if a regularly interrupted current be sent through a multipolar field, and the needed initial momentum be given to the revolving multipolar armature, each closing of the circuit will synchronize with poles always in a corresponding position of approach, and the momentary pull will be sufficient to continue the rotation until the next pull occurs, at the next approach of poles. This is the principle of La Cour's "phonic wheel" (7), a principle also employed by Lord Rayleigh (8).

A motor of this type is mounted on the main shaft of the tonoscope drum.[3] The drum, serving as a balance wheel and being connected to the motor by a coil spring, furnishes the right degree of inertia and flexibility in the transfer of the pull.[4]

A large 10 v.d. tuning fork is used as an interrupter. It is energized by primary cells, and is encased in a box which is kept out of the way in a closet so that no noise shall come from it. A 16 c.p. lamp used for resistance in the motor circuit, is mounted

[2] In the early models the drum was driven by an ordinary direct current motor. An assistant at the back side of the drum observed and recorded the actual speed at the time of every reading. This was done by means of the stroboscopic effect of the intermittent light in a vacuum tube in circuit with a standard tuning fork. Every reading had to be adjusted with reference to an elaborate table of corrections calculated for different steps of variation from the true speed.

[3] In the early stages the motor was placed outside of the drum and the power was transferred by belt. This was neither as convenient nor as reliable as the present method, but it left the motor available for other laboratory purposes.

There is a great demand for such a synchronous motor in laboratories because, with constant speed and power, many of the laboratory problems are readily solved and we gain a higher degree of accuracy than can be obtained by any other form of electric motor or by kymograph clockworks. As the speed may be transformed up or down, this motor will take the place of many cruder devices in the physics and psychology laboratories for securing constant motion, marking intervals, measuring time, making exposures, etc.

[4] In the type of motor here needed this principle has not before been successfully developed for practical use. The hitch lies in the jerky nature of the pull. By attaching a flywheel to the axle by a flexible connector we get a flexible moment of inertia which solves the problem.

between the prongs of the fork and proves a convenient means of keeping it at a sufficiently constant temperature, the temperature being that to which the fork is raised by the heat of the lamp within the box.[5]

A 110 volt direct current is completed through the motor and a mercury contact interrupted by the fork. The current is reduced by the lamp resistance. The make-and-break is short-circuited with a condenser to avoid forming of an arc. A large amplitude of the fork, fully 10 mm., also helps in preventing the tendency to arc.

A rheostat inside the tonoscope case, with a switch on the surface, serves for the adjustment of current, as there may be fluctuations in the supply main. A small detachable crank for starting fits the end of the main shaft which comes out flush with the edge of the case on the side. To start the tonoscope one has only to start the fork, give the drum a turn up to approximately one revolution per second and close the switch. Once started, the instrument will run indefinitely and there is no care or distraction in the running of it.

The screen.—The stroboscopic screen is formed by mounting a sheet of aluminum in the shape of a drum over a heavy balance wheel. A section of this drum is seen through the opening on the front of the case (Fig. 1). This screen is 50 cm. wide and has a circumference of 242 cm. The balance wheel is heavily mounted on ball-bearings resting on a heavy iron frame. The whole instrument is enclosed in an oak case with doors on every facet.

The size of the drum is determined by the minimum area for the legible distribution of 18,500 markings, or stroboscopic dots. In the present screen the dots are bored holes, three and one-half mm. in diameter. The inside of the drum being dark, the holes show up clearly as black spots on the light aluminum surface. These holes are spaced with the highest mechanical accuracy and are arranged in 110 parallel rows, each completing the circumference of the drum in uniform spacings for each row (Cf. arrangement of dots in screen in Fig. 2). One row has 110 dots and the dot frequency in the remaining rows increases by one dot for each row up to and including 219. Thus we get frequencies to correspond

[5] Like the motor, this fork becomes a sort of "universal" apparatus in the laboratory. Being standardized and always connected up, it becomes the most convenient means for timing purposes. It is notably serviceable in connection with a multiple recorder which makes impressions on ticker tape.

to each intergral vibration-frequency in an octave of tones, the octave of 110 v.d. to 220 v.d. This is approximately the octave from *A* up to the *a* below middle c̍

This octave was chosen after much experimenting as being the most serviceable, all factors taken into consideration. Within this octave the tones are read directly, and above and below it they are read by multiples. The number of holes in each row is shown in plain large figures on the scale. When the drum revolves the row formation stands out clearly and each row points to a number.

The "framing-effect".—As may be seen in Fig. 1, there is an upper and lower scale, one on each edge of the shield. It is necessary that the holes should be large enough to be easily legible under the prevailing conditions of fusion, and also that they shall be widely enough spaced in both directions to be easily read. It is also essential that a single little sensitive flame shall light up the whole exposed surface of the screen. This forces upon us the difficult problem of securing compactness. In the early models no other solution was seen than to restrict the instrument to a part of an octave, but a unique solution was finally found. This consists in alternating rows of widely different frequency as may be seen by observing the actual numbers of the adjacent lines on the screen in reading alternately on the upper and lower scale. The numbers on the upper scale are consecutive from 110 to 164, and the numbers on the lower scale from 165 to 219, the rows reading on one scale alternating with those reading on the other. To illustrate, if rows 150 and 151 were adjacent they would need to be separated by a wide space in order to be differentiated, because their movements are so nearly alike, but if they are separated by another row, for example the 205, the differentiation between the two original rows becomes clearer and the spaces between them may be materially reduced, for, when rows 150 and 151 stand approximately still and their individual dots stand out clear and distinct, row 205 moves so swiftly that it forms one continuous line or gray streak which has a most serviceable separating or framing effect for the adjacent rows. When the rows are arranged as shown on the scale, this differentiating, or framing effect will operate for each and every row that may be standing out for reading. This contrivance makes it possible to reduce the screen to about one-third of the size otherwise required, and still makes the reading more legible than it would

have been on a screen three times as large without some contrivance like this.

The sensitive light and sound transmitter.—A fundamental requirement in this principle of measurement is that the light shall be made intermittent through the action of sound waves. This may be accomplished in various ways. In the simplest arrangement an ordinary manometric capsule is used and the singer holds a funnel before his mouth in such a way as to effectively collect the vibrations. Acetylene gas supplied by a motorcycle tank is used for this sensitive flame. We have not yet determined the most effective form of capsule or the maximum upper and lower limit of its vibration response, but have found that this varies with numerous conditions, such as the vibration frequency, the volume, the smoothness, etc. of the tone. This capsule may be used in recording from such musical instruments as send a fairly concentrated volume of waves in one direction, such as tuning forks, wind instruments, reed instruments, and the siren. With all these it is, however, advantageous to use a Helmholtz or a Koenig resonator as a selector although it is not necessary in all cases. As a rule the shorter the speaking tube or horn, the less danger there is of interference in the sound waves.

While this mechanical transmission through a manometric capsule is for most purposes the simplest means, and is entirely satisfactory, especially in singing, we have electrical devices that have distinct advantages. The receiver of a microphone may be converted into a manometric capsule by building a gas chamber on the ear side and supplying it with a gas inlet and a jet nipple. The vibration of the receiver membrane controls the gas flame in the same way as in an ordinary capsule. The microphone transmitter is used with this as in ordinary speaking. The best type of commercial instrument that may be readily adapted for this purpose is the phonette or the acousticon made by the General Acoustic Company, New York. The acousticon known as type D seems to be the most serviceable.

While the electrical apparatus may be a little more delicate to handle it has the advantage that it is more sensitive and can be used for the recording of a tone which would not be strong enough to register in any other way. It also makes it possible to set this apparatus in front of the singer so that he may sing for a record

without being aware that a recording instrument is present in the room. The singer may be isolated in a quiet room or in familiar surroundings, in order not to be disturbed by the presence of another person and the main instrument. The measurement may even be made at any long distance covered by telephone connection, as all that is necessary is to put the microphone transmitter in front of the singer at one end of the telephone line, and connect it with the microphone receiver on the tonoscope at the other end.

When sound vibrations are strong enough completely to make and break the circuit, the ordinary telephone receiver may be used as a capsule in the manner just described. On certain musical instruments a mechanical interrupter resting on the resonating chamber of the instrument, for example, a violin may be used.

Under the same circumstances a vacuum tube may be used in place of the gas-flame capsule. The intermittent light is then caused by the interruption of the current in the primary circuit of an induction coil which has the vacuum tube in the secondary circuit.

If two simultaneous records are desired, one record may be taken on each side of the tonoscope. Indeed, four records may be taken simultaneously by using both the upper and the lower facets on the back and the face of thé drum, there being doors on the case for this purpose.

The stroboscopic reading requires fairly complete darkness. To avoid darkening the room a hood (not shown in the figure) has been built to fit over the reading surface of the tonoscope. This hood forms a dark chamber and the inner surface, being bright, serves as an excellent reflector for the light. For intensive reading at a given point on the scale, a small sliding hood is made on the same principle. It has the advantage of centering the light upon the point of reading in the scale. A reflecting mirror (not shown in the figure) is used to distribute the light over the visible portion of the screen for ordinary use.

The siren.—For certain purposes it may be desirable to get a key-note or a standard pitch from the tonoscope itself. The dots on the screen were therefore made as holes. At the base of the front of the case is a siren blow-pipe supported on a horizontal carrier so that it may glide freely over the surface of the drum, while a pointer indicates on the scale just what hole-frequency is blown. This makes it possible to produce as siren tones all the tones from 110 to 219 by one-vibration steps, excepting those rows of holes

which happen to be closed by the contact with the balance wheel. The siren blow-pipe is connected with a compressed air tank or it may be blown directly by a mouth tube. A speaking tube is used to carry the sound to the observer's ear and the opening and closing of this tube by means of a clamp starts and stops the sound.

The siren tone is not a tone of good quality. But a beautiful tone may be produced by projecting a beam of light through the holes in the screen upon a selenium cell in circuit with a telephone receiver. It so happens that the fluctuation in the resistance of the selenium cell takes approximately the form of a sine curve, and that produces a tone of most remarkably clear and smooth timbre in the receiver. One may, however, use any sort of instrument for giving the standard tone, as the pitch of the instrument can be read off on the tonoscope at any moment.

The reading.—Although the reading is simple and direct, it is necessary to mention some of the underlying principles. The first task is to see which row stands still, or the nearest still. This row indicates the desired record and will be seen irresistibly the moment the tone is produced, because all other rows are blurred or in rapid motion. Having identified the line which stands still we must next know within what octave the tone lies. If, for example, row 128 stands still this may represent a tone of 64, 128, 256, 512 vibrations, or even higher. Now from 110 v.d. to 219 v.d. the correspondence is direct and the dots appear as actually spaced on the drum within an octave above this, the dots double in number and therefore stand only half as far apart; and, within the second octave above, they quadruple in number. It is therefore easy to see instantly from the spacing of the dots within which octave one is reading. If the spaces in row 128, *e.g.*, are one-half of the original the tone is 256 v.d.; if they are only one-fourth of the original steps, the tone is 512 v.d., etc.

But in fine reading we deal with fractions of vibrations. If instead of one row standing still, two rows move slowly in the opposite direction, the tone lies between these, and the fraction is determined by the relative rate of movement of the two rows. In a very accurate recording of instruments this may be done to a high degree of certainly by timing the movement with a stop watch over a considerable period of time. To do this we observe how many dot spaces are moving up or down and apply this general rule: if the ascend-

ing row has been counted, *add* the fraction of dot space per second; or, if the descending row has been counted, *subtract* it from the integral number of the row observed. It is best to count the faster moving of the two rows.

Sources of error.—There can be no time-error in the transformation of the sound wave into an illumination wave; they must synchronize, since one is the direct cause of the other. There can be no progressive change in the speed of the motor because if the motor does not run in step with the fork it must stop. The only possible error on the physical side lies in a tendency to pendular oscillation of the drum which may show a tendency to "hunt" when starting or when the current is too weak. By allowing a minute for the "finding" immediately after starting, and by securing a right adjustment of the strength of the current, this hunting movement may be reduced to an inappreciable or negligible quantity. The presence or absence of this source of error may, of course, be ascertained at any moment by registering the tone from a standard fork as a control. The limit of accuracy in the registering of the apparatus is therefore set by the limit of constancy in the driving fork. This fork being carefully balanced, firmly mounted, and kept in fairly constant temperature, shows a very high degree of constancy and compares favorably with a standard 100 v.d. fork.

But the main source of error lies in the reading, particularly of high tones. This need, however, be a source of error only in rapid reading. By making the tone long enough to observe the rate of fractional movement, one may secure any desired degree of accuracy in reading, as, *e.g.*, in registering a tuning fork, by timing the fractional movements of the dots for a sufficiently long time. In brief, without giving numerical records, we may say that the limit of accuracy in the use of the instrument is really set by the limit of accuracy of a tuning fork, the driving fork. For actual tests of accuracy in reading see Miles (6).

The use of the tonoscope.—The tonoscope furnishes us the first ready and, at the same time, reliable means of measuring directly the pitch of a tone as sung, spoken, or played with a musical instrument. Heretofore, graphic recording has been the only reliable method. This has the merit of accuracy but is entirely too indirect and laborious to be of general use in practical work. As we have seen, it registers the tone as sung or played under natural

conditions, and the record is simultaneous with the tone. The scope of its usefulness is therefore very great. It furnishes us an approach to countless problems both in pure and applied psychology. The psychology of tonal expression is a field practically unworked as compared with the psychology of the appreciation of tone, largely because we have not before had any convenient means of measurement.

A few concrete illustrations from the laboratory may be cited. In standardizing the pitch discrimination test (9) it was found necessary to compare the relative reliability of available instruments. Tuning forks, string instruments, reed instruments, wind instruments, and sirens were all tested by direct registration upon the tonoscope. Temperature coefficients, air pressure coefficients, and resonance coefficients were worked out by the same mode of registration. While most of these measurements could have been made in other ways, the tonoscope proved at least a good, labor-saving device.

The settling of disputed questions of pitch has been interesting. For example, there was a pitch discrepancy in the playing of the oboe and the French horn in a symphony orchestra. Each player was given an opportunity to register a specific tone in the tonoscope, and it was found that the oboe was playing consistently 1.5 v.d. flat. A vocal soloist had a tendency to flat relatively high notes. She observed the error and learned to make the right correction. A singer was practicing to eliminate an undesirable fluctuation of the pitch of the voice and was much helped in practicing before the tonoscope as before a mirror. In a recent article (10) I have described some measurements which at the present time could be made successfully only with the tonoscope.

There is a conspicuous place for the tonoscope in the musical conservatory. The ear of the singer or player is too generous because it seldom has any objective correction. The pupil persists in constant errors because there is no objective check on the ear. But the tonoscope does for the ear what the microscope does for the eye. It magnifies and objectifies to the eye, bringing out even small details of the pitch of the tone.

An actual experiment in training of the voice by the use of the instrument revealed among other facts the following (5): A group of six singers practicing daily for twelve days, part of the time

with the instrument and part of the time without it, showed that the average result of training with the instrument was superior to the average result of training without it, by forty-two per cent. in the ability to strike a tone, by fifty-five per cent. in the ability to sing musical intervals, and by twenty-six per cent. in the voluntary control of the voice in sharping or flatting; and the ability gained by virtue of the aid of the instrument was transferred in large part to ordinary singing.

The entire article by Dr. Miles in the present volume (6) should be regarded as supplementary to this description and particularly as furnishing illustrations of the use of the instrument. The general trend of usefulness of the tonoscope in the psychological laboratory, the musical conservatory, and other situations in which the registration of the pitch of tones is desirable may be indicated in a partial outline of measurements which may be made with it, as follows:

I. Striking a tone.

1. Voluntary control of the pitch of a vocal or instrumental tone—the first and simplest test of ability to sound a given tone in true pitch.
2. The effect on the pitch of a tone of conditions varied under control; *e.g.*, the character of the standard tone, the absolute pitch, the mode of tone production, distraction, practice, seeing the registration when producing the tone, information about previous errors, deliberate correction, etc.
3. The pitch and reliability of the pitch of an instrument, as in tuning, testing, standardizing, and comparing instruments.

II. Sustaining a tone.

1. Degree of accuracy or ability in holding a tone.
2. Constant tendencies—sharp or flat.
3. Artistic effects in singing a "constant" tone, as in pitch tremulo.
4. Inflection of speech.

III. Minimal producible change.

A fundamental measure of discriminative action in the voluntary control of the pitch of a tone—a sort of psychophysic foot-rule which serves the same purpose on the motor side as the minimal producible change serves on the sensory side; *e.g.*, as a unit of measurement in the study of individual difference, or the effect of any other controlled variable.

IV. Tonal transition.

1. The mode and the accuracy of attack and release of tones in singing, in playing, or in inflection of speech (a) in measuring precision, (b) in registering some artistic effect, and (c) in musical or oratorical training.
2. Testing of instrument; *e.g.*, in proving that the piston pitch pipe is unfit for the sounding of a key-note because the note is necessarily attacked by a gradual sliding up to the key.

V. Tonal intervals.

Accuracy in producing musical intervals; *e.g.*, single steps, the natural scale, the chromatic scale, melody as in singing or playing of an air; (a) for the purpose of psychological or aesthetic study, or, (b) as a means of training in the musical conservatory.

VI. Transcribing of speech or musical records from the phonograph or any other recording instrument.

There are thus two quite distinct fields in which the tonoscope in its present form may be employed, namely, in the research laboratory and in the practical work of the musical conservatory.

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ACCURACY OF THE VOICE IN SIMPLE PITCH SINGING

BY

WALTER R. MILES

Contents

The experiments here reported deal with two phases of simple pitch singing: (1) the ability of the voice to reproduce the pitch of a tone, and (2) the ability to make faint shadings in pitch, sharp or flat. The aim has been to formulate, if possible, a standard test for the measurement of each, to establish norms, and to investigate some of the underlying psychological factors.[1]

[1] The extensive measurements made would have been impossible were it not for the previous labor of Professor Seashore in perfecting a recording apparatus, the Tonoscope. Dr. Seashore has furthermore proved himself an unfailing source of inspiration and suggestion throughout the experimentation. The author is also under heavy obligations to Assistant Professor Mabel C. Williams, Dr. Thomas F. Vance, Messrs. Bruene and Malmberg, and the many observers for their kind and prolonged assistance.

HISTORICAL

The first investigator to employ the experimental method in attacking the problem of the accuracy of the voice in singing pitch was *Klünder* (11) 1872.[2] He used a manometric flame with two connected speaking tubes, an organ tone sounding in one while the observer sang simultaneously in the other. The difference in vibration number between the standard and sung tones was determined by counting waves. The average ± errors found on three tones, 128, 192, 256, v.d. are 0.761, 0.434, and 0.257 per cent. (of standards) respectively. The difference between 0.761 and 0.257 was thought to be due to the vocal cords and not to hearing.

Klünder was not satisfied with his method or his results and continued working on the problem, publishing a second time in 1879 (12). Again he used organ tones as standards and had his observers sing simultaneously with them, either in unison or in specified interval. The recording was done on smoked paper by means of two phonautographs. The two records were compared directly, that for the organ tone being used as a standard, and deviation in the pitch of the voice from that of the standard was computed in terms of .25 v.d. That Klünder was primarily interested in the physiological side of the problem is indicated by the questions which he set himself:

(1) Does our ear control the voice or is it controlled by the feeling of tension in the larynx? (2) How firmly does the voice attack tones? (3) Are the fluctuations of the voice such that give proof of control by the ear? (4) How many stress degrees of muscular tetanus are we justified in accepting through the performance of the muscles of the larynx?

Klünder found that for the pitches 96, 128, 192, 256, v.d., respectively, he himself as observer made the following ± errors: .32 v.d., .47 v.d., .62 v.d., and .59 v.d. This however was somewhat better than any of his other observers could do.

From this Klünder concludes that the voice is very accurate in reproduction of pitch and he answers his questions in substance as follows:

[2] Previous to this time Scott (17) and Blake (3) had developed phonautographs for registering voice curves.

(1) The vocal cords are held in labial tension by muscular tetanus. (2) The musculature allows from 40 to 170 different tensions in the tetanus. (3) The regulation of the pitch of the voice takes place directly through the sensation of tension in the larynx.

Seashore (19) in 1910 published in a very condensed form the results of experiments carried on in 1905 by himself and E. A. Jenner. Previous to that time, however, much work had been done in perfecting a registering apparatus, the tonoscope, which is fully described by Professor Seashore in the foregoing article in this volume of the Studies. Some preliminary experimenting also was done in 1901-'02 with the help of Edward Bechly, the results of which have never been published. Seashore and Jenner in their work sought to answer two questions: (1) Can we facilitate development of control in the pitch of the voice by using an aid to the ear in training? (2) May the ordinary limits of accuracy be exceeded by training with such an aid? In attacking these problems they used three measurements: (1) accuracy in reproducing a given tone, (2) accuracy in singing a required interval, and (3) the least producible change in the pitch of the voice. The standard or fundamental tone was 100 v.d., produced by a large tuning-fork; the intervals were the major third, the fifth, and the octave above this. The least producible change was determined for each of these four tones (1) in the least producible sharp and (2) in the least producible flat from the note as actually sung. Each period of practice consisted of one hundred and sixty trials, which took about forty-five minutes. The tests continued twelve days, approximately successive. During the first five days the singer depended entirely on the ear as in ordinary singing: then followed five days of singing with aid, *i.e.,* the observer was informed of the result of each trial immediately after it was made. The records of the eleventh day were taken without aid, while on the twelfth day aid was again given. Six men acted as observers. The conclusions of this investigation are quoted as follows:

"(1). The aid enhances the ability to strike a tone which has been heard. The superiority of the aided series over the unaided amounts to 42 per cent. (2) The aid enhances the ability to sing an interval. The superiority of the aided series over the unaided amounts to 50 per cent. for the major third, 50 per cent. for the fifth, and 60 per cent. for the octave. (3) The voluntary control

of the pitch of the voice is improved by the aid. The average superiority of the aided series over the unaided for all intervals amounts to 26 per cent. (4) There is probably some transfer of gain from the aided training to following unaided singing. (5) There is no evidence of transfer of the gain in the accuracy of the memory image. This is undoubtedly due to the fact we have here to do with memory rather than discrimination and the acquisition of accurate memory images is a slow process—too slow in this short series. (6) The gain in the discriminative control of pitch of the voice is fully transferred. (7) Improvements in the ability to sing a tone or an interval, and the ability to produce a minimal change, are very much more pronounced and more rapid in the aided than in the unaided series. (8) The second question is not answered absolutely by our records, but it seems probable (a) from the radical and immediate improvement of the aided series over the unaided, and, (b) from the introspection showing a tone which without the instrument seemed entirely satisfactory to the ear could be corrected by the ear after the error had been pointed out by the instrument, that a higher degree of accuracy of pitch in singing may be attained by aiding the ear in the training than would be possible to attain without such aid. No matter how keen the ear of a trained musician, it can be shown in a single test that his ear has been "too generous"—too easily satisfied, for when the error is pointed out objectively he can recognize it. We thus find cumulative evidence to show that the singer can not reach the physiological limit of accuracy by the ordinary methods of voice culture, because he has no objective criterion by which he can check up the accuracy of his ear. (9) The major third, the fifth, and the octave are approximately equally difficult intervals to sing. If we express the average error in relative fractions of a tone (1/25 of a tone) instead of in vibrations, the ratio is 1.4, 1.5, and 1.4, for the three intervals named above. The average error expressed in terms of vibrations shows that the difficulty of a natural interval varies approximately with the magnitude of the interval. (10) The minimal change is a relatively constant fraction of a tone within the octave. This is true for both the aided and the unaided series. If we reduce the records from vibrations to twenty-fifths of a tone, the minimal change is 3.1, 3.1, 3.6, 3.3, for the fundamental, the major third, the fifth, and the octave respectively. This is surprising,

because within this part of the tonal range the pitch discrimination is normally measured by a constant vibration frequency instead of by a constant part of a tone."

Cameron (4) 1907, varied somewhat the conditions of the experiment as performed by Klünder. In the first series the subject was asked to sing any tone of medium pitch, a second tone of low pitch, and a third of high pitch, and to sustain the pitch selected in each case as uniformly as possible throughout the singing. The second series was like the first except that each tone was interrupted by the insertion of short pauses of .3 second duration. In a third series, somewhat longer than those previously mentioned, the ability of one observer to imitate organ tones in the range 94 v.d. to 303 v.d. was tested. The tones were reproduced in sequence and, in chance order, partly simultaneously with the standards and partly by singing the tones immediately after the organ had ceased sounding. In a fourth series various distracting tones, (1) harmonious or inharmonious with the standard tone; (2) of greater or less interval from the standard; and (3) higher or lower than the standard, were introduced either at the beginning or just preceding the beginning of the reproduction by the observer. The more important results of the study are here summarized:

"(1) In the singing of a tone a sudden marked rise in pitch usually occurs near the beginning of the tone. This rise in pitch is so general as to seem to indicate a universal tendency. (2) No tone is sung entirely uniformly. It oscillates in pitch from period to period throughout its length in a somewhat irregular rhythmical fashion. (3) Very marked differences exist in different individuals with regard to their ability to imitate a standard tone. The subjects tested varied in degrees of accuracy in imitation of standard tones of different pitch from a small fraction of 1 per cent. to 13 per cent. of error. (4) There is manifest throughout a tendency to sing a tone higher than it should be sung. Thus the end of a tone is usually higher than the beginning and the sung tone (as a whole) is almost invariably higher than the standard tone. (5) Distractions when causing disturbances may affect the whole of the sung tone or only the beginning of the tone. In either case the effect of the distraction may be to cause the sung tone to vary from the standard (a) in the direction of the distracting tone; or (b) in the opposite direction from the distracting tone. (6) Sung tones vary-

ing from the standard under the effect or distraction are usually harmonious with the distracting tone. When the distracting tone is inharmonious with the standard tone, distraction is more likely to occur than when the two tones form a harmony. (7) A person may more or less closely imitate a tone which he has heard when his attention was engrossed in singing another tone of a standard pitch."

An important contribution to the general problem of control of the pitch of the voice in singing was made by *Berlage* (2) in 1910. During the summer of 1907 Berlage carried on a series of experiments in which definite time intervals were inserted between the breaking off of the standard tone and the beginning of the reproduction by the observer.[3] These intervals were of the following values stated in seconds: 1, 2, 3, 4, 5, 7, 10, 15, 20, 25, and 30. The tones were all sounded as *"a"* ('a' in 'ah'). This series is an amplification of the methods of Klünder and Cameron, and was undertaken for the purpose of finding the time interval most favorable for the imitation of tones, which when found became one of the conditions of further experimentation.

In the winter of 1907-'08 Berlage's general problem was to determine the influence of articulation and hearing in the vocal reproduction of tones. In this series (second) as in the third series by Berlage the standard tones to be imitated are voice tones. The variation of conditions consisted in having the standard tones sung part of the time by the observer and part of the time by the experimenter thus showing the immediate influence of hearing and of loud articulation in tone-reproduction. It seemed desirable to determine to what extent the influence of articulation is due to the larynx, and to the mouth cavity. For this purpose, in a third series of experiments, all the standard tones were sung by the observers, the vowel quality being varied under control. The standard and reproduction were sung, sometimes to the same vowel as *"i"*, *"i"*, or *"u"*, *"u"*, and at other times to different vowels as to *"i"*, and *"u"* or *"a"* and *"u"*. The chief conclusions reached from Berlage's experiments are the following:

(1) "Accuracy in the reproduction of a "strange" voice tone decreases rather regularly with the increasing time interval of from 1 to 30 seconds. Accuracy is greatest with an interval of from

[3] Berlage designates these tones as 'foretone' and 'aftertone'. 'Standard' and 'reproduction' are used throughout this study.

1 to 2 seconds. The values found here, for the variable average error, in the case of the observers amounted to only .5 v.d. and .6 v.d. (2) Observers reproduced their own voice tones more accurately than those of another (time interval 3 seconds). (3) The increase of precision shows itself chiefly in a decrease of the constant error. In the reproduction of outside standards and especially when they are near the boundaries of the voice range there is a tendency toward a constant error near the middle of the voice range. (4) In the reproduction of one's own tones vowel change works a disadvantage upon precision. With the standard tone sung as *"u"* and the reproduction as *"i"* there is a tendency for the latter to be lower, and vice versa when the vowels are changed. (5) In the reproduction of an outside standard the variable average error expressed in vibration frequency becomes larger with rising pitch, while if expressed in per cent. of vibration frequency it diminishes. (6) In the reproduction of one's own tones the variable average error expressed in vibration frequency remains rather constant with rising pitch. (7) The amount of departure of the individual tone sections (measured off in .1 second periods) from the general average of the reproduction shows no tendency, in the variations carried out in these experiments, to change according to the ordinal number of the tone sections in the course. (8) Only in the first .1 second is the reproduction regularly lower than the rest of the tone course. (9) Reproductions after the time intervals of from 3 to 10 seconds, in the case of two observers, show a sudden raising or lowering of the tone after the tone has progressed some .4 to 1.2 seconds. (10) The average departure of individual tone sections from the average for the tone is greatest in the reproduction of one's own tones. (11) The total amount of departure, expressed in vibration frequency grows with rising pitch so that—not considering rather marked irregularities with the individual observers—the amount of variation expressed in per cent. of a tone remains about constant."

The latest published study of this general problem to come to our attention is that of *Sokolowsky* (22) 1911. His apparatus consisted of a combination of the Einthovan string-galvanometer and the Weiss phonoscope. The organ tones, which were used for standards, acted on the string-galvanometer and the sung tones on the phonoscope. Both tones were registered in a convenient way for comparison by means of the Blix-Sandström photokymograph.

Sokolowsky secured the coöperation of seven professional opera singers, three men and four women. The observers were allowed to choose the vowel to which they sang the tones. The musical *"a"* was chosen most frequently. There were three short series of experiments: (1) singing a given tone simultaneously with the sounding of the tone by the organ (unison); (2) allowing a time interval between the organ tone and its reproduction. (The intervals used were 30, 60, and 120 seconds, during which the observers were instructed not to hum or sing to themselves); and (3) singing a specified interval from a simultaneously sounding organ tone. The musical intervals selected were the third, fourth, fifth, sixth and octave.

The results from these three series of experiments may be summarized as follows: (1) Curves for 8 tones were secured in series I. The average pitch was 251 v.d. (range 165 to 296 v.d.), the average error was ± 0.44 per cent. The average pitches for men and women respectively were 197 and 286 v.d., with average ± errors of 0.51 and 0.40 per cent. (2) The introduction of a time interval increases the average error to ± 0.99 per cent. as compared with ± 0.44 of the previous series. Errors are usually larger with an interval of 60 seconds than with 30 seconds. (3) The average error in series III is ± 1.51 per cent. The largest errors, average ± 3.28 per cent. are on the fifth, while the smallest, average ± 0.78 per cent. are on the third. (4) Of the entire number of tones counted (46) 36 are sung flat and 10 sharp. The errors on the side of sharping are divided among three women and one man; those on the side of flatting between three men and three women.

Guttmann (6) 1912, in his consideration of the psychophysics of singing gives some attention to the problem of accuracy in reproducing pitch and states that recently he has been engaged in an extensive research in this field. The results are to be published shortly in one of the psychological journals, but in a preliminary way he says that they agree in general with those secured by Klünder and Sokolowsky, but he thinks that the results of the latter (unison curves) are "too good".

Other investigators, among them *Hensen* (10) and more recently *Marbe* (14), *Grützner* (5) and *Scripture* (18) have developed methods for recording the pitch of the voice, but these seem not to have been used in gathering data on our problem.

THE TONOSCOPE

In the investigations of Klünder, Cameron and Berlage the vibration frequency of the tones was recorded in tracings on smoked paper. Sokolowsky photographed his records; after these had been rendered permanent the waves were counted and the pitch determined by comparison with a time or standard line. This method, commonly known as "graphic recording" has been used with various refinements by many investigators in the field of phonetics. While reliable, it is at best indirect and very laborious.

Seashore and Jenner in their research made use of an early model (20) of the tonoscope. This instrument as lately improved was used by the author in the present experiments.[4] It has several advantages which recommended it as an instrument for the measurement of the pitch of tones. In the first place readings are made quickly and directly. The instant a tone is sounded the vibration frequency is indicated by a row of dots. The experimenter has simply to note the number of this row and to record it. He is, therefore, enabled to secure a large number of observations in a relatively short time. It is not difficult to take two hundred records in thirty minutes. In the second place the experimenter has the advantage of knowing how the test is progressing. If a preliminary practice series is desired to acquaint the observer with some procedure we have in the direct readings from the tonoscope an index to the observer's understanding of the test. The observer must be kept actively trying throughout the experiment. In psychological tests, such as the imitation of tones by singing, there is so much repetition in the program for the observer that his attention easily wanders. Large and unnatural errors are therefore likely to be found in the records. Here the tonoscope as a recording instrument has an advantage over other methods as it provides for detecting these errors as soon as they occur. The experimenter as he takes each reading notes roughly the attack, the steadiness, and the degree of success with which the reproduction approaches the standard. He thus easily becomes acquainted with the unusual range of variability and forms an estimate of the observer's power to control his voice. When a

[4] The instrument is fully described in the preceding article in this volume of Studies, "The Tonoscope", by Professor Seashore. A reading of that article is essential for an understanding of the present report.

tone of unusual divirgence is given he therefore immediately recognizes it and can take cognizance of it, asking for introspection or for a new trial, and all with scarcely any loss of time. He may thus check up and to some extent control the observer,—keep him at his best. Furthermore the possibility of encouraging the observer or even of giving him full information regarding the success or failure of each trial is in itself a most important asset.

The tonoscope has been criticised as giving only an approximate result, because the pitch of the singing voice is not uniform and it is therefore necessary in reading the instrument to select the predominating pitch. This criticism stands or falls according to the needs of the problem to be attacked. If one were studying the oscillations of the voice, or the variations of the individual sections of a tone, as for example the difference in pitch between the first tenth and the fifth tenth of a second of a tone, it would be better to use a graphic method. But even in such problems as these the tonoscope is not without its possibilities. The characteristics of tonal attack in singing are easily discernible in the configurations on the screen. With many of the problems which lie in our field there is no need for so detailed a record. The predominant or modal pitch of a tone of from one to two seconds in length is all that is needed for much of the work in the psychology of pitch singing. The tonoscope can of course meet this condition admirably, as it is this modal pitch which stands out clear and distinct, forcing itself upon the attention of the experimenter.

Tonoscope reading test.—The method of reading the tonoscope, and the various sources of error having been fully treated by Professor Seashore in the accompanying article, there is no need to repeat them here.

In order to determine the degree of accuracy in the reading of the tonoscope the following experiment was performed. A set of ten large, movable-disc, tuning forks ranging from 128 to 131 v.d. was so tuned that no two forks had a pitch difference of over 3 v.d. and in the great majority of cases the differences were much smaller. A revolving shutter, rotated by the tonoscope shaft, was so arranged as to expose the mouth of a resonator connected with the sensitive light for the following time intervals: .25, .50, .75 and 1.00 second. In this way a tone sounded before the shutter was registered by the tonoscope for just the period during which the

resonator was exposed.[5] The presentation of the tones and the recording of the observations were in charge of two helpers. The experimenter did nothing but watch the moving screen and call out the readings. He had no way of knowing the real reading in any case. Five trials were given on each fork with each exposure interval. The order of the forks was determined approximately by chance. There was an interval of about five seconds between tones.

After the fifty trials with the .75 second exposures were finished, the pitch of each fork was carefully determined with the tonoscope, counts being made by the stop-watch during periods of from 6 to 15 seconds. These records formed the basis from which to compute the errors in the first test. The assistant then changed the pitch of all the forks and the above procedure was repeated with a .50 second exposure. Again the forks were changed and the same procedure was followed for the 1.00 second and the .25 second exposures in turn. Thus fifty records were obtained for each of the four exposure periods and the conditions were such that the reader could have no accessory clue. The record is summarized in Table I.

To test the reading ability for tones one octave higher, *i.e.* 256 v.d., where it will be recalled the tonoscope reading, and hence the errors, must be doubled, a set of seven small forks was provided. These were weighted so that no pitch difference between any two forks was greater than 3 v.d. The test was made with the exposure interval of .75 second.

In making the pitch difference between the forks come within a range of 3 v.d. we approximate the condition presented when working with voice tones that require accuracy in reading. If an observer is asked to reproduce a tone or to sing an interval the experimenter knows approximately the point on the scale where the reading should occur. He is watching this point. Should the reproduction be nearly correct and the tone fairly constant for, say .50 second, he can read according to our result (see Table I) within an error of less than $\pm$ .2 v.d. If however the reproduction goes wide of the mark, for example to the extent of 6 v.d. there is no need of reading in fractions smaller than halves.

[5] This arrangement is not ideal in that, as the tone is turned on and cut off by the disc, slightly disturbing waves are set up and show on the screen. In test No. 4 where the tone sounded for .25 second this was felt to be very disturbing. The real time given for the reading of the tones in all these tests was thus slightly less than that represented by the several discs.

TABLE I. The degree of accuracy in the reading of the tonoscope

Exposure 1.00 sec.		Ave.	error	.12	v.d.	; m.v.	.10	v.d.
" .75 "	(128 v.d.)	"	"	.15	"	; "	.12	"
" .75 "	(256 v.d.)	"	"	.19	"	; "	.17	"
" .50 "	(128 v.d.)	"	"	.18	"	; "	.11	"
" .25 "	(128 v.d.)	"	"	.65	"	; "	.27	"

EXPERIMENTS SERIES I: ACCURACY AND THE VOICE RANGE

In the first five series of experiments the purpose was to answer questions concerning some factors which must be considered in any adequate test of voice control. (1) How does accuracy of control vary with the range of the voice? (2) How does the intensity of the standard tone affect the pitch reproduction? (3) What is the relation of voice volume to voice control? (4) Are the reproductions affected by the timbre of the standard tones? (5) Do vowel changes (timbre changes) in the reproductions cause changes in the pitch of the reproduction? The sixth series represents an effort to combine into a single test the results of our previous experiments, together with those of other investigators, and to give this test to a sufficiently large group that we might be enabled to determine from the results some of the norms of voice control.[6]

Seventeen men with splendid enthusiasm gave their services as observers in the experiments of Series I. From among this number several were selected to serve as observers in Series II, III, IV, and V. The observers were all of mature age and more than half their number had had some training in the methods of experimental psychology. P, the only professional musician in the group, is a teacher of "Voice" and a thoroughly trained tenor soloist. H, a baritone of extensive special training, has for some time been the leader of a large choir. He is a soloist of ability. Ma., W, and V. Z. have all had special training in singing, and much experience in solo, quartette and glee club work. S, C. Mi., Ro., An., Wi., and V. H. all have had considerable experience in general singing but are without special training. Ri., Ab., Mc., Br., D, and Bh. very seldom sing in public but they enjoy music.

[6] Gutzmann (7) and Sokolowsky (22) suggest some of the above problems, especially Nos. 1 and 5 as being important. These articles and suggestions however did not come to the attention of the writer until the experimentation was completed.

For Series I the standard tones were provided by a set of twenty tuning forks ranging approximately by the chromatic scale from C_1, 64 v.d., to and including a′, 426 v.d. The first fourteen forks beginning with 64 v.d. were large and carried discs. All the tones were of good quality and their duration of tone was more than ample. Some of the forks were of different vibration frequency than that indicated by the notes of the chromatic scale; for example, the pitch of the fork that corresponded to G was 182 v.d. in place of 192 v.d. These differences were made in order to check the observers from judging and singing the various steps as musical intervals.

An independent selection of five forks was made for each observer after a preliminary determination of his voice range. These forks covered approximately one and one-half octaves in the middle of the range and were fairly distributed. In giving the test the experimenter presented the tones to the ear of the observer, who, after listening for 1.5 seconds and allowing a time interval of 1 second, reproduced the pitch of the tones as accurately as possible. Proceeding from the lowest to the highest and then in reverse order back to the lowest, each tone was given twice in succession, the test consisting of twenty trials on each standard tone.

The results of these experiments are present in Table II. O denotes the observer; P, the pitch of the standard tone; E, average error; m.v., mean variation; and C.E., constant error. These five successive columns give the record of the respective standards for each observer. The footings in the table show the averages of the figures above stated, first in terms of vibration (absolute) and second, in terms of percentage of a tone (relative) at the respective levels. The average C. E. in the footing is the average of C. E. regardless of sign; in the second the sign is taken into account giving group tendency of the constant error, or group constant error (G. C. E.). These footings are represented graphically in Fig. 1.

Taken as a whole these records show that accuracy in the reproduction of the pitch of a tone, as measured by the average error (E) with its mean variation (m.v.) the average of the constant errors (C. E.) and the general tendency of the constant errors (G. C. E.), tends to be a constant in terms of vibration frequency. This is shown in Fig. 1 (A) by the fact that the four curves, for

the absolute variation tend to remain horizontal lines whereas the four curves for the relative variation (B) tend to fall in inverse ratio to the rise of the pitch. The slight tendency to deviate from the constant in terms of vibrations is in the direction of decrease in accuracy with rising pitch. This, in the case of the highest tone,

TABLE II. Accuracy and the voice range

O	P	E. m.v. C.E.	P	E. m.v. C.E.	P	E. m.v. C.E.	P	E. m.v. C.E.	P	E. m.v. C.E.
		1.6		1.2		.9		1.9		1.2
P.	128	1.1	160	.6	182	.8	256	1.6	320	1.2
		—1.5		—1.2		+ .4		—1.6		+ .4
		1.4		3.1		.7		2.9		3.7
H.	95	.6	120	1.1	160	.7	213	1.4	286	1.4
		+1.4		+3.1		— .1		—2.9		—3.7
		1.5		.8		3.0		3.7		3.1
Ma.	95	.7	120	.8	160	1.1	240	1.0	286	1.3
		—1.5		+ .4		+2.9		+3.7		+3.0
		.9		1.0		1.7		2.5		1.8
W.	120	.9	144	.8	182	1.6	256	1.5	320	1.7
		+ .2		+ .4		— .2		+1.9		— .1
		3.1		1.6		1.7		1.1		2.7
V.Z.	120	1.1	144	.7	182	.9	256	1.1	341	1.4
		+3.1		+1.6		+1.4		+ .3		+1.7
		2.6		1.8		1.0		2.1		7.1
S.	120	.8	144	.9	182	.8	240	1.2	286	1.8
		—2.4		—1.5		— .3		— .9		+7.1
		.9		.7		.8		1.3		1.7
C.	95	1.2	144	.7	182	.6	256	1.9	341	1.5
		+ .2		— .0		— .3		+ .8		— .9
		2.9		1.1		1.6		2.7		3.5
Mi.	86	2.3	120	.8	182	1.9	256	2.7	341	1.8
		+4.0		+1.0		— .1		+1.0		+3.4
		2.3		2.9		2.1		4.5		2.5
Ro.	95	4.5	144	2.2	182	2.0	240	1.8	320	2.0
		+ .1		+2.5		+1.3		+4.3		+ .2
		5.0		1.6		2.5		4.5		4.6
An.	95	2.3	120	1.5	160	2.8	213	2.6	256	3.5
		+3.7		+ .8		—1.0		—4.1		—3.1
		3.5		4.2		2.0		1.2		4.2
V.H.	107	1.7	128	1.4	160	1.7	182	1.0	240	2.2
		+2.7		—3.6		—1.0		+ .1		+3.9
		2.3		2.9		2.1		4.5		2.5
Ri.	95	1.9	144	2.3	182	1.9	240	1.5	320	2.1
		+2.5		—1.6		+ .2		— .5		+2.7

TABLE II. (*Continued*)

		3.2		5.5		3.1		1.3		1.4
Ab.	95	1.0	128	1.4	160	.9	182	.8	256	.9
		+3.1		+5.5		+3.1		+1.7		+ .9
		1.7		3.9		1.9		1.0		2.6
Mc.	95	.5	120	1.3	160	.8	213	.9	256	1.2
		—1.6		+3.8		—1.7		+ .4		+2.0
		2.9		5.6		6.4		3.7		2.7
Br.	95	2.2	144	1.0	182	1.1	256	1.0	384	2.8
		+2.7		+5.2		+6.4		+3.6		+1.3
		1.7		3.3		5.9		6.3		11.2
D.	95	.9	128	3.1	160	2.4	182	6.6	256	5.4
		—1.4		— .3		—5.9		—3.4		—6.5
		1.6		2.4		3.0		5.1		2.5
Bh.	120	1.1	144	1.6	182	1.1	240	1.8	286	1.4
		—1.3		+ .9		+3.0		+5.1		+1.5
P	103.0		135.0		172.9		230.6		299.4	
Average in v.d.	E	2.3		2.6		2.4		2.8		3.5
	m.v.	1.5		1.3		1.3		1.8		2.0
	C.E.	2.0		2.0		1.7		2.1		2.5
	G.C.E.	+ .8		+1.0		+ .5		+ .5		+ .8
Ave. in per cent. of a tone	E	.18		.15		.11		.09		.10
	m.v.	.11		.08		.06		.06		.05
	C.E.	.15		.12		.08		.07		.06
	G.C.E.	+.06		+.06		+.02		+.02		+.02

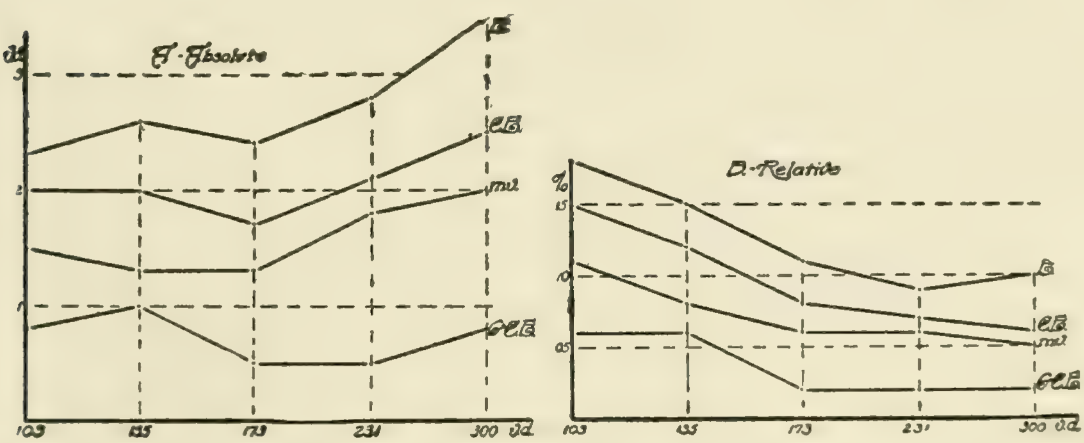

Fig. 1. Accuracy and the voice range. (Table II)

is to be accounted for mainly by the fact that some observers were erratic on this tone, probably because the tone was higher than the observer commonly sings. As a matter of fact only half of the observers, nearly all of whom would be classed as bass or baritone in their range, show any tendency to decrease in accuracy at this

point; five show the tendency to increase in accuracy and the remaining four tend to remain constant.

This result is in harmony with results found by *Preyer* (16), *Luft* (13), *Meyer* (15) and *Vance* (26) on the sensory side, that pitch discrimination is approximately constant in terms of vibration frequency within this range. It is in harmony with the finding of Berlage (2) as quoted above: item 5 (second part), that average error diminishes with rising pitch if expressed in per cent. of vibration frequency; and item 6, with reference to the reproduction of one's own tones.

It is interesting to compare and to contrast these records with those of Seashore and Jenner (19), item 9, showing that the average error in the singing of a natural interval (third, fifth, and octave) varies approximately with the magnitude of the interval; (see also Sokolowsky's results above) and, item 10, showing that the minimal change is a relatively constant fraction of a tone within the octave.

The tendency for the C. E. to be in one direction (+) will be considered in a later section in connection with the constant errors in our other series of experiments.

EXPERIMENTS SERIES II: INTENSITY OF STANDARD

In the experiments of Series I, as stated above, two successive trials were made on each fork. Occasionally upon the presentation of the tones for the second trial at reproduction the observer would say "Let me hear that again; it sounds higher (or lower) than before", or "Is that the same fork?" Such remarks by careful observers led to this consideration of the intensity factor.

The same forks were used with the respective observers as in Series I. The tones were presented to the ear by the experimenter as before. But with half of the trials the standards were made about as strong as possible by striking the forks a heavy blow and presenting them near the ear. The other standards were made as weak as could be heard with distinctness. The observers were encouraged to sing with a medium volume of voice and not to imitate that of the forks, as is the natural tendency. Twenty records were made with each fork, ten on the "weak" and ten on the "strong" in the double fatigue order, as regards pitch and intensity. No successive trials were made on the same fork except on the highest and lowest. Having sung the tones from the highest the

observer would sing them in reverse order from highest to lowest; but a short pause was introduced between such successive reproductions. Of the eight observers tested, P, Ma., V. Z., C, An., V.H., S and Mi., the first six had no definite knowledge of the object in view.

The results are shown in Table III and graphically represented in Fig. 2. *"W"* denotes weak and *"S"* strong, while the other notation is the same as that previously used. It will be seen that the intensity of the standard tone has a decided effect upon the accuracy of reproduction.

(1). Increase in intensity causes a lowering in the pitch of the reproduction. The G. C. E. for S on each of the five levels is less than that measure for *W.*, the minimum amount of difference being 1.4 v.d., the maximum 4.1 v.d. and the average for the five pitches, 2.3 v.d. In all the forty individual constant errors with the exception of two (see V. H.'s lowest tone and C.'s highest; in this latter *"W."* and *"S."* are just the same) the reproductions of the "strong" standards are lower than those of the "weak". If we compare these averages (C. E.'s and G. C. E.'s) with those of the previous series of experiments we find not only that the "strong" C. E.'s and G. C. E.'s are lower in the majority of cases than those of Series I, but that these measures for the reproductions made from the "weak" standards are somewhat higher than those of the former series. The effect of intensity in other words, is evident in both "weak" and "strong" standards, the former heightening the seeming natural tendency to sharp and the latter overcoming this tendency with a more powerful one to flat.[7]

(2). Strong standard tones cause general inaccuracy of voice control. Most of the observers stated that they were less sure with the "strong" standards. Others complained that the test made their ears tired. Reference to the mean variations and also to the E.'s and C. E.'s will show that in the majority of cases these amounts are

[7] When the conditions of this experiment (Series II) were explained to P., the professional musician, he remarked off hand as he began the test: "Loud tones would make your nerves more tense and would in general tend to make you sharp." He was asked then and at other times during the test to let any conscious tendency to flat or sharp take care of itself *e.g.* not knowingly to correct for it. At the last P. said: "I am equally satisfied with my reproductions of weak and strong." Cf. P's. record in Table III.

TABLE III. Intensity of standard tones

Ave. P.	105 v.d.		135 v.d.		174 v.d.		237 v.d.		301 v.d.	
	W.	S.	W.	S.	W.	S.	W.	S.	W.	S.
O.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.	E. m.v. C.E.
	1.3	2.1	.7	1.2	2.3	1.4	1.1	.7	3.0	1.0
P.	1.0	.6	.5	.9	1.2	1.4	.7	.7	1.3	.8
	−.7	−2.1	+.3	−1.0	+2.3	+.3	+1.1	+.2	+2.9	+.5
	1.1	1.1	1.0	1.4	3.2	1.7	3.1	2.1	2.1	1.4
Ma.	.9	.9	1.0	.8	.8	1.7	.6	1.0	.7	1.0
	+1.0	−.7	−.2	−1.4	+3.2	+.8	+3.1	+2.0	+2.1	−1.4
	3.8	1.6	2.9	1.1	3.2	.7	1.9	4.8	2.0	1.8
V.Z.	.7	.9	1.3	.7	.5	.7	1.5	1.2	1.7	1.3
	+3.8	+1.2	+3.1	+1.0	+3.2	−.2	−1.2	−4.8	+.3	−2.1
	1.2	.7	1.9	1.1	1.7	.7	3.0	2.4	2.8	2.6
C.	.5	.7	.5	1.1	.8	.7	1.1	.9	2.0	1.0
	+1.2	+.2	+1.9	+.3	+1.7	−.1	+3.0	+2.4	+2.6	+2.6
	6.4	5.4	1.9	1.1	1.9	2.3	3.2	7.8	3.7	8.1
An.	6.4	2.8	1.3	1.1	1.9	1.3	1.2	1.7	1.8	1.2
	+6.4	+4.8	+1.9	−.1	−.5	−1.9	−3.1	−7.8	−3.4	−8.1
	3.1	2.9	3.9	3.5	1.0	1.9	2.2	2.1	2.9	8.0
V.H.	1.6	1.7	1.0	1.5	.8	1.8	1.9	2.1	3.0	3.4
	+2.6	+2.9	+3.9	+2.9	−.7	−1.2	+.8	+.3	−.5	−8.0
	1.8	4.2	1.0	.7	.8	3.6	2.1	4.7	5.3	5.5
S.	.5	.7	.5	.6	.8	1.0	1.2	.9	1.3	1.4
	−1.8	−4.2	+1.0	−.4	−.2	−3.6	−1.8	−4.4	+5.3	+5.5
	9.3	8.5	1.5	1.7	2.2	5.4	4.0	4.5	7.8	4.7
Mi.	1.9	4.1	.9	.9	2.0	2.9	3.1	4.1	1.9	2.9
	+9.3	+8.5	+1.5	+.5	−.8	−5.4	+2.4	−2.1	+7.8	−4.6
Av. E.	3.5	3.3	1.9	1.5	2.0	2.2	2.6	3.6	3.7	4.1
Av. m.v.	1.7	1.6	.9	1.0	1.1	1.4	1.4	1.6	1.7	1.6
Av. C.E.	3.4	3.1	1.7	1.0	1.6	1.7	2.1	3.0	3.1	4.1
G.C.E.	+2.7	+1.3	+1.7	+.3	+1.0	−1.4	−.5	−1.8	+2.1	−2.0

larger with strong standards, thus indicating conditions that operate against the best vocal control.

The matter of intensity has been considered in the field of pitch discrimination, where it must really be worked out. Seashore (21) makes the following statements concerning it:

"Extensive experiments show (1) that both trained and untrained observers may be influenced by intensity in their pitch judgment; (2) that although there is a tendency among the untrained, especially the ignorant, to judge the loud tone the higher, it may work either way; (3) that the same individual may show one tendency at one time and the reverse at another; (4) that for trained observers the two tendencies are about equal; and (5) that the tendency is more serious for large than for small intensity differences. Introspection shows that this confusion rests largely on motor tendencies, or motor images. We associate high and strong with strain—the reversal can in some cases be traced to a correction, conscious or unconscious, based on knowledge of this danger.

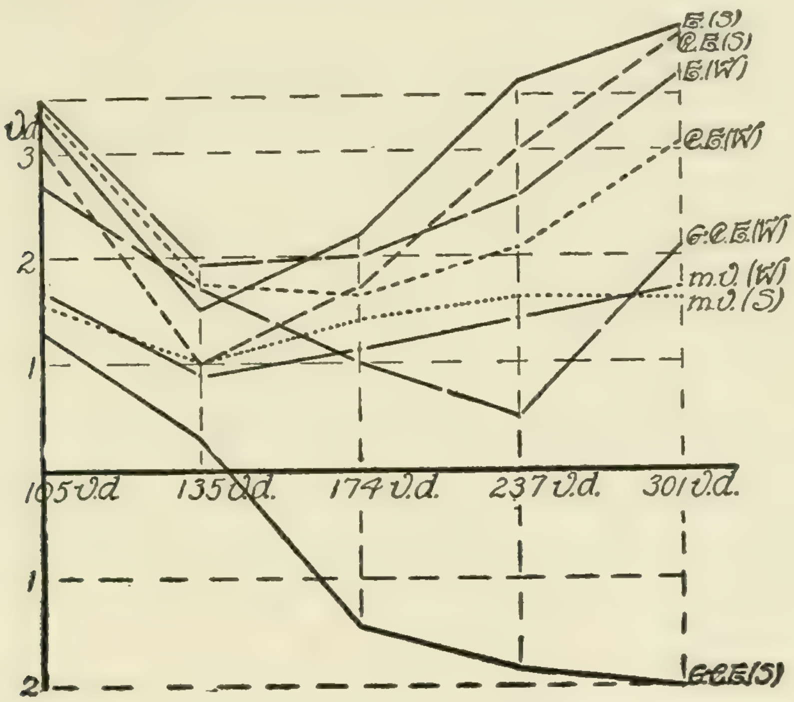

Fig. 2. The influence of intensity of standard tones.

Experiments show that the just perfectly clearly perceptible tone is most favorable for accurate results. It is ordinarily purer than a stronger tone and favors concentration. Experimenters must guard against a very common tendency, usually unconscious, to facilitate the discrimination by making the tones loud; and untrained observers usually desire (unwisely) a loud tone."

These conclusions are found on tests made by Anderson (1) at the level of 435 v.d. Our results just stated led to a re-examination of the effect of intensity on pitch. Hancock (8) found that as

measured in terms of hearing alone the tendency to hear a relatively low strong tone as low is greater than is shown in this series for singing. All these facts make clear that in singing from a standard tone greater care must be exercised to keep the tone constant and at a most favorable strength. We have no adequate quantitative data to show what strength is best but the facts available tend to support the statement made by Seashore (21) that the just perfectly clearly perceptible tone is most favorable for accurate results.[8]

EXPERIMENTS SERIES III: VOLUME OF THE VOICE

The effect produced by varying the intensity of the standard tones suggested a parallel question concerning the relationship of voice volume and accuracy of reproduction. This problem was attacked in the following manner. The forks selected were the same for each observer as in the voice range test, Series I; they were presented to the observer's ear by the experimenter who endeavored to keep the intensity as nearly constant as possible, and the observer was instructed to reproduce the tones in three degrees of voice volume, "loud", "medium" and "weak". Ten trials were made on each fork with each of these three degrees of loudness of voice, the order being as follows: one trial on each fork from lowest to highest and after a pause from highest to lowest with "medium" intensity; from highest to lowest and back to highest with "loud" intensity; from lowest to highest and back to lowest on "weak"; highest to lowest and back on "medium" and so forth until the 150 trials were made.

These records are summarized in Table IV and represented in part in Fig. 3. In this table *L, M* and *W* represent respectively loud, medium, and weak, other notation is the same as in the foregoing tables.

Here we find again, as in the foregoing series, the tendency for accuracy in singing to remain a constant in terms of vibrations, except for the extreme notes, at which there is a decrease in efficiency, especially at the high note. The form of the average error curve (E)

[8] The force of the blow changes the pitch of a fork, (See Winkelmann's Akustik Vol. 2 p. 358) lowering it slightly, but this change in these forks could hardly be detected and certainly fails to acccount for the error in reproduction. See also Seashore (21).

here is entirely analogous to the form of the curve of pitch discrimination referred to above (16, 13, 15 and 26) but it represents a shorter range, as the voice has a shorter range than the ear.

The constant error for men here, as in the foregoing series, is in the direction of sharping. It is a relatively constant fraction of a vibration for all pitches except the highest. The records for the medium and the weak tones practically coincide,[9] and compare very favorably with those of Series I., but there is a uniform tendency to sing the loud reproduction highest. The average difference between the loud and the weak (see the G. C. E.'s) is here .6 v.d. This is not a contradiction, but the reciprocal of the results found in Series II: namely, that the loud (or strong) standard is reproduced low.

It will be remembered that in Series II the standard was made strong, the observer tried to produce a tone that subjectively seemed the same in pitch, and that practically all of his reproductions were flat. This result in the light of Series I, where sharping was the rule, seemed to warrant the conclusion that the strong tone is judged low. Now in Series III we have a confirmation of this; here the standard is of medium intensity while the reproductions are varied: loud, medium and weak. It seems therefore that the instant the observer commences his loud reproduction he is subject to the same error in judgment as was revealed in Series II, and that to make his reproduction subjectively equal in pitch to the standard, he thinks it necessary to raise. This brings about abnormal sharping: the average G. C. E. of Series III is +1.3 v.d. as against +.7 v.d. for Series I, where intensity differences were at a minimum.

The agreement of the errors (G. C. E.'s) in these two series (II and III) at once offers an explanation for them: the error is primarily one of hearing which is basal and the chief cause for the error in singing.[10] This is in harmony with the contention of Klünder

[9] Medium and weak tended to be confused by the observers who would frequently have to be reminded that they were not making sufficient difference between them. This would imply that they each seemed more natural and less distinct than the loud, which is borne out by the fact that the average G. C. E. for weak (Series III) is identical with that for Series I, *i.e.* +.7 v.d. However it should be noted that the curves for weak are less regular than in Series I.

[10] It is interesting to note than in Series II the high strong is flattened most, while in Series III the high loud is sharped most.

TABLE IV. Volume of the voice and accuracy

Ave. P.		109 v.d.			137 v.d.			176 v.d.			241 v.d.			308 v.d.		
O.		L	M	W	L	M	W	L	M	W	L	M	W	L	M	W
P.	E.	3.0	2.0	1.3	2.8	1.4	.9	[illegible]	.9	1.8	1.9	1.3	1.5	1.2	2.7	4.1
	m.v.	.6	.8	.7	.5	1.2	.5	.8	.9	.9	1.1	1.3	.6	1.1	1.6	.8
	C.E.	−3.1	−1.9	−1.2	−2.7	− .9	+ .1	− .3	− .2	+1.6	−1.7	− .5	+1.5	+ .8	+2.4	+1.4
Ma.	E.	2.0	1.8	1.3	2.0	1.4	1.7	2.5	3.0	3.9	2.2	1.0	1.1	1.4	1.7	2.4
	m.v.	.7	1.0	.5	1.6	1.4	1.5	1.3	1.3	.9	1.0	1.0	1.1	1.2	1.5	1.3
	C.E.	−2.0	−1.5	−1.3	−1.5	+ .1	− .5	+2.4	+3.0	+3.9	−2.2	+ .5	+1.1	−1.2	−1.2	+2.3
V.Z.	E.	3.9	3.5	3.2	2.9	2.5	2.6	2.3	2.0	1.3	2.2	2.5	2.1	1.0	1.2	1.3
	m.v.	1.0	1.1	1.4	1.1	1.2	1.4	1.6	1.9	.7	1.2	1.0	.7	1.0	1.2	1.0
	C.E.	+3.9	+3.5	+3.2	+2.8	+2.5	+2.3	+2.0	+1.8	+1.0	−1.7	+2.3	−2.0	− .5	− .4	+1.2
S.	E.	2.3	2.3	2.7	1.2	.9	1.1	3.2	2.4	2.6	.9	1.4	.9	7.6	5.2	5.2
	m.v.	.8	.4	.5	1.2	.8	1.0	1.1	.6	.7	.8	.9	.8	1.9	1.4	1.5
	C.E.	−2.2	−2.2	−2.6	+ .1	− .5	+ .6	+3.1	−2.4	−2.6	+4.4	−1.0	− .3	+7.5	+5.2	+5.2
C.	E.	1.4	1.2	1.0	1.9	1.4	1.7	1.5	.4	.7	3.0	3.6	2.4	5.6	3.9	2.8
	m.v.	.5	.6	1.0	1.1	.6	.8	1.5	.4	.8	.9	.4	1.1	1.5	1.6	1.1
	C.E.	+1.4	+1.2	− .6	+1.3	+1.4	+1.7	− .5	− .4	+ .4	+3.0	+3.5	+2.3	+5.5	+3.8	+2.8
V.H.	E.	9.0	4.9	2.5	2.1	1.7	1.6	2.9	2.2	4.3	1.6	2.2	2.0	2.2	4.2	3.0
	m.v.	1.8	2.1	1.3	1.4	1.5	1.6	2.3	2.0	3.6	1.7	2.8	2.0	1.5	2.4	2.8
	C.E.	+8.9	+4.9	+2.5	+2.1	− .8	+2.0	−2.1	−1.2	−2.5	+ .4	+ .8	− .4	−1.3	−4.2	−2.0
Mi.	E.	2.3	1.6	1.7	2.7	2.6	2.7	3.1	2.3	2.0	3.1	1.3	2.6	9.5	3.5	3.0
	m.v.	1.6	1.5	1.2	1.5	1.6	2.0	2.4	2.3	1.6	2.3	1.4	2.2	3.5	1.0	2.6
	C.E.	+2.2	+1.2	[illegible] 1.3	+2.7	+1.9	+2.3	+1.5	+ .3	−1.2	+3.1	+ .7	+1.8	+9.5	+3.5	+1.2
Av. E.		3.4	2.5	2.0	2.2	1.7	1.8	2.3	1.9	2.4	2.1	1.9	1.8	4.1	3.2	3.1
Av. m.v.		1.0	1.1	.9	1.2	1.2	1.3	1.6	1.3	1.3	1.3	1.3	1.2	1.7	1.5	1.6
Av. C.E.		3.4	2.3	1.8	1.9	1.2	1.3	1.7	1.3	1.9	2.3	1.3	1.3	3.8	3.0	2.3
C.G.E.		+1.3	+ .7	+ .2	+ .7	+ .5	+1.0	+ .9	+ .1	+ .1	+ .8	+ .9	+ .6	+2.9	+1.3	+1.7

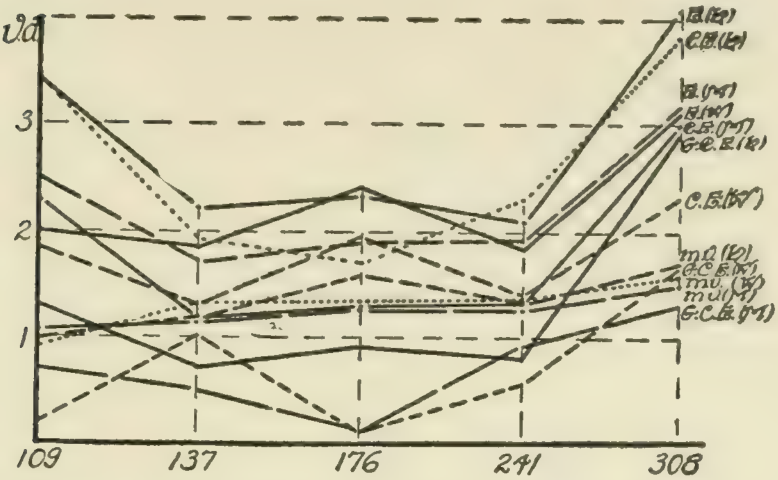

Fig. 3. Volume of the voice and accuracy (Table IV).

(12) that the ear is the chief criterion for regulating the voice. But the result quoted from Hancock (8): that the hearing error is greater than the singing error, (when dealing with a low, strong tone), together with the fact (Series III) that there is relatively more flatting with a strong standard than there is sharping with a loud reproduction, would conform to the conclusion reached by Stern (24) that the kinaesthetic sense of the singer is also an important factor.

One would expect a larger mean variation (m.v.) for the tone that has the largest error, but the table shows the mean variation to be practically equal for all three intensities of sound. This may be taken as a mark of the relative constancy of the motive for the intensity error.

The agreement and the remarkable uniformity in these two laws as shown in Series II and III would indicate that we are here dealing with an important factor of which we must take cognizance, both in the hearing and the producing of musical tones.

EXPERIMENTS SERIES IV: TIMBRE OF STANDARD TONES

Klünder (11 and 12), Cameron (4) and Sokolowsky (22) in their researches used organ tones for standards, while Berlage (2) made use of tones from the voice. Having ourselves used tuning forks it seemed advisable to ascertain if timbre differences in the standards affect the accuracy of reproduction.

The standards selected for the test were: a large disc tuning fork (144 v.d.) sounded before a resonator, the dichord (137.5 v.d.) energized by bowing, and an organ pipe blown by mouth. In using the latter, because of the variability of the blow and hence the uncertainty of the pitch sounded, the vibration frequency of each standard tone was read on the tonoscope and entered in a parallel column with the reproductions. The tones were so far as possible of uniform intensity, they were sounded for approximately 2 seconds and after the interval of 1 second reproduced on *a*, as in "law" with medium volume of voice. Twenty trials were made on each standard, and because the effect of timbre was the point of interest, the reproductions were in groups of five successive trials, the standard of course being sounded before each attempt.

TABLE V. Timbre of standard tones and accuracy

	Fork 144 v.d			String 137.5 v.d			Pipe Av. 150 v.d.		
O.	E.	m.v.	C.E.	E.	m.v.	C.E.	E.	m.v.	C.E.
P.	2.5	.6	+2.5	1.8	.5	—1.9	.9	.4	— .1
S.	1.0	.9	— .8	1.9	.8	—1.9	1.1	.5	— .3
Ma.	1.2	1.3	+1.2	1.2	.5	+1.2	1.3	.8	+ .6
V.Z.	1.3	.5	+1.2	2.1	.4	—1.9	.6	.5	— .5
Mi.	2.0	.6	+2.0	.6	.5	+ .3	.5	.4	— .2
Av. E.	1.6			1.5			.9		
Av. m.v.		.8			.5			.5	
Av. C.E.			1.5			1.4			.3
G.C.E.			+1.2			— .8			— .1

The results of this series of experiments are summarized in Table V. Judging by the magnitude of the average error and the constant error, the record is in favor of the organ pipe. This is probably due to the fact that this tone is most nearly like that of the human voice in tone-color, or timbre. The introspections of our observers, all of whom have good musical ability and were practiced in observation, are however not in accord with this. Four of the five stated that the string was the easiest standard to imitate. P, the one professional musician in the group, felt that he did best on the fork. But reference to the table shows that it was here that he made his largest errors and even the largest made by any observer on that standard. S. stated that the string was by far the best as a standard but made his smallest errors, and the smallest made by anyone, on the fork. It must be noted also that S. has had more practice with forks than any other member of the group. Practice

is undoubtedly a factor and the value of it for a particular observer depends chiefly on what associations are awakened by a given tone-color. Purity, for example, may be thought of as thinness, and secondarily as highness of tone. While tuning forks, being relatively pure and free from over tones, are at a disadvantage on the side of richness, it is also true that in most groups the observers are about equally unpracticed in singing with forks, which is an advantage from the standpoint of measurement. The forks also are decidedly more constant in pitch than any other type of standard tone. Two of the observers noticed a tendency to imitate the timber of the standards.

From the above observations it seems fair to conclude that richness of tone favors accuracy in the reproduction of any particular standard.[11]

EXPERIMENTS SERIES V: VOWEL QUALITY AND ACCURACY

Berlage (2) introduced the problem of the influence of vowel quality (or change in the timbre of the singing voice) upon accuracy in imitating pitch, and made measurements on this point for the purpose of determining the effect of mouth resonance upon the pitch of the reproductions. In considering the problem here there is no thought of discrediting the results found by Berlage. The tonoscope method of recording has enabled us to take many more records than were used by him in computing his results and the matter is of such far reaching importance that it seemed worth while to include in our study a series on this factor, limiting our measurements to the following vowels:

u as oo in "toot"
o as o in "no"
a as in "ah"
e as e in "there"
i as i in "machine"

In addition to these, humming the tone was introduced in the test as the "hum" seemed to have no marked vowel quality.

[11] Starch (23 p. 52) in his conclusions on the effect of timbre in the localization of sound makes this statement: "The richer and more complex a sound the more accurately it can be localized."

TABLE VI. Vowel quality of the voice and accuracy

	u	o	a	e	i	"h"	u	o	a	e	i	"h"	u	o	a	e	i	"h"
O.	E. m.v. C.E.		144 v.d.				E. m.v. C.E.		182 v.d.				E. m.v. C.E.		240 v.d.			
P.	.6	.6	.4	.8	.6	.5	.7	1.6	1.4	1.5	.5	.4	2.6	1.9	2.0	1.2	4.0	2.4
	.4	.6	.4	.5	.7	.6	.8	1.0	.8	.8	.5	.4	1.2	1.5	1.0	.7	.4	.8
	− .2	+ .1	− .2	0	+ .4	− .3	− .5	−1.4	−1.4	−1.5	0	− .4	+2.6	+1.4	+2.0	+1.2	+4.0	+2.4
S.	1.0	.8	.8	1.1	.9	.9	.9	.9	1.0	.9	1.3	1.0	1.4	1.4	1.4	1.5	1.4	1.3
	.7	.8	.5	.8	.6	.8	.7	.9	.7	.8	.8	.8	1.2	1.0	1.2	1.2	1.0	1.1
	− .9	− .1	+ .6	+ .8	+ .4	+ .7	− .9	− .8	− .7	− .6	−1.2	−1.3	− .7	− .6	−1.1	− .1	+ .7	+ .6
C.	.7	.9	.9	.8	2.2	1.3	.7	.9	1.1	1.0	1.0	.7	2.5	2.3	2.5	2.6	3.3	2.4
	.6	.4	.7	.8	.5	.6	.7	.7	.9	.9	.6	.7	1.2	1.3	.9	1.1	1.4	1.2
	+ .4	+ .6	+ .5	+ .3	+2.2	+1.1	− .2	− .8	− .6	− .4	+ .9	+ .1	+2.2	+2.1	+2.4	+2.4	+3.0	+2.2
V.Z.	.9	1.0	.5	.4	1.5	.4	.8	1.1	.6	.7	1.5	1.0	2.3	3.3	2.5	2.7	4.0	3.1
	1.1	.7	.5	.3	.4	.4	.4	.9	.5	.6	1.1	.8	1.1	1.7	1.1	1.3	1.6	1.0
	+ .9	− .4	+ .2	+ .3	+1.5	+ .2	− .5	−1.0	− .4	+ .1	+ .8	− .3	+2.0	+2.8	+2.1	+2.4	+4.0	+3.1
Wi.	2.4	1.6	1.9	2.1	4.7	4.2	1.4	1.1	1.3	1.2	1.0	1.0	2.1	1.4	1.1	.9	1.6	.7
	1.0	1.2	.8	.9	.7	.8	1.1	1.0	.9	.6	1.0	.9	1.5	1.4	.9	.8	1.3	.8
	+2.1	+1.5	+1.9	+2.1	+4.7	+4.2	+1.1	− .5	−1.2	+1.2	+ .2	+ .4	+1.4	+ .2	− .6	− .4	+1.4	− .2
Mi.	2.3	1.6	1.3	1.7	1.7	1.8	1.2	1.3	1.2	1.1	1.5	1.1	3.2	2.2	4.5	3.8	4.8	3.7
	1.7	1.5	1.2	1.7	1.6	1.9	1.2	1.3	1.3	1.1	1.5	1.1	2.1	1.5	2.1	1.9	1.8	2.0
	−1.8	− .9	+ .4	+ .3	+ .6	− .7	− .7	− .8	− .3	− .3	+ .5	− .4	+2.6	+2.2	+4.5	+3.8	+4.8	+3.7
Av. E.	1.3	1.1	1.0	1.1	1.9	1.5	1.0	1.1	1.1	1.1	1.1	.9	2.4	2.1	2.3	2.1	3.2	2.3
Av. m.v.	.9	.9	.7	.8	.8	.9	.8	1.0	.8	.8	.9	.8	1.4	1.2	1.2	1.2	1.2	1.2
Av. C.E.	1.0	.6	.6	.6	1.6	.9	.6	.9	.8	.7	.6	.5	1.9	1.5	2.1	1.7	3.0	2.0
G.C.E.	+ .1	+ .1	+ .6	+ .6	+1.6	+ .9	− 3	− .9	− .8	− .7	+ .2	− .3	+1.7	+1.0	+1.5	+1.5	+3.0	+2.0

Three forks of the large disc variety were used as standards, the pitches being: 144, 182 and 240 v.d. and each of these three tones was reproduced to the five vowels and the "hum" twenty times, a total of 360 trials for the individual observer. The test was divided between two equal periods. The order of reproducing was two trials on each fork to each vowel in the double fatigue order, illustrated as follows: 144 to *u,* 182 to *u,* 240 to *u,* pause, 240 to *u,* 182 to *u,* and 144 to *u;* then 240 to *o,* 182 to *o,* etc., followed by 144 to *a,* 182 to *a,* and so on throughout the test, the order of the vowels being *u, o, a, e, i,* and "hum." All standards were presented to the ear for a duration of approximately 2 seconds and an interval of 1 second was allowed before the singing.

That the vowel quality is a factor influencing the accuracy of reproduction is borne out by the results of the series as shown in Table VI. The average error (E) and the mean variation (m.v.) are given merely for an index to the reliability of the record. They are both large as compared with the constant error (C.E.) which is the factor in terms of which we desire to measure the effect of vowel quality on reproduction.

Although there are characteristic differences for the three pitch levels and for the different individual observers, the results in Table VI may be fairly represented by a single curve (Fig. 4). This

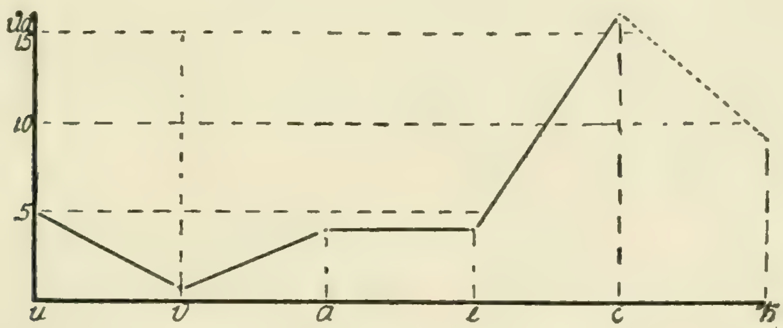

Fig. 4. Vowel quality of the voice and accuracy. (Table VI.)

shows graphically the algebraic average of the records (G. C. E.) for each of the vowels and for the three levels, 144 v.d., 182 v.d. and 240 v.d. There is a tendency for the vowels to fall into three groups: namely, (1) *o* sung the lowest, (2) *a, e* and possibly *u* sung moderately sharp and (3) *i* sung decidedly sharp. These facts would seem to point to the general conclusion that the higher the dominating overtone in a vowel clang, the higher that vowel will be sung. In Fig. 4, *u* offers the single exception to that rule.

The hum was supposed to be neutral as it was moderately weak and the record was made from the nasal breath. This assumption is confirmed by the record which gives the hum a middle place with *a* and *e*.

It must be remembered that there is, in the foregoing records which were sung on *a,* a tendency to sharp by about the amount of sharp for *a* here. That tendency is probably due to some other cause than timbre. It may therefore be suggested that *a* and *e,* the vowels usually sung when one is free, are fairly neutral; *o* (and possibly *u*) are sung relatively flat and *i* relatively sharp. This view, it will be observed, is confirmed by the hum.

Our results seem to differ radically from those of Berlage (4), second part of item 4, in the observations which are common to both. But our method also was radically different; moreover, his conclusion (item 4) is somewhat modified when we read in his article p. 76 where the results of the vowel experiments are discussed. "Accordingly one may look upon a slight increase in the variable error as probable with vowel change" (*i.e.* when the observer tries to reproduce his own pitch but on a different vowel) . . . "other generalities cannot be deduced for the table . . . "

These results in reference to vowel quality are of so far reaching significance for speech and song that we may not venture further discussion for the matter must be made a special object of investigation for verification of the empirical data and in search of an interpretation. It seems safe however to proceed in our work using *"a"* as the vowel quality for reproductions.

EXPERIMENTS SERIES VI: ACCURACY IN SINGING

Having gained some insight concerning the influence of voice range, standard tone intensity, voice volume, standard tone timbre, and voice timbre, on the accuracy of voice control, we now turn to the main problems of our research. These may be restated as follows: (1) What is the average error of the human voice in reproducing the pitch of a tone? (2) What is the average minimal producible change of the voice? (3) Is there any general tendency to sing sharp or flat? (4) How does the average performance of men and women compare on the above three points? All the studies referred to in the historical account contain results which cast light on some of these problems. But in almost every case these

results and problems are secondary to the main interest of the study; and moreover the number of observers and observations is usually quite limited. In Series VI therefore, we have made these problems the central issue on a large group of persons to give our results significance as norms.

Apparatus and method

Standards. With the aid of the tonoscope, eleven large disc forks were tuned to the following pitches: 128, 128.5, 129, 130, 131, 133, 136, 140, 145, 151, and 158 v.d. The series of pitch increments between the forks was therefore: .5, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v.d. as measured from 128 v.d. This series of tones was used for men. For the women a second set was provided on 256 v.d. as a basis, namely, 256, 256.5, 257, 258, 259, 261, 264, 268, 273, 279, and 286 v.d. In this second set it will be noted that the same pitch increment (absolute) were used as in the 128 v.d. set instead of the relatively equal increments. In this respect the procedure was based upon the conclusions reached in Series I.

Koenig resonators were provided for each set of forks. As the increments were small it was found that one resonator would speak sufficiently well to several tones. In the case of the 128 set three resonators were used: first, 128 v.d. to and including 136; second, 140 and 145 v.d.; and third, 151 and 158 v.d. For the higher set two resonators were found sufficient: first, 256 v.d. to and including 268 v.d.; and second, 273, 279, and 286 v.d. Both series of forks as reinforced by the resonators gave tones of pleasing quality and medium intensity.

Observers. Two hundred and one individuals, ninety-four men and one hundred and seven women, took the test which is about to be described. This number comprised those enrolled in the elementary psychology courses in the University of Iowa, 1912-1913. Of these about one hundred fifteen were sophomores; the remaining were upperclassmen. None of them had had any practice in this test. Among them were some excellent vocalists and some others who claimed never even to hum or whistle and to have difficulty in recognizing old and familiar tunes if unaccompanied by words. No one was excused because of his inability and no one was selected because of ability, for it was desired in so far as possible to secure what might be considered an average group. A previous lecture on

the measurement of musical capacity had successfully aroused the interest of the observers so that they entered into the experiment with zest, many of them desiring to secure their individual results.

The charge. The instructions were given by word of mouth to each person, although the appointments were so arranged that one observer was present while another was taking the test and so became familiar with the procedure before he actually entered upon it. Supposing the observer to be a man the instructions would be as follows:

"Mr. ———, we have here a series of eleven tuning forks. This one (striking the 128 v.d. and presenting it before the resonator) is c below "middle c", it is a tone of 128 v.d., the lowest tone in the series; we will call it "o". This one (striking and presenting the increment fork 30, 158 v.d.) is considerably higher than o as you easily notice, and is the highest one in the group. These other forks all represent pitches between the two which we have sounded. The test to-day consists in singing these eleven tones one after the other as they are given. They will be presented in pairs. First we will sound the o, the lowest one of the tones; you will listen carefully to it and then sing a tone of the same pitch. Immediately after your singing, the highest tone in the group (30, 158 v.d.) will be sounded; you will listen and sing that one. Then the o will be sounded again and, after you sing it, there will come the next to the highest tone (23, 151 v.d.); and so on we will come down one step at a time always reproducing the o before each of the interval forks. When you have tried all the tones in the series you will go back over them in the reverse order. Simply imitate as nearly as possible the pitch of each tone as it is given, always remembering that the o is the lowest one in the series. Sing all the tones with a natural voice volume and use the vowel *"a"* (a as in "ah") and whenever you feel dissatisfied with any trial ask for a repetition."

Following these instructions, in order to put the observer at ease and to satisfy his curiosity, the experimenter gave a brief explanation and demonstration of reading on the tonoscope.

The test. The forks were presented to the resonator by a helper who gave his attention solely to the task of sounding the tones in the right order and with as nearly uniform intensity and duration as possible. The tones were sounded with medium intensity varying towards the "weak." The observer sat on a high stool or stood at

for some cause or other have been dissatisfied with attempts and desired new trials giving them immediately they would in the new trials unconsciously repeat the identical pitch given before. This same tendency has sometimes been evident even in large and unusual errors which the experimenter might rule out, asking for new trials. In view of these considerations it seemed best in our general test to adopt the principle of many standard tones and no successive trials on the same tone.

The increments between the forks were made small and of varying magnitudes for two reasons: first, in using these small increments we do not complicate our work with the factor of musical intervals, and second, in using a series of small increments we make possible the measurement also of the ability to make faint shadings (sharp or flat) in the pitch of the voice. The selection of increments is arbitrary. These particular steps were chosen because they have been found satisfactory in work with pitch discrimination (21) at the level of 435 v.d. and, as stated before, extensive research by Vance (26) and others shows that pitch discrimination is practically constant in terms of vibration frequency in the middle range of tonal hearing here covered. This is also the ground for making the increments for the women the same number of vibrations instead of the relative parts of a tone, in which case they would have been doubled.

Sounding the two tones. Seashore and Jenner (19) employed the method of "least producible, or minimal, change". The observer sang the standard or a tone at a given interval from it and then reproduced his own reproduction, save that he made it "the least possible" sharp or flat according as the experimenter might direct. While this will undoubtedly become a standard method in extensive work with an observer it is not suited to tests of a single sitting, first, because ability is rapidly improved by practice and, second, because the observer tends to be easily satisfied with his effort. The better way is not to rely on the changing subjective standard of the observer but to provide a series of constant objective increments and give him the opportunity to find his own level as by the method of constant stimuli in lifted weights or pitch discrimination. Such a series has been provided in the standards and increments mentioned above.

Order of standards. Manifestly the standards might be presented

to the observer in any one of a number of different orders. After trying out the matter thoroughly with the help of three good observers we selected the order of presentation above described for the following reasons: (1) to give the tones in pairs (0-30, 0-23, etc.) takes direct advantage of all the latitude which the series provides. Most observers can easily detect the difference 0-30, while many (theoretically about 25 per cent.) would be baffled to find a difference between 23 and 30; (2) to begin with the largest increment and work towards the smallest has the double advantage of establishing confidence in the attitude of the subject and of stimulating effort; (3) to give the increments in a series and in double fatigue order rests the voice from the unusual strain of making the least producible change, and (4) to explain definitely at the beginning of the test that all the increments are in one direction, *i.e.* above the 0, simplifies the problem and puts it more definitely under control than if uncertainty as to change of direction in standards were allowed. The test is therefore not to measure the judgment for direction of pitch difference but the judgment and expression of the amount of pitch difference between two tones. In pitch discrimination it is well known that much depends upon the direction of the expectant attention.[12] And should we present the standards of our test in a chance order we would complicate it exceedingly at the critical point of the smallest increments.

Time intervals. At the very beginning of the test the intention was to allow an interval of 1 second between the breaking off of the standard tone and the singing by the observer. But the method

[12] An idea of the influence of this same source of error operating in the field of singing may be gained from the following illustration. The author in instructing a very fine observer thoughtlessly said, (the error was altogether unintentional); "We have here two forks, the first, 128 v.d., and the other one 3 v.d. higher, 131 v.d. You will please sing them one after the other. I will give the lower one first." Then the forks were presented and reproduced as directed. When we came to the twelfth trial the observer remarked: "I seem to feel strain to bring the 131 v.d. up". In the moment of reflection following this remark the writer recognized that he had made a mistake in instructing the subject, as the so-called "131 v.d." was really 3 v.d. lower than 128 v.d., or 125 v.d. We find in the twelve trials made that the average reproduction of 128 v.d. is 123.6 v.d. while the average pitch given for the supposed 131 v.d. (really 125 v.d.) is 124 v.d. The misunderstanding and therefore expectant attention changed the direction of the reproductions, and brought in much larger constant errors than are usual for this individual. It should also be noted that the errors are minus.

was soon given up as, in this case, cumbersome and unpractical, and furthermore we did not care to complicate our test with the factor of tone memory. (See Berlage (2) and Sokolowsky (22)). The observers in their usual singing with musical instruments make no such perceptible time intervals. They sing with the tones of the instrument, perhaps holding them somewhat longer than is done by the instrument. When the standard has been sounded the attention is centered, the muscles of the larynx almost involuntarily assume a particular tension and it is unnatural to wait for the beating of a metronome or some other signal to begin singing. If the unpracticed observer is told to make his own interval, unless checked up diligently, he will very soon be making intervals that are exceedingly short, if indeed he is not singing simultaneously with the standard tones. The method followed therefore was to sound the forks for approximately 1 second, encouraging the observer to begin his tone during the sounding of the fork and to hold it longer than the fork.

It may be objected that one might sing fairly accurately judging simply on the secondary criterion of beats between his voice and the standard tone. Helmholtz indeed (9 p. 326) suggests this as a convenient method for the singer to use for checking his own accuracy in practice exercises. While it would be possible for a highly practiced observer it can hardly have much influence in our test. The author made it a point to question frequently regarding the way observers judged of their success in reproducing tones and was not able to find any one who knowingly made use of this criterion. It is however quite possible that the roughness of 6 or 8 or more beats per second may occasionally have caused some observer to be dissatisfied with his attempt. But the tonal fluctuations and adjustments which are necessary to bring about a lessening of the frequency of beats between the voice and an outside standard are easily recognized with the tonoscope; no such "finding" process was observed.

Another time interval which must be considered is that between the o (128 or 256 v.d.) and the increment fork of any particular pair. In order that the standards for minimal change of voice may have their greatest value the interval indicated must be as short as possible admitting of a quick, direct comparison of tones; otherwise the test practically resolves itself into the singing of a single

tone. Hence the presentation of the increment fork followed immediately upon the close of the observer's reproduction of o, the subject being encouraged to make the reproduction about 1 second in length. The increment forks were struck while the o was being reproduced. This was, however, no distraction as only a slight blow on the practically noiseless sounder was necessary, and the forks could not be heard until presented before the resonators. Following the reproduction of each increment fork there was a period of about 2.5 seconds before the next sounding of o.

Other factors. In the matter of intensity of standard and intensity and vowel quality of the voice we took direct advantage of our previous work and adopted such conditions as would give the most normal results according to those findings. By the use of resonators at a considerable distance from the observer's ear we found a satisfactory means of controlling the intensity of the standards,[13] while the intensity of the voice had to be judged subjectively and watched by the experimenter. And in the selection of *"ä"* we are using that vowel quality which according to Berlage and our own results affects least the constant error of the reproductions.

Tables of data

The constant error (C. E.) and mean variation (m.v.) were found for the ten trials on each fork of the ten pairs given in the test. These twenty C. E.'s and twenty m.v.'s for each individual tested are embodied in Table VII, which has been divided into two parts, A. and B., for the men and women respectively. In the first column of the table, at the left, are given the numbers which stand for the individual observers. This numbering is in no sense a ranking, but simply for convenience in handling the data and aid in identification. Odd numbers are used throughout to refer to women and even numbers to men. The second column from the left shows the C. E. and m.v. (the latter is under the former) for the ten trials on o. when used in the pair o-30. The same measures for the ten trials on variant (or interval) tone 30 are given in column three, and each of the successive smaller increments are represented in the same manner. The arithmetic averages for the constant error

[13] It is possible that the pitch of a standard is not only varied by its intensity but also by its position when held near the ear.

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(C. E.) and the mean variations (m.v.) on both standard and variant are presented in the two columns headed O. V. (Arithmetic). The algebraic averages for the constant errors (C. E.) on both standard and variant are given in the two O. V. columns at the extreme right of the table.[14]

The consolidated footings for Table VII, section A and B, are given in Table VIII. The top notation is thus the same as in Table VII. A contains the final footings for men and B for women. The footings are set out as follows: Ave. m.v. is the average mean variation for the respective points in terms of vibrations; C. E. % +, the per cent. of individuals who made a constant error in the direction of a sharp; % —, the per cent. of those who flatted; % O, the per cent. of those who made no appreciable constant error in the ten trials; C. E., v.d., the average magnitude in vibrations of the constant errors, without regard to sign; and G. C. E. v.d. the tendency of the constant erors for the group, the algebraic mean. At the right, the grand averages for both groups are presented under the headings designated above.

Comparison of the abilities of men and women

The most striking general feature of these experiments is the the fact that women show the same ability as men, vibration for vibration, although the women sang an octave higher than the men.

The data on which this assertion is based may be traced most readily in the curves, Figs. 5-8, 10. In Fig. 5 C. it is seen that the curves for the average constant errors on the standard as well as on the variant practically coincide. On the standard they are almost straight lines, the variation for the men being from 1.36 v.d. to 1.66 v.d. with an average of 1.54 v.d., while in the case of the women the variation of this measure is from 1.52 v.d. to 1.81 v.d. with an average of 1.65 v.d. The curves for the variants do not come so near coinciding; they are of the same form, but the women have the advantage, their range of C. E. falling between 1.69 v.d. and 6.71 v.d. with an average of 4.86 v.d., while that for the men lies between 2.59 v.d. and 7.15 v.d. with an average of 5.32 v.d. As further confirmation of the fact that the average con-

[14] There would be little gained by placing E., the crude error, in our table as this measure is something of a cross between C. E. and m.v. and serves simply to indicate the distribution of the constant errors.

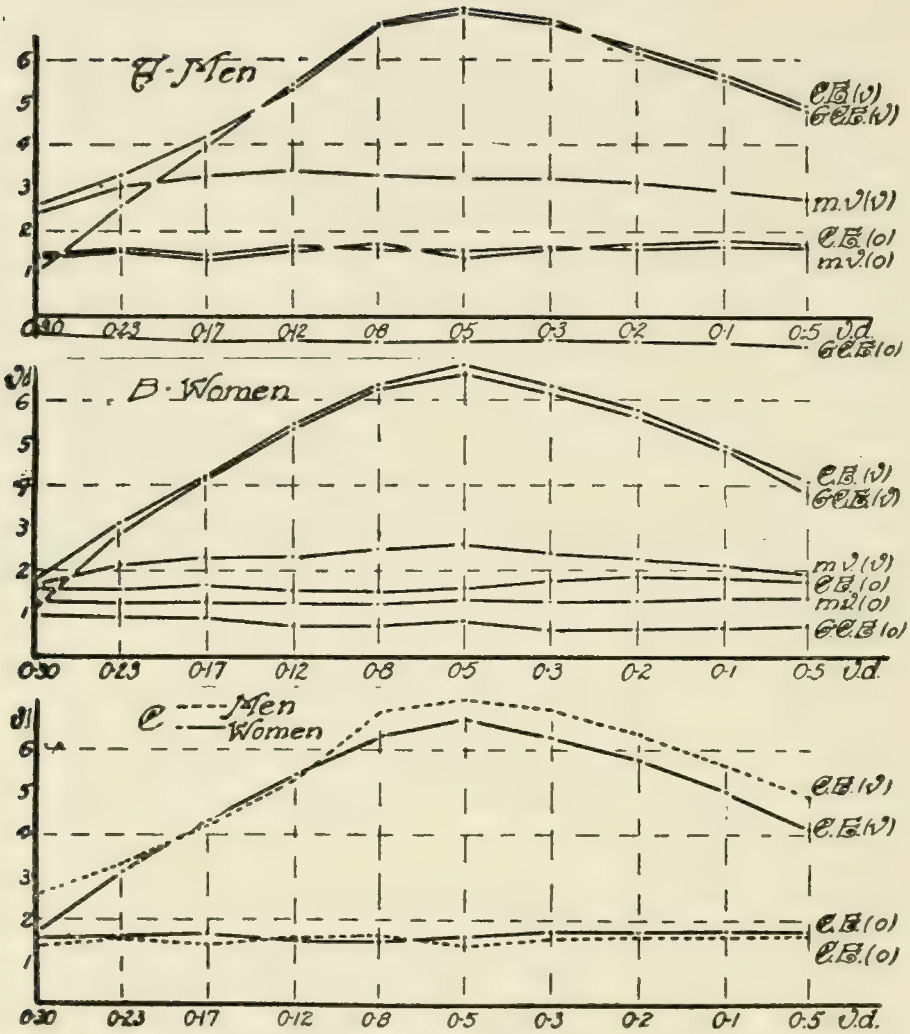

Fig. 5. The data in Table VIII. If there had been no errors all curves would coincide with the base line. The amount of deviation is indicated at the left in terms of vibrations: the increments on the base line. O denotes the standards (128 v.d. for men and 256 v.d. for women); V the variants; C. E. average (arithmetic) constant error; G. C. E. the algebraic constant error or general tendency of the group; and m.v. the mean variation. G. C. E. above the base indicates plus or sharp and below minus or flat.

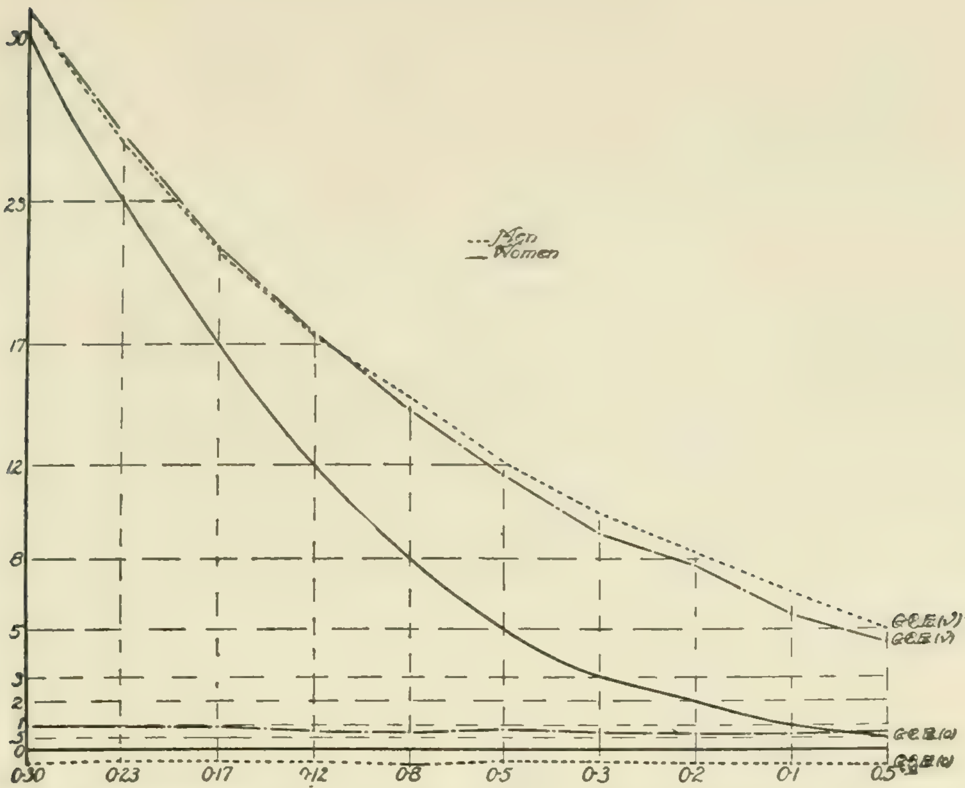

Fig. 6. Intervals as sung. (Table VIII). The distribution of the group constant errors (G. C. E.) for the standards (128 and 256 v.d.) and the variant in each interval. The intervals represented by the forks are shown in the heavy solid curves with which the other curves would coincide were there no errors in singing.

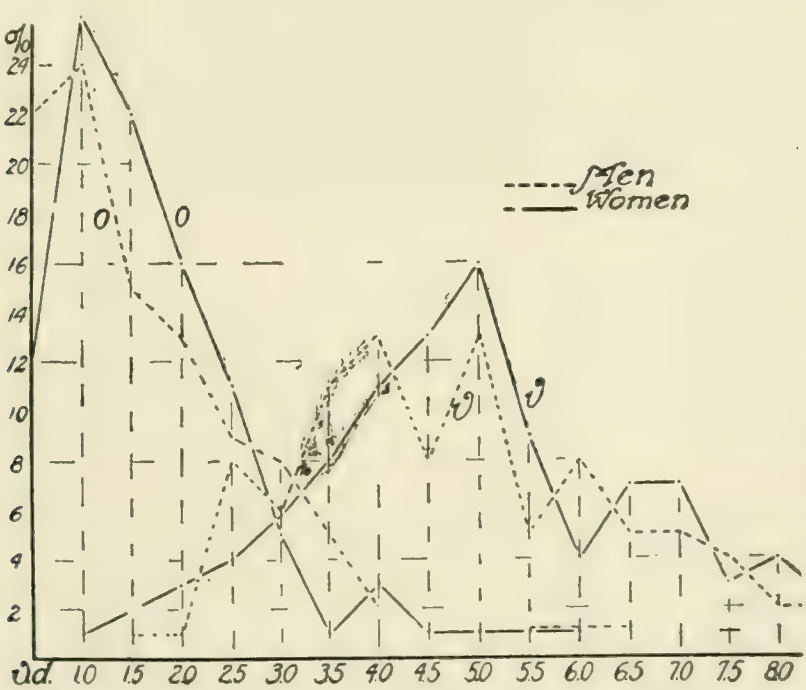

Fig. 7. The distribution of the average constant errors of all intervals for each observer with reference to the magnitude of the error. The data for this figure are found in the columns headed Arithmetic Average in Table VII. O, the standard tone: V, the variant.

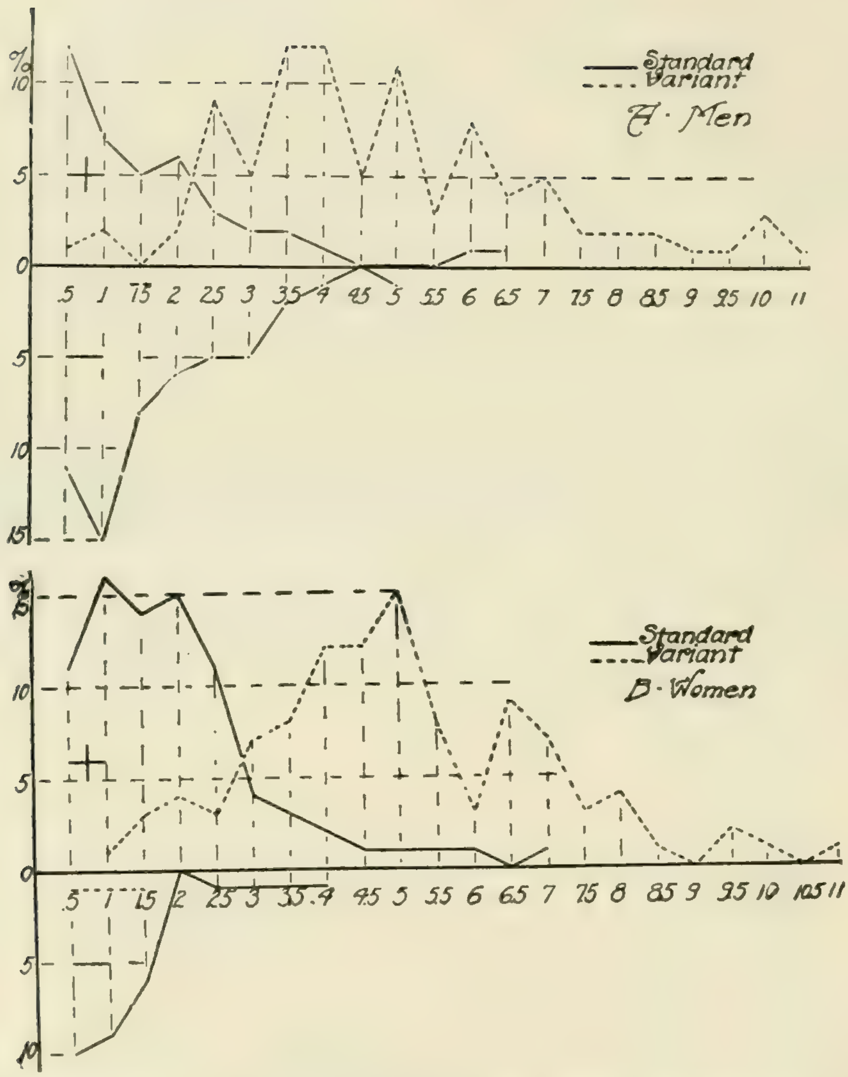

Fig. 8. Distribution of constant error, flat being denoted by — below the base and sharp by + above.

stant errors for both men and women represent approximately equal magnitudes attention is called to Fig. 7 in which is presented the distribution of the average constant errors of all intervals for each observer with reference to the magnitude of the error. The men have a slightly better record on the O. but the women have a more than compensating advantage on the V.

A corresponding agreement in the records for men and women is seen also in the constant tendency for the group (G. C. E. Fig. 5 A and B, 6, and 8 A and B). While the women tend to sharp and the men to flat on the standard (see Fig. 6) the amount is not far from equal in the two cases. (Cf. Table VIII, 65 per cent. of men flat on O while 67 per cent. of women sharp). In view of the general tendency of both men and women to sharp on the variant this difference in the tendency on the standard gives an advantage to the women as regards accuracy in the singing of the interval. An advantage which amounts to an average of over 2.0 v.d.

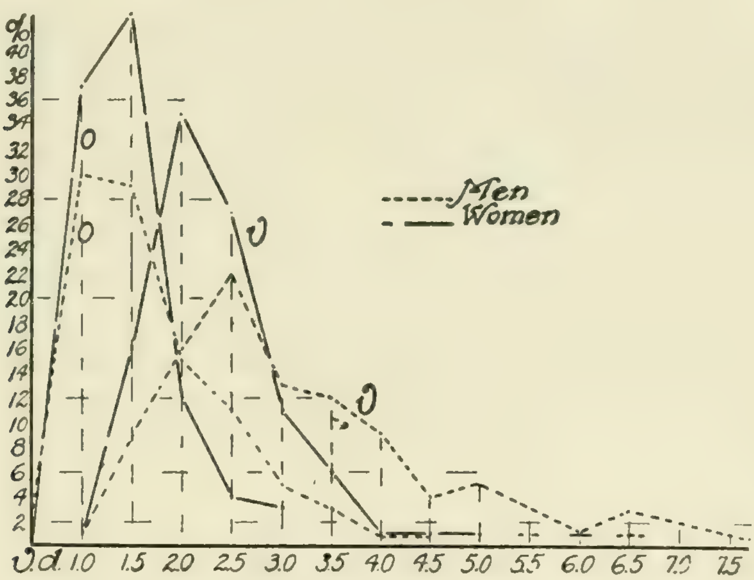

Fig. 9. The distribution of the mean variation (m.v.) for individuals (Table VII, average m.v. at right) with reference to the magnitude of the variations. O, m.v. of the standard tone; V, m.v. of the variant.

In the mean variation (Figs. 5 and 9), which is an important criterion, the advantage is more clearly in favor of the women, particularly in the singing of the variant. There are more men than women with a relatively large variation: but the mode in the case of O is slightly better for the men than for the women. The averages

of the men (Table VIII) are 1.54 and 3.05 v.d. as against 1.29 and 2.21 v.d. for women.

Taking all the data into account the general balance of all scores results practically in a draw:[15] men and women sing with equal accuracy (in terms of number of vibrations of error) although the former sing at 128 v.d. and the latter at 256 v.d. If on the other hand we count the error in relative parts of a tone instead of vibration for vibration, the women sing twice as accurately as the men. It may, however, be shown that the former statement represents the more logical point of view.

This result is in harmony with the results found in Series I with reference to accuracy within the tonal range. It was there found that so long as the singer was certainly within his natural range the man could sing the two tones here considered, 128 v.d. and 256 v.d., with nearly equal accuracy, in terms of vibrations and that, therefore, he tended to sing the higher twice as accurately as the lower. The difference here discussed is therefore not peculiarly a sex difference, but distinctly a matter of psycho-physic law of voice control within the tonal range. Men and women have equal ability in pitch discrimination (reference 21 p. 44), so also in voice control they have equal ability level for level within the tonal range. The fact however remains that women's voices are pitched in a higher register than men's voices and therefore, from the musical point of view, they can sing their tones relatively more accurately.

This result is, after all what we should expect for the principal limit upon accuracy in singing is accuracy in hearing and we know that both men and women can hear a difference of, *e.g.*, 1 v.d. as easily at 256 v.d. as at 128 v.d.

The mean variation

Fig. 5 shows that the mean variation is larger for the variants than for the standards. This is because the former are more difficult. It should be noted that this difference in the mean variation is a measure of the relative difficulty of the two tones as felt

[15] The following facts are significant: (1) there are fewer poor observers among the women; (2) women have smaller mean variations than men; and (3) women more nearly reproduce the intervals. It seems quite likely that in a mixed college group such as we have here, the women give more attention to vocal music than do the men, which may account for their superiority in this test.

and would also be a measure of the relative degree of accuracy in the singing of them were it not for the operation of the two motives for sharping the variant about the middle of the series of the increments. The fact that the mean variation is unaffected by the operation of these two motives is an indication of their fairly rigid operation.

The constant error

Figs. 5 and 6 show that the singing of the standard tone is not affected by the magnitude of the increment to be sung. The constant error is small and uniform. This is due partly to the fact that the standard tone was the same in all trials and therefore tended to become more or less automatic, and partly to the fact that the standard was sung first and that therefore the difficulty in marking off the interval would tend to crop out in the variant tone.

The singing of the variant follows the law that (1) all these small increments are overestimated and that (2) this overestimation increases gradually from the largest interval (0-30) and reaches a maximum in the cases of both men and women (Fig. 5, A and B) at the 5 v.d. interval from which it gradually again diminishes.

There are probably several motives operating to produce this overestimation; the fact that the maximum falls in the increment 5 v.d. points to a relationship between the hearing and the singing of the interval. The median for the least perceptible difference in pitch for this same group of individuals falls on 3 v.d. The increment 5 v.d. in singing would therefore represent one of the smallest increments actually heard. The distribution around this would be analogous to the distribution of the records in pitch discrimination for this group.

It is probable that, as in visual perception of space, all small angles are overestimated, there is in hearing of pitch a tendency to overestimate the smallest increments perceived. If we represent the uniformly increasing series of increments of pitch difference as a sharp wedge the apparent magnitude would be represented by a wedge blunted and thickened.

The operation of such a principle has been demonstrated for hearing in the matter of localization of sound. Starch (23) found that when a correction is made for the least perceptible change in the direction of the source this correction is always overdone.

The lack of fine control of the voice to reproduce the smallest

differences that are heard is another element involved. This factor is partly due to lack of knowledge and practice in this kind of voice control. The small differences which are actually heard larger than they really are, are sung still larger on account of this general lack of control for the making of fine shadings in pitch. This overdoing of a difference may perhaps be regarded as another phase of the same principle as the overestimation of small differences in pitch in hearing. At any rate the enlarging of the small discriminated increments is without doubt much increased in the singing. These small increments are overestimated in hearing (when heard) and are again overdone in the singing; and that this enlarging is proportionate up to the threshold for pitch discrimination.

In applying these principles to the interpretation of the relative magnitude of the errors in the singing of these increments we must bear in mind that where the small differences are not heard there would be a tendency to repeat the standard in trying to sing the variant—this happens not only because the difference is not heard, but even when an effort is made to sing an imperceptible sharp theoretically known to exist there is a tendency for the voice to "fall into the groove" of the standard tone which has been sung immediately before.

On the other hand it seems reasonable to take account of the fact that in this test we are asking the observer to do something with which he is almost entirely unfamiliar. In the larger intervals he recognizes differences but overestimates and oversings them. This overestimation increases regularly from the largest interval, 0-30, to 0-5, as was above noted. At 0-3 most of the observers fail to hear the difference because the conditions of the test do not provide the immediately successive presentation which is most favorable for the discrimination of pitch differences. Therefore, at 0-3 failing to hear the second fork higher, recognizing that he has not yet reached the smallest possible interval, and knowing that the second fork is higher than the O, our observer concentrates his attention, trying harder and harder until the last interval is sung. He is in large measure freed from the factor of overestimation in hearing for he hears no difference. He will very likely tell you that the forks sound just alike, but he knows and is reminded that the second one of each pair is higher. This knowledge forms the basis of his control of the voice. Quite naturally under the circum-

stances he resorts to the tendency (noted above) to take his cue for the second tone not from the fork but from his own previous tone. He "falls into the groove", however, just long enough to get his bearings, then sharps from this point, the magnitude of the sharp being governed roughly by the subject's pitch discrimination ability. In about 8 per cent. of the individual records of 0-.5 the records on the .5 v.d. are not sharp or may be slightly flat; in other words, the observers took the risk of making no sharp.

Applying these factors in the interpretation of the error in the singing of these small intervals of different magnitude, we find that, (1) the average overestimation is relatively small for the smallest increments because in many cases the difference is not heard and in singing a very small interval the voice uses its previous reproduction as the standard, sharping from it, and (2) the overestimation of the small increment is greatest for the smallest increments perceived and gradually diminishes as the increments grow larger so that it tends to disappear on the average when the magnitude of a half-tone is reached. Therefore, our test seems to have met the conditions for measuring the minimal producible change in the pitch of the voice. The increments from 0-30 to 0-5 serve to work down the voice, to make clear to the observer what is to be done, and to center his attention for most careful control. The four smaller increments, 0-3 to 0-.5 are the place where the "ability to make faint shadings" is really tested and under usual conditions the reproductions on the smallest increment, 0-.5, would seem to give the best measure.

If from the records on 0-.5 (algebraic C. E. or G. C. E.) we compute the magnitude of the smallest interval as *actually produced* by the individual observers and distribute these magnitudes according to their frequency, we have the curves of Fig. 12. The median value of the measures represented in Fig. 12 is 4.0 v.d. for women and 4.5 v.d. for men. There are more extremely poor observers among the men so that the average smallest intervals produced are 5.6 v.d. and 3.7 v.d. for men and women respectively.[16] These median values are in harmony with the results for pitch discrimination and may be taken as measures of the ability to produce minimal changes sharp or flat in the pitch of the voice.

[16] A part of this difference between men and women is to be accounted for in the fact that on the average the men flatted the O.

Dr. D. A. Anderson made a test on "minimal change in the pitch of the voice" in the Iowa Psychological Laboratory in 1909. His observers imitated the pitch of one standard fork and then sang the tone the least possible sharp or flat according as directed, making ten successive trials in each direction. There were 115 women and 65 men in the group tested. From the unpublished results of this test we learn that the average minimal producible change for men was 5.5 v.d. and for women 4.6 v.d as against 5.6 v.d. and 3.7 v.d. in our test. In comparing these results it must however be noted that 45 of Professor Anderson's poorest observers, most of them men, made no records which entered into his averages.

Seashore (19) reports the results of some tests of "minimal producible change" given to a small group of observers. The average records for six men on five successive days are as follows; 3.4, 3.5, 3.0, 2.6, and 2.7 v.d. Evidently the factor of practice entered here. However, the average of these results, which represents the only other available data on this ability in voice control, falls on the mode of our curve (Fig. 12) for men.

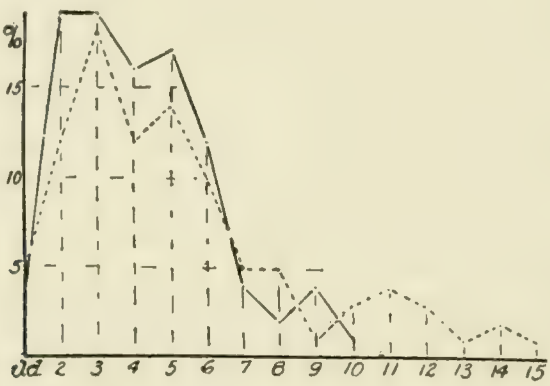

Fig. 10. Distribution of the magnitudes of the smallest interval actually produced by men and women. The method of computing the average magnitude of the smallest interval produced by each observer is illustrated in the following example: if the C. E. on O is —.9 v.d. and on V is +1.4 v.d. then the produced interval would equal the difference between —.9 v.d. and +1.4 v.d. plus .5 v.d. (the real step between the forks) = 2.8 v.d. If the C. E. on O is plus it is of course subtracted from the sum of C.E. on V and .5 v.d. Men dotted line; women broken line.

The average constant error (C. E.) on the standard is small and uniform, as is also the mean variation and the constant tendency for the group, (G. C. E. on the standard). Accuracy in the standard is not influenced by any difference in the magnitude of the in-

crements. This is chiefly because the standard tone was sung before the variant was sounded, and partly because a sort of "rut" was formed for the singing of the repeated standard.

The researches previously reviewed contain scattered measures on this ability. Klünder (1) found that he could reproduce an organ tone of 128 v.d. with an average crude error of .47 v.d. He rejected however the records of some other observers who showed larger errors. Cameron (4) worked with seven observers and tried a number of organ tones. The records by three of these observers gave an average error of about 6.6 v.d. Berlage (2), whose three observers reproduced voice tones, does not give the pitch of the standards. The average error for the three men, singing with an interval of from 1 second to 2 seconds, is .50 v.d.[17] Seashore (19) gives 1.2 v.d. as the average error of 100 trials by each of six men, on standard 100 v.d. Sokolowsky (22) with his seven professional singers finds an average error of 1 v.d. at the average pitch of 251 v.d.

The group constant error

Throughout the previous pages there have been references to the tendency of both men and women to sing sharp when reproducing a tone. The difference in the direction of this error in the standard for men and for women is so constant that, while small, it points to some motive in the character of the tone, the mode of singing, or some tendency characteristic of a given pitch level. The distribution seen in Figs. 7 and 8 shows that the sharps and the flats are not far from equal both in the number and the magnitude for men; for women the sharps predominate in both magnitude and number.

Cameron (4) noticed this tendency and called attention to it. In his experiments it appeared especially in sustained tones.[18] We have not worked with sustained tones but have found the same

[17] Berlage's tables are needlessly complicated by his using the signs with opposite from the usual meaning.

[18] Berlage (2) did not find this tendency to sharp and was surprised, but we must remember that he worked with voice tones for standards (the richest tone possible) and our experiments seem to show that with rich standard tones the sharping of the constant error is considerably decreased. Sokolowsky's (22) results are also negative as regards any general tendency for both sexes to sing sharp. The errors on the twenty tones sung by women, however, *show an algebraic average of +1.03 v.d.,* although eleven of these tones were sung flat.

tendency with reproductions of one and two seconds in length. Reference to our tables (G. C. E.) will show that almost without exception sharping is the predominant direction of the constant errors in all six series of our experiments. The tendency to sing sharp is not materially affected by the level of the pitch so long as the tone remains within the range of the voice; it is increased by loud volume of voice, weak volume of standard, certain vowel formants such as are found in "e" and "i", and by purity of the standard tones.

The best cases

The question naturally arises, to what extent the presence of a few cases of very large error affect the averages. To cast some light on this and also to gain an idea of the performance of the best observers in the group the author made a selection of twenty-five persons of each sex. The selection was made chiefly on the basis of a small Ave. m.v. in the standard (0). The size of the Ave. C. E. of 0 and the Ave. m.v. for the increments, were used as secondary criteria. There are some records, for example N. 9, which from the standpoint of the constant errors alone are very near the ideal curves, but because of rather large mean variations must be omitted from these selected groups. The selection of women was as follows: Nos. 1, 3, 13, 15, 21, 55, 61, 63, 77, 85, 93, 97, 105, 107, 113, 117, 125, 153, 159, 169, 177, 181, 183, 201, and 209. The men's records chosen were: Nos: 6, 8, 10, 12, 16, 28, 50, 62, 68, 72, 82, 88, 102, 106, 110, 114, 120, 126, 128, 144, 146, 148, 154, 156, and 164.

The separate tabulation of these fifty supposedly best cases reveals the presence of the same general tendencies in these selected groups as have been noted in the large groups, with the difference that they are not so pronounced and that here the men in a relative comparison make a better showing than the women, in that their overestimation especially of the smaller pitch increments is less. Therefore blame for the large errors (overestimation of intervals) can hardly be shifted to a few individuals as indeed we might have shown by referring to Figs. 7 and 8 which demonstrate that the distribution of the errors forms fairly normal frequency curves.

Correlation of singing with pitch discrimination

Pitch discrimination records are available for eighty-two of the men, and one hundred and four of the women who acted as observers

in our tests. The well-known formula of the Pearson "Product-Moments" was employed and resulted in the following correlations:

			r	P.E.r
Men:	Size of ave. smallest interval produced	with Pitch. Disc.	+.21	.072
	" " " m.v. on O.	" " "	+.04	.074
	" " " C.E. on O.	" " "	+.08	.074
	" " " m.v. on V.	" " "	+.33	.066
	" " " C.E. on V.	" " "	+.15	.073
Women:	" " " smallest interval produced	" " "	—.11	.065
	" " " m.v. on O.	" " "	+.27	.061
	" " " C.E. on O.	" " "	+.11	.065
	" " " m.v. on V.	" " "	+.51	.048
	" " " C.E. on V.	" " "	—.07	.065

It will be recalled that in order to be satisfactory a coefficient "should be perhaps three to five times as large" as its probable error. This rule liberally applied to our results leaves us the coefficients +.33 and +.51 both of unquestionable reliability. These coefficients represent the correlation between pitch discrimination and the average mean variation in singing the intervals, for men and women respectively.

The test of 1910

A series of musical tests, given by the writer in the Iowa Psychological Laboratory, during November and December of 1910, included one on Accuracy in Reproducing Tones. There were ninety men and one hundred and seven women, members of the elementary psychology classes who took this test.

The apparatus besides the tonoscope consisted of five large forks with pitches as follows: 128, 256, 320, 384, and 512 v.d. The experimenter instructed the observer to take the 256 v.d. fork, strike it gently, bring it to his ear, listen carefully, and then to reproduce the same pitch. This he repeated with fork 256 v.d. Then taking the 320 v.d. fork he proceeded as described. The last four forks were gone over five times in this manner, which gives ten trials on each tone, forty trials in all. The test is thus very simple, the reproduction of four tones (two successive trials on each) which are at the same time natural musical intervals: major third, fifth and octave. No restrictions were placed upon the observer in the matter of humming or singing with the standards. As the fork was in his hand he sang with it or after it as seemed best to him. About one-half the observers preferred to take the fork away from the ear before beginning to sing. The men sang the tones one octave below

the pitch of the fork. When it was difficult for them to commence doing this, the 128 v.d. standard was used for orientation.

The results for this series of 8,000 reproductions are given in Table IX. The notation is the same as in Table VIII. The test of 1910 was complicated by the factor of natural musical intervals, it was also considerably shorter and simpler than the one of 1913 but in comparing it with the latter we find the results in practical agreement on some points.

(1). There is a uniform tendency for the majority of observers to sing sharp. Here again the tendency appears to be greater for women than for men, the G. C. E. for men being + .26 v.d., for

TABLE IX. Accuracy of singing: test of 1910

90 men	128 v.d.	160 v.d.	192 v.d.	256 v.d.
Ave. m.v.	1.42	1.36	1.43	1.79
% + C. E.	47	63	51	63
% — C. E.	53	37	49	37
Av. C. E.	1.62	1.62	1.72	2.70
G. C. E.	+ .26	+ .65	— .06	+1.06

107 women	256 v.d.	320 v.d.	384 v.d.	512 v.d.
Ave. m.v.	1.89	2.15	2.07	2.83
% + C. E.	81	90	84	86
% — C. E.	19	10	16	14
Av. C. E.	2.59	3.91	3.47	5.90
G.C.E.	+2.39	+3.36	+3.11	+4.76

women + 2.39 v.d., a difference of 2.13 v.d. as contrasted with 1.30 v.d. in the previous measurements. In the test of 1910, as mentioned, the men and women used the same forks, the men singing the standards one octave low. Therefore the tendency for men to sing less sharp than women in the 1913 experiments can hardly be attributed to a timbre or sound volume difference between the sets of forks. The men are much more evenly divided between the sharping and flatting tendencies than the women, for example on 256 v.d. the one tone which both sexes had in common, the percentages in favor of sharping are 63 and 86 for men and women respectively. (2) The average constant error (arithmetic) on 128 v.d. is slightly larger in 1910, 1.62 v.d. as against 1.54 v.d. The mean variation for 128 v.d. are 1.42 v.d. (1910) and 1.54 v.d. These differences are rather slight. (3) Men and women sing their one common tone (256 v.d.)

with equal accuracy: m.v. 1.79 v.d., Av. C. E., 2.70 v. d. (men) to m.v., 1.89 v.d., Av. C.E. 2.59 (women). It would seem from a comparison of available norms for voice range in the sexes (Helmholtz (9) and Zahm (27) that 256 v.d. should be about as high for men as it is low for women, and that it is well within the average range of both. We have here therefore a confirmation of our previous conclusion, *i.e.*, that men and women sing with equal accuracy vibration for vibration. However the errors in this case under consideration (1910) are much larger than the results of Series VI would lead us to expect. This is true of all the tones sung by the women and renders them incomparable with the previous results.

Recommendations toward a standard test

The recommendations which follow must be considered as preliminary and as applying simply to the two measures of singing ability considered throughout this study, *i.e.*, the ability of the voice to reproduce pitch, and the ability to produce voluntarily small changes sharp or flat in the pitch of the voice.

1. The two factors may be tested together with advantage. They are closely related phases of the same thing. Neither of them should be taken in combination with such factors as accuracy of tone memory, or judgment for musical intervals.

2. Use a graded series of standard tones similar to that commonly employed in testing for pitch discrimination. Such a series has obvious advantages over the use of a single standard; (1) If several observations are to be made at a single sitting the effects of practice are not so great. (2) The small pitch intervals make clear to the observer what he is expected to do with his voice. (3) The variety of standards (and hence degrees of difficulty) reduce monotony and fatigue. A graded series furthermore has advantage over any other series: (1) it keeps the test comparatively free from complication with the singing of musical intervals, and (2) when the standards represent small steps of pitch difference the observer discriminates more carefully and is not so likely to be satisfied with a mere approximation.

3. Use tuning forks for standards. They are very easily manipulated, are not subject to certain sources of error commonly met in the control of reeds, pipes and strings, and are readily arranged

into a graded series as recommended above. Any disadvantage, if indeed it may be so called, from the standpoint of the purity of the fork tone seems more than compensated for in having a definable quality and a standard on which all observers are equally unpracticed.

4. Begin with the largest pitch increments and proceed to the smallest and then in reverse order back to the largest. This will economize effort, provide the best practice, and help to control the attention. For general testing ten intervals representing as many degrees of difficulty, ranging from 0-30 to 0-.5 are not too many. For extensive testing of one observer or in working with highly practiced observers the increments which are distinctly above the threshold for pitch discrimination may be omitted.

5. Give the tones in pairs, presenting the variant tone immediately after the reproduction of the standard, thus securing a rapid adjustment which favors discrimination in the kinaesthetic sensations from the larynx. As an alternative procedure the two tones might be presented in immediate succession as in the pitch discrimination tests, the observer carrying the standard in mind while listening to the variant, and then singing them in quick succession.

6. Control conditions: (1) The forks should be presented before resonators which are some distance from the observer and care must be exercised to present them with uniform intensity. (2) The observer should use a medium volume of voice in singing the tones, (3) The experimenter should select the vowel to be sung and insist on a good quality. (4) If time intervals are used between standards and reproductions they should be short, not longer than two seconds at most. (5) Time intervals should be introduced between pairs of tones. These should be at least 2 seconds in length. Longer intervals would doubtless be better as the voice could the more easily be kept out of a "rut" in reproducing the standard. (6) Secure effort on the part of the observer who is too easily satisfied with his own performance.

Our test is one of motor control. As a musical test it bears the same relation to the motor side as pitch discrimination does to the sensory side. In fact it is in a practical way the motor pitch discrimination of the singer, and as far as singing is concerned it is more important than simple sensory pitch discrimination.

SUMMARY OF CONCLUSIONS

Among others the following general conclusions may be gleaned from the foregoing experiments.

1. The human voice is about equally accurate, in terms of vibration, at all points well within its range; therefore, the high tones are sung relatively (per cent.) more exactly than those which are low.

2. A strong standard tone (especially with low forks) is reproduced as decidedly lower than a weak standard.

3. The voice can most easily reproduce pitch for those standard tones which have a rich timbre, such as the organ tone.

4. Measured in terms of average error the voice is less accurate when its volume is large.

5. Vowel quality affects the accuracy of vocal reproduction of tones. The *"i"* (as i in machine) is reproduced the highest, *"o"* the lowest, and *"a"* occupies a middle position.

6. Men and women sing in their representative ranges with equal accuracy vibration for vibration of error.

7. Women show better relative voice control than men, if judged on the basis of their mean variation.

8. With women there is a general tendency to sing sharp. Men are about equally divided in this regard, sharping however being slightly more frequent.

9. The average error of the voice in reproducing a tone given by a fork is 1.5 v.d. for men at range 128 v.d., and 1.5. v.d. for women at 256 v.d. in a representative group of students.

10. A small perceptible pitch difference between two tones is overestimated in the signing.

11. The average minimal producible change of the voice for men at 128 v.d. is about 5.5 v.d., and for women at 256 v.d. it is 3.5 v.d.

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THE EFFECT OF TRAINING IN PITCH DISCRIMINATION

BY

FRANKLIN ORION SMITH

CONTENTS

The present investigation forms a part of a series of researches in the Iowa laboratory,[1] upon the tonal hearing. The problem was to determine the effects of training in tonal hearing, considering age, sex, musical education, general intelligence, and kinship.

The investigation consisted of a preliminary training series, a ten days' practice series and the correlation of the results with those of other researches. The experiments were conducted in the University and in the public schools of Iowa City and Cedar Rapids in 1908-1912.

Method of procedure

The tuning forks and accessories which were employed in this research are fully described by Professor Seashore in his report for the American Psychological Association on the standardizing of pitch discrimination tests.[2] The experimental precautions, both subjective and objective, were observed as set forth in that report. The only change made in the apparatus consisted in using two resonators instead of one, which is a decided improvement because one resonator alone does not speak sufficiently well at the extremes where increments as large as 23 or 30 v.d. are used. The methods of procedure recommended in the above named report were followed, as

[1] The writer wishes to acknowledge his manifold indebtedness to Professor Seashore for his supervision and coöperation, which have made this research possible. To Dr. Mabel C. Williams and to companions in research who are working upon related problems in the laboratory, he expresses his grateful appreciation for assistance.

[2] Psychol. Monog. No. 53.

described on pages 39-43 of that report. The "heterogeneous" method was used in all preliminary experiments and with unclassified groups. This consists in presenting the increments 30, 23, 17, 12, 8, 5, 3, 2, 1, and .5 v.d. in the order named a number of times and finding at what level in that series the threshold falls in from ten to twenty trials. The mean variation of the records for all such sets is then computed by the method described on page 42 of the above named report, as follows:

"For ordinary work we therefore recommend as a measure of variation in the record the use of the mean variation (m.v.) computed as follows: Regard the difference between successive steps as equal psycho-physic steps and, with the increment which is nearest to the median as a base, multiply the number of cases which are one step from this base by 1, the number that are two steps away by 2, the number that are three steps away by 3, etc.: divide the sum of these products by the total number of cases (sets)."

The homogeneous method is the ordinary method of right and wrong cases or constant stimuli, counting the threshold at 75 per cent. correct cases. This method was used in dealing with individuals or groups formed on the basis of preliminary tests.

The preliminary training consisted of two tests which are designated as the first and second preliminary tests respectively. The observers consisted of pupils in the elementary and high schools, and students in the University. The ages vary from nine years to maturity. Most of the observers were unmusical in the sense that they had received no special training in music. These tests were made in the schoolrooms under good conditions. The temperature and ventilation were regulated by automatic systems (except in two small grade schools). The regular teacher remained in the room during the experiment maintaining normal conditions of order and school spirit. These general conditions did not differ materially among the schools nor among the different rooms of the same school. The tests were carried on in the morning between nine and twelve o'clock, each test lasting twenty to twenty-five minutes.

Since it was not practicable in all cases to employ the homogeneous method, all the group tests were made by the heterogeneous method. In figuring the results the nearest whole vibration (except 0.5 v.d.) was taken. The increments in the series of tones used (0.5, 1, 2, 3, 5, 8, 12, 17, 23 and 30 v.d.) are referred to as units and are con-

sidered equally difficult to distinguish. That is, 23 to 30 v.d. is assumed to be as difficult for one whose threshold is 23 v.d. as 1 to 2 v.d. is for one whose threshold is 1 v.d.

In case of defective hearing the pupil was seated where he would be certain to hear; or, if the deafness was serious, he was excused from the test. The rhythm of the work period was not so easily controlled. The tests were comparatively short and every effort was made both by the experimenter and the attending teacher to keep the effort up at a high pitch throughout the test. Indifference is perhaps the largest source of error in the few cases where it was manifest. This could be recognized directly at the time of the test and usually also by the distribution of errors in the records.

One of the most striking and yet perplexing facts about pitch discrimination is that there is often no relation between the feeling of certainty and the correctness of the judgment. The judgment is often based upon a clear illusion. This illusion of hearing in the case of wrong judgment aids much in the encouragement to sustained effort.

Anticipatory judging is a fruitful source of errors. Under the influence of expectant attention the observer anticipates the second tone the moment he hears the first. The experience is analogous to the illusion of lifted weights. With a strong expectation of hearing the second tone high, or low, the organism is set to make the appropriate response and this has marked influence upon the judgment. Closely related to anticipatory judging is the tendency to compare the present tone with the preceding pair. In fact this tendency often leads to anticipatory judging especially when the first tone of the present pair is compared immediately with the last tone of the preceding pair.

The confusion of pitch and intensity is a troublesome source of error, particularly with unpracticed observers. Making the tones actually objectively equal in intensity does not always allay the difficulty as disturbing associations may tend to create confusion. High tones are intrinsically louder than low tones. A slight difference in intensity is often interpreted as a difference in pitch.

In computing the characteristic figure of a record it is necessary to take account of internal evidences and make a "correction" as is explained in the report of this test referred to above, pages 45-48. This must always be a matter of "good judgment" and can not be

done mechanically. Certain factors may however be quite clear and exact. The distribution of the records in the heterogeneous test with respect to the operation of the laws of chance is one of the most telling. A record of, *e.g.*, 8 v.d. may on examination of the distribution of the errors be found to contain indisputable proof of a threshold of 2, or 1, or .5 v.d. as the case may be.

Sometimes when a source of error has been noted a study of the distribution may show where it operated and where it did not operate. A small mean variation, *e.g.*, 1.0 or less is almost certain proof of the reliabliity of the actually computed median. The study of the internal evidences therefore has its principal use in cases showing a large mean variation. All our records were examined with reference to internal evidence of error in the computed median and, it must be frankly admitted, wherever such evidence was found the correction was made. All the records here used in the heterogeneous method are therefore "corrected" records. Fig. 1 shows, it will be seen, that the tendency of the correction is to lower the record and that most of the corrections are made for those who have poor records.

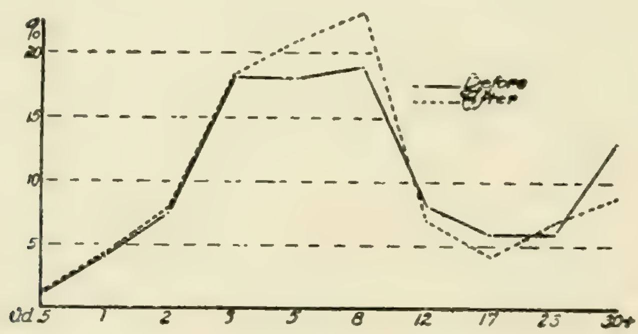


Fig. 1. Distribution of 476 pupils for one day's practice before and after the records had been corrected.

The effect of instruction

As a preliminary to the training series two tests of about 25 minutes each were given to 1980 pupils by the heterogeneous method in their regular class divisions. In the first period the test was begun without any explanation beyond what was necessary to direct them about reporting "higher" or "lower". The second period was opened with simple and diversified explanations and illustrations of what pitch is. This explanation was based upon a previous study of the kinds of difficulties encountered. Pitch was differentiated

from intensity, duration, volume, timbre, etc. in familiar talk and by different instruments.

Unfortunately the two factors of instruction and experience, or direct observation resulting in a growing familiarity with the problem, are not isolated. We have simply the records for the two periods and must interpret the gain as due to both of these factors, which are, of course, inseparably associated.

To facilitate comparison the observers were divided on the basis of these tests, into A, B, and C grades in accordance with the possession of a good, medium, or poor ear. Grade A includes those who

TABLE I. Distribution of those who improved in the preliminary test

	30	*23*	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*	*A*	*B*
30+	11	11	10	15	6	3	2	1	0	0	59	
	23	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*			
30	13	13	11	17	7	3	0	0	0		64	11
	17	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*				
23	13	12	15	2	3	0	0	0			45	24
	12	*8*	*5*	*3*	*2*	*1*	*0.5*					
17	17	38	14	4	1	1	1				76	36
	8	*5*	*3*	*2*	*1*	*0.5*						
12	25	30	16	3	1						75	55
	5	*3*	*2*	*1*	*0.5*							
8	103	43	15	10	0						171	101
	3	*2*	*1*	*0.5*								
5	126	35	8	8							177	159
	2	*1*	*0.5*									
3	122	45	13								180	197
	1	*0.5*										
2	44	7									51	177
	0.5											
1	9										9	147

Italics designate increments; the other figures give the number of cases for each of the respective degrees of improvement; thus, of those who had a record of 30+ in the first test, 11 went to 30—, 11 to 23, 10 to 17, 15 to 20, 6 to 8, 3 to 5, 2 to 3, and 1 to 2 in the second test. *A* shows the total number of cases at each increment in the first test; *B* same in the second test.

hear differences of less than 3 v.d.; grade B those who hear differences of 3 to 14 v.d.; and grade C those who hear differences of 14 to 30 v.d. or above.

The records show that 54 per cent. made no improvement in the second test; 46 per cent. of all observers made better records in the second preliminary test than in the first. The amount gained varies from 1 to 8 units. The average amount gained varies from 3.8 v.d. at nine years of age to 0.3 v.d. at maturity.

Table I analyzes the distribution and the amount of gain by the

cases (46 per cent.) which improved with the instruction. Of the 46 per cent. who improved, only 7 per cent. changed from grade C to grade A in the second test. Of the 425 pupils (22 per cent.) who improved and were in grade B at the beginning, 255 (60 per cent.) changed to grade A in the second test. Measured by the first test 26.5 per cent. of those who improved were in grade A. Measured by the second test 70 per cent. were in grade A. Of the changes to grade A, 96 per cent. were from grade B; and 91 per cent. of the changes to grade B were from grade C.

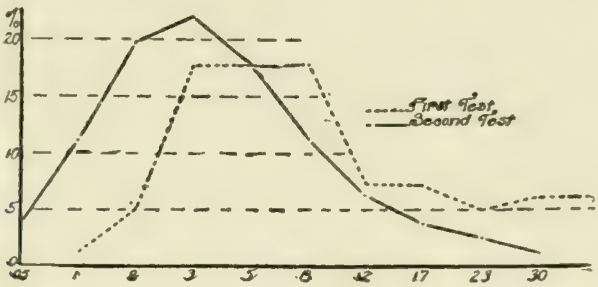


Fig. 2. The effect of instruction. Distribution of 907 pupils who made improvement from the first to the second preliminary test.

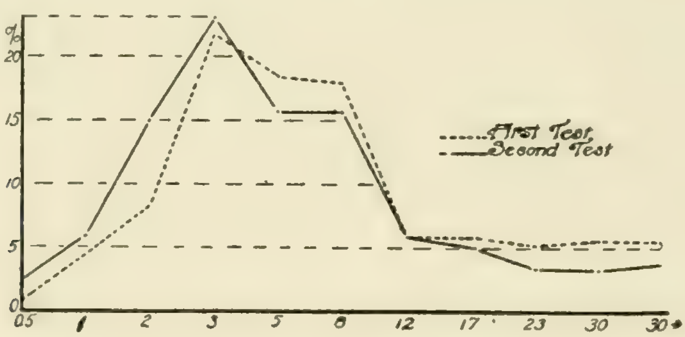


Fig. 3. Distribution of entire group, 1980 cases, in preliminary tests.

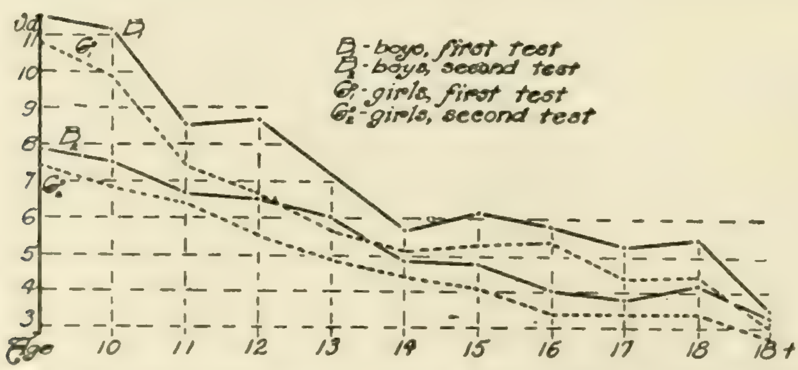


Fig. 4. Distribution of improvement in the preliminary tests by age and sex, 417 boys and 490 girls.

The effect of the instruction and experience thus gained from the first to the second test is shown in Fig. 2 which represents only the 46 per cent. of cases in which improvement was made. Fig. 3 shows the effect upon the whole group of 1980 cases. Fig. 4 shows the distribution of improvement by age and sex.

TABLE II. Distribution of forty-seven out of fifty-four university students who improved with individual instructon

	23	*17*	*12*	*8*	*5*	*3*	*A*	*B*
30	1	1	1	2	1	0	6	
	17	*12*	*8*	*5*	*3*	*2*		
23	0	0	1	1	0	0	2	1
	12	*8*	*5*	*3*	*2*	*1*		
17	0	0	4	1	1	2	8	1
	8	*5*	*3*	*2*	*1*	*0.5*		
12	2	2	1	1	0	2	8	1
	5	*3*	*2*	*1*	*0.5*			
8	3	6	2	1	0		12	5
	3	*2*	*1*	*0.5*				
5	4	1	4	2			11	11
			Below 5					28

Notation and plan of this table same as in Table I.

A similar test of the effect of instruction was made in a class of 200 adults. After two preliminary tests, one heterogeneous and one homogeneous, the poorest one-fourth of the group were taken and instructed individually as to the actual nature of pitch hearing. An effort was made to find out what particular difficulties they were encountering, and explanation and illustration were based progressively upon this information. As a class these had made but little improvement in the second preliminary test, both the first and the second having been given "without instruction". But as a result of this personal instruction all but 7, *i.e.* 47 out of the 54 made rapid improvement. The change in the record for the group is shown in Fig. 5 by giving the distribution at the beginning and at the end of the period of individual instruction. The distribution of the gain is analyzed in Table II.

The fact that these were adults familiar with the class room and trained in many psychological experiments, yet made such marked response to the instruction and individual help, doubly emphasizes the importance of thoroughness and individual attention in the instructions if the records are to be entirely reliable.

One of the best experimental proofs that we have showing the efficacy of individual care and instruction is found in the un-

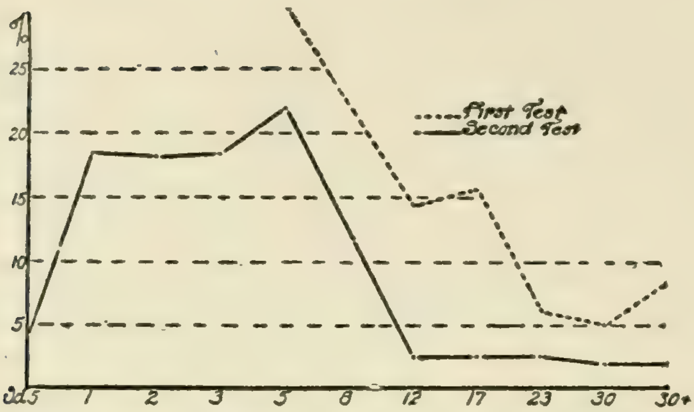


Fig. 5. Distribution of 54 university students in individual tests (Table II).

published experiments of Dr. H. S. Buffum, which have been summarized in the above mentioned Psychological Association report by Professor Seashore, as follows:

Dr. Buffum experimented on twenty-five eighth grade pupils in a grammar school room. He first made a fifteen minute individual test of each pupil and classified them on this basis into three groups with modes at 3, 8, and 17 v.d. respectively. The object was two-fold: (1) to determine the effect of practice and (2) to determine the success of the preliminary examination. For this purpose he gave them twenty forty-five-minute periods of training.

The results show (1) that for no group is there any evidence of improvement with this practice, and (2) that all except two children remained throughout the whole practice series within the group to which they had been assigned. Of these two, one who had been assigned to group III was immediately found to belong to group I as there had been a failure to understand the preliminary test; and the other, although retained in group II, proved really to be near the dividing line and could have been classified in group III. Evidently the physiological threshold had been reached in twenty-four of the twenty-five cases in the preliminary test."

In Dr. Buffum's experiment the fifteen-minute preliminary classification was so efficient as practically to eliminate poor records due to ignorance of the test.

The significance of instruction is further proved by the records in successive classes in the university for a period of years. It is found that the average record has improved slightly from year to year. There is no reason for believing that this is due to anything

but improved skill and technique and increased care in the instructions and charge to those about to be examined.

In the above records we have conclusive evidence that effective instruction is of the greatest importance in making records on pitch discrimination. It is not a poor ear, but ignorance that accounts for the bulk of poor records in a first test. Those who made a fine record in the first test are, of course, not subject to this source of error; and those who have poor records but show no improvement after instruction or prolonged training may also be free from this source of error. It is a safe rule to say that all tests should be preceded by efficient instruction; if this can be made individual, so much the better; and all who show poor records must be subjected to more intensive and searching instruction before the record can be accepted for serious purposes.

The effect of practice

The first of the two extensive experiments in practice was a series of group tests by the "heterogeneous" method covering a period of ten days. The second was a series of individual tests on adults by the "homogeneous" method. In addition to these certain special training series will be described.

The group tests were made on 476 pupils (215 boys and 261 girls) in two elementary schools selected from those in which the preliminary tests had been made. These practice tests were conducted in the same manner and under the same conditions as the preliminary tests except with regard to instruction. Each test was preceded by a brief warming-up exercise in which the pupils answered orally. This also helped to keep interest alive. A short rest period was taken at the middle of each test. At this time opportunity was given the pupils to ask questions about the test.

Running parallel with the class tests were certain individual tests which were carried on in the afternoon following a given set of class tests. At the noon intermission the records of one or two grades were checked up and pupils whose threshold for that day was between 20 v.d. and 30 v.d. were given individual practice. The object of these individual tests was to give special assistance to backward pupils, aiding them to distinguish different tone qualities and to form right habits of attention. These tests include 71 boys and 35 girls constituting the poorest in the group tests.

With regard to the general musical preparation of these pupils it may be said that music was taught systematically in all the grades, and that the schools were provided with Victor graphophones in which high grade selections were played regularly.

For comparison the cases under observation may be divided as follows: Group I, those who made no improvement either with instruction or practice; Group II, those who made no improvement in the practice; Group III, those who made little (1-3 v.d.) improvement in the practice; and Group IV, those who made marked improvement (3 v.d. +).

The records of these practice series on children are set forth in Tables III-IX and Figs. 6-10. Table III gives the daily average threshold for the twelve days of training by ages, section A showing those who do not improve with training and B those who do improve with training. Table IV gives the daily average threshold for those who improve with training regardless of age for the four

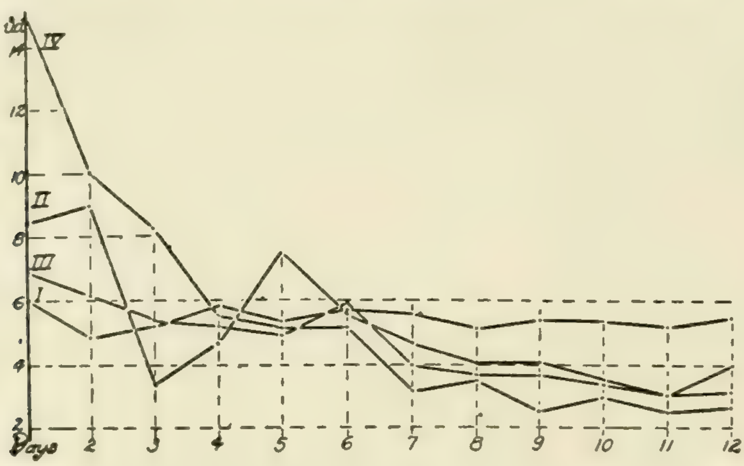


Fig. 6. Daily average, by groups, of those in the practice series (Table VI).

Fig. 7. Daily average by sex (Table V).

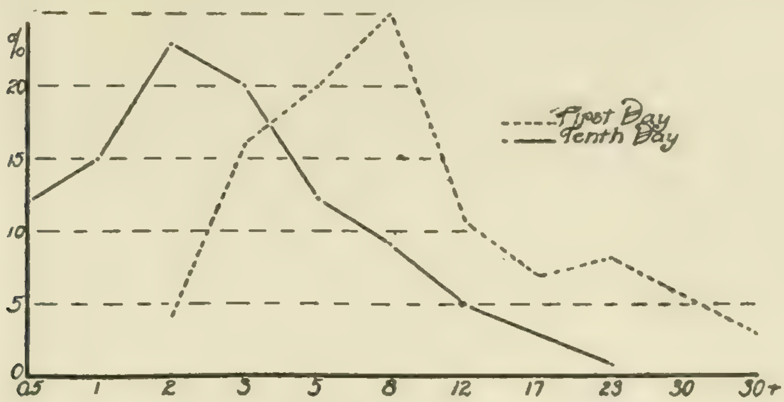


Fig. 8. Distribution of 270 pupils who improved with practice (Table VI).

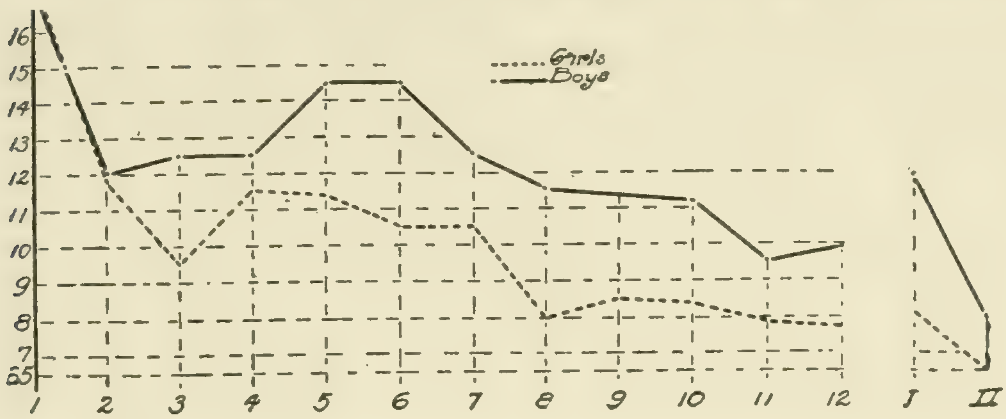


Fig. 9. Daily average record of those who were given special individual help (Table VII).

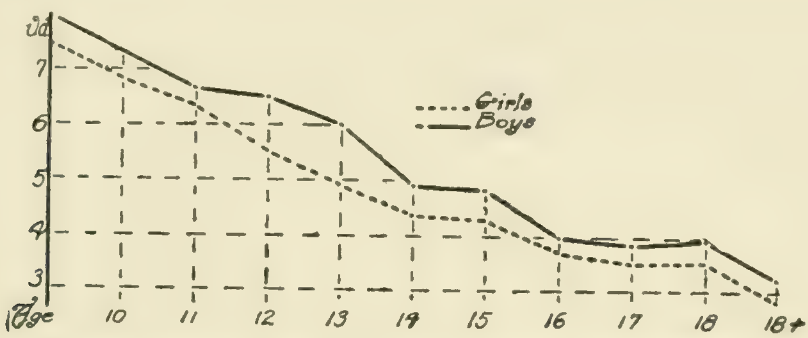


Fig. 10. Comparison by ages of the average (median) abilities of boys and girls.

groups. Table V gives the averages of the same separately for the boys and girls, Groups II, III, and IV combined. Table VI gives an analysis of the distribution of those who improve with practice. Table VII (Fig. 9) contains the record of those who were given individual tests or help during the practice, showing the daily record and the record of two individual tests in the average for the group. Table VIII gives a comparison of the mean variations with reference to sex and age. Table IX gives the distribution of those who attain the approximate physiological threshold in different days of the practice.

TABLE III. Daily average threshold, by age

A. Group I: those who made no improvement with training

Age	1	2	3	4	5	6	7	8	9	10	11	12	Number
9	6	6	6	6	8	·7	8	10	9	8	7	8	29
10	6	6	4	7	8	8	6	7	7	8	6	8	31
11	8	4	6	7	5	7	6	7	5	6	7	8	47
12	8	8	8	5	7	8	7	7	8	8	8	5	23
13	5	5	5	5	4	5	5	5	4	4	5	5	29
14	6	5	5	5	4	4	5	4	4	4	4	5	32
15	5	6	5	5	4	4	4	4	4	4	4	4	15

B. Groups II, III, IV combined: those who made improvement

Age	1	2	3	4	5	6	7	8	9	10	11	12	Number
9	7	5	5	5	4	4	4	4	4	3	3	2	16
10	7	7	5	5	4	4	5	3	3	3	3	3	43
11	10	10	8	7	8	5	5	5	6	5	4	5	43
12	7	5	4	3	3	3	2	2	2	2	2	2	53
13	8	8	5	5	6	4	4	4	3	2	2	2	51
14	7	7	5	5	5	5	4	3	4	3	3	3	34
15	7	8	7	5	5	5	4	3	3	3	4	3	30
Ave.	7.5	7.1	5.7	5.0	5.0	4.3	4.1	3.4	3.5	3.0	3.0	2.9	
% gain		9	30	15	0	15	4	15	0	9	0	2	

TABLE IV. Daily average by groups

	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	Number
Group I	5.9	4.8	5.2	5.8	5.1	5.5	5.4	5.0	5.4	5.2	5.1	5.4	206
Group II	8.5	9.0	3.3	4.6	7.6	5.3	4.7	4.0	4.0	3.5	3.0	5.0	52
Group III	6.9	6.2	5.4	5.1	5.0	5.8	4.5	3.5	3.5	3.4	3.1	3.0	172
Group IV	15.0	10.0	8.2	5.5	5.1	5.1	3.1	3.4	2.5	2.9	2.6	2.7	46

TABLE V. (Fig. 7) Daily average by sex for Groups II, III, and IV

	Number	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*
Boys:	215	8.1	6.5	6.3	5.8	5.6	5.2	5.3	5.2	5.0	4.6	4.6	4.7
Girls:	261	6.3	5.3	5.2	5.2	4.9	4.8	4.7	4.7	4.5	4.0	3.5	4.5

TABLE VI. Distribution of those who improve with practice

	30	*23*	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*	*A*	*B*
30+	0	1	1	2	2	1	0	0	1	0	8	0
	23	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*			
30	2	3	2	4	1	2	1	1	0		16	0
	17	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*				
23	4	5	4	4	2	1	1	0			21	3
	12	*8*	5	*3*	*2*	*1*	*0.5*					
17	5	7	3	2	1	0	0				18	8
	8	*5*	*3*	*2*	*1*	*0.5*						
12	8	10	8	3	1	1					31	14
	5	*3*	*2*	*1*	*0.5*							
8	14	26	15	6	6						67	25
	3	*2*	*1*	*0.5*								
5	15	26	10	4							55	33
	2	*1*	*0.5*									
3	12	16	14								42	55
	1	*0.5*										
2	4	8									12	59
						Below *2*						73

Notation and plan of this table same as in Table I.

TABLE VII. Daily average record of those who were given special individual help

Days	1	2	3	4	5	6	7	8	9	10	11	12		
Boys:	17.3	12	12.5	12.5	14.5	15.5	12.5	11.4	11.3	11.2	9.5	9.8	*12.*	*8.*
Girls:	17.7	11.8	9.5	11.5	11.4	10.5	10.5	8.	8.5	8.4	7.9	7.8	*8.2*	*6.5*

Italics, average record on the first and the second individual tests respectively.

TABLE VIII. Average mean variation from the individual records in the preliminary and final tests.

	Boys (215)		Girls (261)	
Age	Prelim.	Final	Prelim.	Final
9	1.82	1.93	1.99	1.97
10	1.66	1.81	1.76	1.60
11	1.63	1.71	1.68	1.82
12	1.70	1.53	1.51	1.68
13	1.47	1.52	1.54	1.60
14	1.45	1.48	1.65	1.65
15	1.64	1.59	1.53	1.38
Total	1.61	1.65	1.65	1.69

TABLE IX. Distribution of those who reach the approximate physiological threshold on different days of practice.

Days	1	2	3	4	5	6	7	8	9	10
Per cent.	6	8	9	9	9	13	12	13	8	7

Of the 476 children 206 (43%) fall in Group I; *i.e.*, so far as the instruction and practice are concerned, these made no improvement that could be traced in the records, due allowance being made for daily variable errors. The number of those who do not improve with practice is relatively greater for the younger than for the older children.

Classifying these 206 on the basis of record into Grade A, those whose threshold is 4 v.d. or less; Grade B, those whose threshold is between 4 v.d. and 14 v.d.; and Grade C, those whose threshold lies above 14 v.d., we find 40 per cent. in Grade A, 51 per cent. in Grade B, and 9 per cent in Grade C. Of the 270 cases (57 per cent.) which show improvement with practice 19 per cent. are in Grade A, 64 per cent. in Grade B and 17 per cent. in Grade C.

Relatively the largest number of cases of improvement occur among those who start out with a very inferior record. This can be shown by comparing the distribution of cases which make improvement for each of the increments as set out in Column A, Table VI with the normal distribution of thresholds for the entire group.

Of those who did not improve ten were unable to hear any of the increments used and judge as required. It was however found upon

making private examination of the seven of these who were available that they could hear tone differences. Two of these could distinguish between A and B on the piano. Two of them seemed unable to grasp the concepts "high" and "low" with reference to the naming of pitch. One of these—a scatter-brain—could, however, sing a half-tone correctly when played on the piano. The other—retarded about five years—could sing a fifth fairly accurately with the piano. Three were able to imitate a pitch difference in the forks of 3 v.d. by singing enough to show whether the second of the two tones was sharp or flat. The other three were, unfortunately, not available for special tests. Thus, of the 476 cases not a single case of so-called tone deafness was found.

The last line in the footings of Table III, B shows that the gain of those who do improve is most rapid in the first part of the training series, 54 per cent. of the gain being made in the first three steps. The further analysis of these figures in Table IV, illustrated by Fig. 6, shows that this principle is true for all three of the groups which show improvement.

All the observers included in Table VII took the first individual test which occurred on different days, from the third to the seventh day. Most of these tests were given early in the practice series. The second test began on the fifth day and extended over the remainder of the practice series. Only 26 boys and 9 girls needed to take this test. A very few were given a third test near the end of the practice but the results were not included in the table. Not only does the individual test yield a lower median than the group test in a majority of cases, but the individual test often influences the later results of group practice. In this experiment 6 boys and 4 girls made immediate and permanent improvement after the first individual test which it will be remembered was accompanied by instruction. In one case the gain was from 30 to 9 v.d.; in another from 23 to 5 v.d. and in a third from 24 to 10 v.d. In some cases improvement did not begin until after the second test, and in the case of 29 boys and 13 girls no improvement was made. Of these only 2 (both girls) made better records in the individual tests.

The average amount of improvement for all cases at each increment decreases with the diminishing of the increment. This is seen in Table VI, and may also be seen graphically in Fig. 8. It must be remembered that this figure does not represent the whole group but only those who improved.

The series is not long enough to guarantee that any or all reached the physiological threshold.[3] The main difficulty in determining this lies in the fact that persons often come to a "plateau" in the record which is due to some motive or condition which may be removed by instruction or training. This, however, gives trouble only when it continues to the end of the training series. Classifying the cases roughly on internal evidences of the records we find that what may be approximately the physiological limit is reached in successive days as set forth in Table IX. From the variations in the records it is quite clear that the data in this table are quite problematical. To get the actual physiological threshold it is necessary to have more favorable conditions for isolation of the observer and the elimination of disturbances. Undoubtedly there may also be several who remain on a "cognitive" plateau throughout this series and would improve under the proper impetus. Yet, due allowance being made for these sources of error, the table shows that there is a "rapid maturing" in this training; 6 per cent. reach their bed-rock level on the first day, 8 per cent. on the second, 9 per cent. on the third, etc.

After the preliminary tests the number who reach the approximate physiological threshold increases gradually to the fourth day. On the fifth day the number increases suddenly from 24 to 41 (9 per cent. to 15 per cent.) and then gradually decreases to the eighth day after which there is a rapid falling off to the tenth day. (Table IX). The results show that 47 per cent. of those who improve reach the approximate physiological threshold by the fifth day of practice.

The mean variation as given in Table VIII conveys three significant items—the result of practice, the variations with age, and the

[3] The term is here used in the sense defined by Seashore (3) page 49-50. "*The Cognitive* vs. *the Physiological Threshold.* In sensory discrimination of this sort we may speak of two thresholds: the physiological, which is set by the limits of capacity in the end organ; and the cognitive, which is set by cognitive limitations. Theoretically we always aim to reach the physiological threshold, but practically we often fall short of this and find a cognitive limit; *i.e.*, a higher threshold due to lack of information, best form of attention, interest, effort, etc.; or to disturbances of some sort. Usually inspection of a record or observations made in the test enable us to tell whether or not we have reached the physological threshold. It cannot be judged by a single rule, although a small m.v. and a well defined mode are pretty sure indications. This distinction is of greatest importance in classification, and in the theory of training."

variations with sex. It must be borne in mind that the unit of the m.v. is not the vibration but the increment, as was described above. That is, the increments increase in a geometric ratio of the second order; therefor, *e.g.*, the increment 17-23 v.d. counts one unit just as do the increments 5-8 v.d. or 1-2 v.d. It follows that as the threshold is lowered the mean variation unit remains relatively constant. Equal power of application of those who have high and those who have low thresholds should therefore show in about equal mean variations; and, conversely, unreliability in judgment will show in increased mean variation equally for the one who has a fine ear and the one who has a poor ear.

The mean variation is slightly larger in the final training test than in the preliminary. The difference is not large—only .04 units—but it is fairly constant for all ages and for both sexes. This is rather remarkable as, in the nature of the test, one would look for evidences of increasing familiarity in the lowering of the mean variation. On the other hand the fact that the procedure does not reduce the mean variation is a most telling proof of the elemental nature of the test. The test is so stripped of conditions for variablity that it is possible to be as consistent in the preliminary trial as in trials after practice.

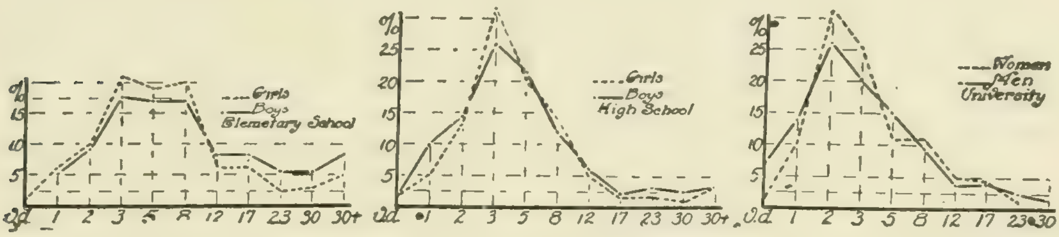
The variablity is a trifle larger for girls, both in the preliminary and the final tests. This is true for all the ages except 12 and 15 in the preliminary and 10 and 15 in the final. Were it not that this has a bearing on the much mooted question of sex difference on this point and that the data here given represent such a large number of cases (1980 in the preliminary and 476 in the final) no significance would be attached to this difference. The second decimal figure is of doubtful value in an average of this kind and, as stated, the variation is in one direction for five ages and in the opposite for two both in the preliminary and the final. On the whole our interpretation is, therefore, that practically there is no significant difference in the variability of the boys and the girls in pitch discrimination.

There is a general, though not great, tendency for the mean variation to decrease with age. This is the measure of growing reliability with age which we are accustomed to find in records of this kind.

In this practice series in the elementary schools there are two

items that count distinctly in favor of the girls. One is that of the 215 boys and 261 girls who took the practice series, 71 boys and 35 girls were considered, on the same basis, poor enough to need individual instruction and drill. This is a distinct mark of superiority in the girls. The other is that the girls in the training series, quite uniformly for all ages, have a lower threshold than the boys by from one to two vibrations. (See Table V, and Fig. 7).

This superiority of the girls over the boys is evidenced also in the preliminary tests with remarkable uniformity as is seen in Fig. 10 where a fairly constant difference is maintained throughout all the ages. The same fact is illustrated from another point of view for the elementary school in Table X and Fig. 11. This difference, however, disappears when we come to the higher ages. Fig. 12, for the high school, and Fig. 13 for the university, based on Table X reveal no recognizable superiority of either sex in the preliminary tests.



Figs. 11, 12, 13. Variation with sex and age. Based on preliminary tests in the elementary schools.

A comparison of pitch discrimination for different ages in the preliminary tests is given in Table X. This shows that the order of superiority is,—university students, high school pupils, and elementary pupils, the respective modes being roughly 2, 3, and 4 v.d. This comparison is however not quite fair, inasmuch as longer tests were given to the university students than to the high school pupils and longer to the high school pupils than to the elementary pupils; and the longer the test the more favorable the results tend to be. As will be shown later, this, together with the better control of experimental condition among the older pupils, may be ample to account for the differences here shown, so that, under equally good conditions of testing, there would probably be no evidence of variation with age.

In Table XI we see that at the age 9 the cases are about equally distributed in the three grades. Grade B remains about

constant for all ages; but the number of cases in Grade A decreases with age as the number of cases in Grade C increases. Fig. 14 shows a comparison for age only.

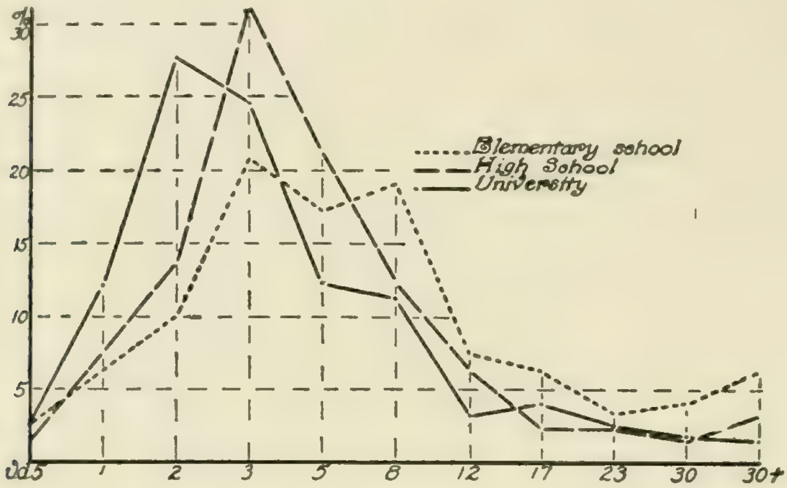


Fig. 14. Comparison of threshold of pitch discrimination for different ages.

TABLE X (Fig. 11, 12, 13 and 14). Variation with age and sex (Preliminary tests).

		Elementary School		High School		University	
	v.d.	*M*	*F*	*M*	*F*	*M*	*F*
Grade C	30+	8	5	3	3	0	0
	30	5	3	2	1	1	0
	23	5	2	3	2	2	1
	17	8	6	2	2	3	4
Grade B	12	8	6	6	5	3	5
	8	17	20	12	14	9	11
	5	17	19	21	20	13	11
Grade A	3	18	22	26	34	22	25
	2	8	10	14	13	26	32
	1	5	6	10	5	14	10
	½	0	1	1	1	7	1

M males; *F* females; numbers indicate the per cent. of cases at each step. A 4 v.d. or less; B between 4 v.d. and 14 v.d.; C above 14 v.d.

TABLE XI. Distribution by age and group in terms of per cent. of cases

Age	9	10	11	12	13	14	15	16	17	18	19+
Group A	30	24	24	16	17	12	13	12	8	4	8
Group B	38	44	40	44	41	44	40	34	34	47	26
Group C	32	32	36	40	42	44	47	54	58	49	66

The comparison of the mean variation for the three groups of ages given in Fig. 15 shows that the reliability of the records of

the high school pupils is practically as good as that of university students. Elementary pupils are slightly inferior but not so much as we would ordinarily find in other tests of discrimination.

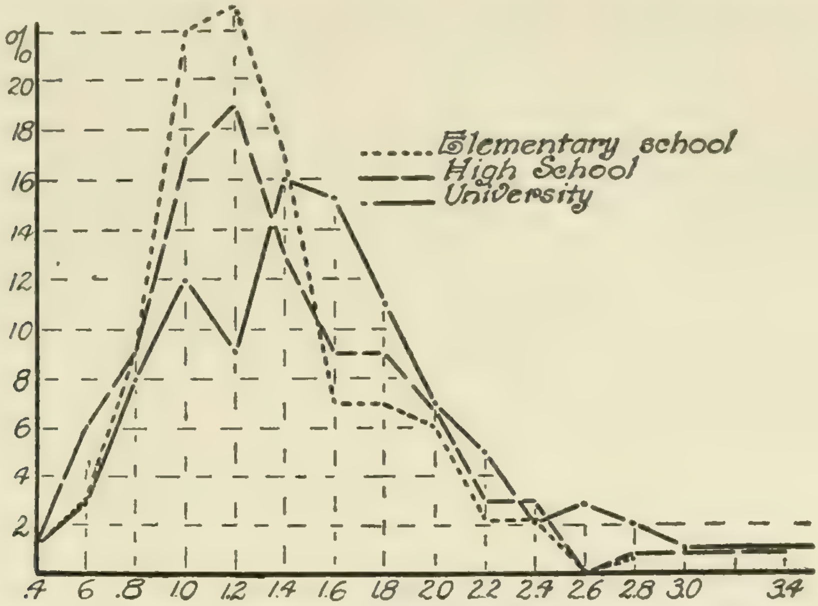


Fig. 15. Comparison of mean variation (m.v.) in the preliminary tests for different ages.

Some records of practice in pitch discrimination have been collected incidentally in this laboratory. The case of C.E.S. is presented (Fig. 16) to illustrate how variable the threshold may be aside from practice. The first practice series of twenty half-hour periods was taken in 1898 with crude methods. No resonator was used, the forks being held to the ear. This, perhaps, introduces the largest source of error in that series. Unfortunately data are not available for determining other causes of the inferiority of this record. Beginning with 1906 the Koenig resonators were used with a good quality of forks. The fact that, from this point on, the record is fairly constant would seem to indicate that the absence of the resonator in the foregoing series was the chief source of error. In 1907 the experimenter was not skilled. In 1910 the tests were made for the purpose of comparing certain conditions of environment, such as the light and sound-proof room, a class room, and out in the open air. From the 43rd to the 48th day a study was made of the effect of the duration of the tone and the time interval between the two tones. On the last four days distractions were introduced. The best record was made while the observer was intentionally tracing a maze.

Something was wrong in 1898. M. C. W. (Fig. 17) made a poor record in the twenty period practice undertaken by the same method and means as in the case of C. E. S. above. In 1908, as soon as the good resonator was introduced, her record was fine and free from fluctuations. She had, however, learned to play the violin and had gained experience in the tuning of forks in the years that elapsed since 1898. Her best records were made with distractions—tracing a maze or crocheting. These records furnish most striking evidence of the importance of reliable apparatus and technique.

In Fig. 18, characteristic results of practice, under most favorable conditions of control are shown; a, b, c, and d are the respective practice curves for four graduate students practicing one hour daily, sixteen days.

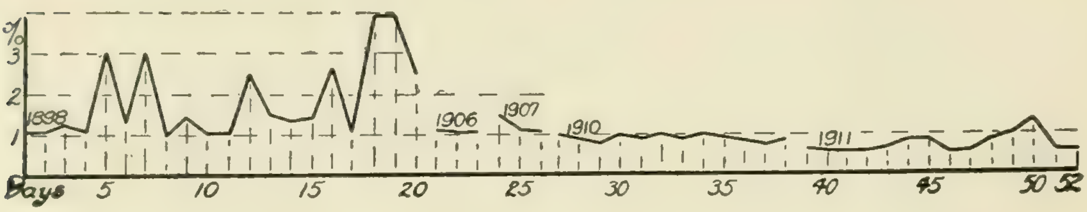


Fig. 16 Record of C. E. S.

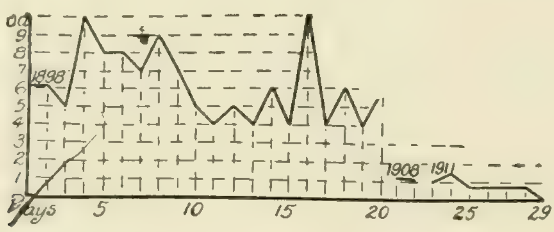


Fig. 17 Record of M. C. W.

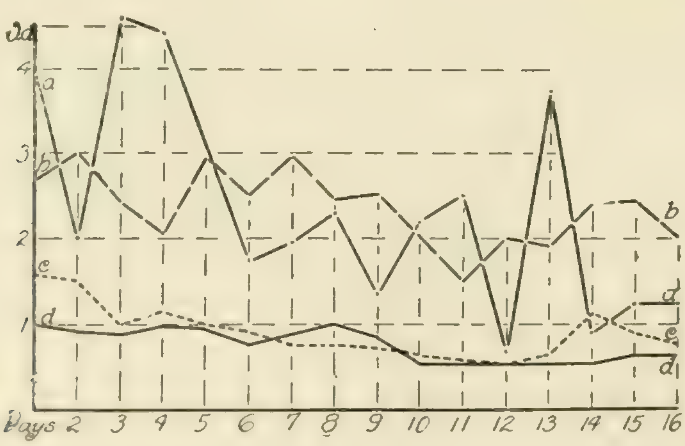


Fig. 18. The effect of training.

Factors in the development of pitch discrimination

Practice in pitch discrimination means (1) the control of a special set of cognitive factors involved in learning to recognize differences in pitch, and (2) the acquisition of skill in listening to musical tones. In most of the recent experiments on practice, such as those made by Book on typewriting, Swift on tossing balls, Bryan and Harter on telegraphy, Judd and others on handwriting, and Huey on reading, the object has been to determine the mode of acquisition of certain special habits. Of these Book distinguishes two sorts, habits of manipulation and habits of control: The latter he defines as certain general or more purely psychic habits acquired in the course of practice. It is to this type of learning that the present analysis is directed. Of these general habits or modes of control we may distinguish three types, (1) sensory control, by which is meant acquaintance with certain sensational facts, such as auditory qualities of the tones, and muscular sensations; (2) associational control, or acquaintance with memory images, as auditory, visual, and motor; and (3) control of special attitudes, as feeling of familiarity, most favorable form of attention, interest, etc.

Auditory and kinaesthetic sensations seem to play the leading rôle in judging differences in pitch. Two types of observers may be distinguished. First, there are those whose perception of pitch is chiefly in terms of tonal qualities. They learn to direct attention to the characteristic sharpness or fineness of the high tones and to the flatness or dullness of the low tones. The particular sensory quality of the tone varies with different persons. One notices that the high tone is sharper, and has a tendency to last longer in the ear than the low tone. Another describes the high tone as finer and more piercing. The lower tone is usually distinguished from the higher as being duller, deeper, heavier, and more mellow. It is also interesting to note that some observers judge altogether by the high tone, while others judge only by the low tone. Some persons seem to have an affective preference for high tones, others for low tones. This forms an apperceptive basis for the judgment.

Second, there is a considerable number who depend largely upon kinaesthetic sensations of the vocal organs in making the judgment. Regarding these Stumpf says: "If the muscular sense in the vocal organs is the same as a former tone that we have heard, we judge that it is the same tone. If we are told that a certain

tone is A, we remember that a tone giving the same sensations is A. If the muscular sense changes in a definite way when we sing two tones, we say that the tones rise. If a distance is noticeable in the change, we judge the second interval to be greater." Stricker did not think of music in terms of notes nor of auditory images, but in terms of muscular sensations in the vocal cords. He speaks of the impossibility of the reproduction of a tone in the memory without bringing into play the actual or intended use of the vocal organs. He considers the connection between tone perception and the innervation of the vocal organs a sort of reflex.

This view is in accord with many introspections in the present investigation. Some of the observers allege that they are not able to tell whether the second tone is higher or lower until they reproduce the tones either audibly or mentally in terms of vocal strain. In one individual test the observer, a university student, was not able to distinguish a smaller difference than 20 v.d. by merely listening to the tones. When he was told to hum the tones, he immediately ran down to 8 v.d. and continued to improve, reaching 2 v.d. Singing seems to enhance the power of discrimination partly on account of the timbre of the voice and partly on account of the motor elements in vocalization. "I carry the first tone over and when I hear the second I hum it to see whether I feel more or less strain in the vocal cords."

These muscular and kinaesthetic sensations are not always confined to the vocal cords. They may start in the vocal cords and spread to other organs; as, for example, "A strain starts in the vocal cords and runs up through my head." The sensations may be initiated in other organs, or they may be felt as general bodily changes. "I feel the tone as a singing in my head." "In case of the high tone, the singing is 'stronger' than in the case of the low." "The high tone seems to make a stronger impression in my ears than the low tone." By impression the observer probably means muscular strain. "I feel the tones as vibrations in the body. They seem to go all through me and cause a sort of strain." "I have a tendency to breathe more deeply for the low tones." "The high tones give me a sense of elation; I seem to mount. The low tones seem to give me an experience of gentle relaxation, a general feeling of calm." "I have a distinct tendency to move up and down according as the second tone is high or low." "Low tones seem to

drag me down; high tones seem to lift me up." "I feel an upward impulse and tend to rise with the high tone." "The mind seems to be a little more tense for high than for low tones." The affective quality of the tone is often the important element in consciousness. "When the low tone follows the high tone it seems to be more pleasing." "The high tones feel different but I can not explain the difference."

In most instances the auditory and kinaesthetic sensations combine into an auditory-vocal perception. Thus the judgment is a highly complex process conditioned by a mass of auditory and muscular sensations. The total result in consciousness, however, is a simple experience, a mark of familiarity which enables the observer to interpret the difference immediately.

In addition to sensory processes the judgment is conditioned by certain associational processes, chiefly auditory and visual images together with certain associations which are built up around these.

Many speak of carrying over the memory of the first tone and comparing the second tone with it through auditory imagery. The clearness of the image, and hence the certainty of the judgment, depends upon whether the interval is short or long. This varies somewhat with different individuals as does also the character of the imagery. Some observers associate certain familiar tones with the image of the present tone. The low tone sounds like the "hoot of an owl." The high tone is associated with the major key and the low with the minor key, or perhaps the observer imagines he hears his own voice singing the tones.

Visual imagery includes localization in space, voluminousness, and color-tone qualities. "The high tone seems to glide up at the end." "High tones seem nearer and low tones farther away." "I have a visual image of a teeter board." "High tones seem to be in the upper part of my head; low tones in the lower part." "I think of ti, do, or do, ti in the musical scale." This observer was unable to describe the tones in terms of auditory imagery. "The high tone appears to be higher up in space than the low." This reply is typical of a large number and seems to play an important rôle in the perception of difference. "The high tone has a swelling, expansive feel in the left ear and seems to have a pull upwards,—a lifting quality—almost to the point of unpleasantness in strength. The lower tone seems to be located in the right of the direction of

the head and below. The high tone is nearer the head, the low tone far away."

In the following case the method of localization is unique. The relative position of the two tones is the reverse of what is usually found. "The low tone appears to be above the high in space. It is also larger so that the two tones would be represented by a heavy above a light line, thus: "

Many observers refer the tones to a musical scale or musical instrument. M. O. thinks how she would play the violin to produce the different tones. H. S. sees her finger move up and down the violin string. O. S. says, "When I think of the second tone as higher I think of it as higher up on the piano." Another says, "I seem to see my fingers moving along the violin string."

M. C. W., a trained psychologist, locates the tone by a peculiar kinaesthetic-visual imagery. The high tones go up to the right and lie in the head, the low tones move down to the left and lie near the left side of the root of the tongue. The first sound is in the aural axis, a little to the right. All are thought of as in the head, though she knows the real source.

"High tones seem long and pointed while low tones seem big and flat." M. describes the interval as a pyramid or cone with the high tone at the apex and the low at the base. For W. "High tones are fine and sharp. They seem thin and compact: I imagine an object contracting. Low tones are relatively rich."

Colored hearing plays an important part in the judgment of some observers. Moritz Katz[4] has reported on color impressions of Schumann, Tieck, Liszt and others. In the present study the following are noted: "The high note seems to be a brighter color, the low darker." "High tones are bright and clear; low tones are dark and murky." M. always thinks of sounds in terms of color. Her impressions are remarkably complex and varied. "When I hear sounds that please me they appear violet. When I am talking with any one whose voice is pleasing, I see violet color. When I listen to a soprano solo I see a section of the rainbow. As the tones become higher they change to bright green and the very highest tones appear like little flames of fire. Low tones are reddish brown. Any rasping or disagreeable sound appears red or brown. When I hear a chorus of mixed voices or an orchestra there seems to be a large mass of violet color and from this on all sides little short

[4] Zeitschr. f. angew. Psychol., 1911, pp. 1-53.

tongues of various shades of green, yellow, and red." She does not remember when she did not translate sounds in this way. L. M., 17 years old, combines spacial and brightness qualities with tones. A very high tone appears to be a bright vertical line. As the pitch is lowered the line grows in width, but diminishes in brightness.

As regards the most favorable form of attention we have two factors, the direction of attention and the level of attention. As regards the direction of attention three modes are possible. One may attend to the beginning, the middle or the end of the tone. More than half of all observers select the middle of the tone as the critical point. The rest are about equally divided between the beginning and end of the tone. Closely connected with these modes of reaction is the snap judgment. With the organism set for a definite point in the tone, the judgment is made the instant this point reaches the focus of consciousness. This form of judgment when once brought under control almost always favors improvement.

It is also observed that it is easier to judge which tone is higher or lower if the forks are presented abruptly. If the tone swells gradually from a faint beginning, it appears to raise the pitch slightly and thus confuses the judgment.

The most favorable level of attention varies with different individuals. The introspections show that for some the closest attention to the tones is required for successful work. Others say the very keenest attention causes high nervous strain which leads to mistakes. The writer has observed this very definitely in his own case. T. F. V. says, "Much depends upon my attitude. If I hold myself in a passive attitude and answer with ease, in a reflex way, I am quite sure to be correct in my judgment; but if I get the attitude of strict attention I cannot do so well. If I can keep in a state of relaxation, I experience no difficulty in giving the judgments." Practice usually results in what Professor Welton calls receptive recognition. When one becomes familiar with tones there ceases to be that active attitude of attention which characterizes the first few tests. The two tones are not thought of separately, but the interval is grasped as a whole and is interpreted by its total effect in consciousness. The factors which enter into the judgment do not come into consciousness, but remain unconscious. All that the observer can state is that he knows the instant he hears the second tone whether it is high or low. There is no consciousness of a memory image and no comparison.

To determine the effect of distraction three series of tests were made. In the first the eyes were closed (no visual or motor distraction); in the second the eyes were kept open and allowed free movement (normal distraction); and in the third the observer was required to trace a maze while performing the test (regulated distraction).

The effect seems to be about equally distributed between helping and hindering. All but two were appreciably aided by distraction at the beginning of the series. At the close only two were especially aided and only one found distraction a hindrance. In all the other cases, when the distraction method became automatic, it ceased to influence the results. Moderate distraction seems to be an aid chiefly as a means of raising the level of non-voluntary attention. The best form of attention for a majority of observers seems to be a periodic fluctuation between sharp and instant attention to the tones and complete diversion during the interval between two pairs of tones. The problem of distraction is an exceedingly complicated one. Perhaps the most striking result of this series of tests was the demonstration that distraction enters even when we are most expected to concentrate upon a single task. Table XII shows the per cent. of right judgments with and without distraction.

TABLE XII. The effect of distraction

Observer	With Distraction	Without Distraction
1	93.9%	92.3%
2	86.2	84.5
3	89.0	91.3
4	91.2	87.4
5	90.8	87.5
6	88.7	92.6
7	86.8	91.5
8	95.8	96.3
9	92.5	91.5
10	93.3	97.3

The following extracts from introspections illustrate the general mental attitude toward the different methods. These were written at the close of the series of tests and express the observer's impression at the time.

D. A. A. "In the beginning of the test I felt distinctly dissatisfied with surroundings and was annoyed by the peculiar effect the room had on the quality of the tone. This was overcome in about twenty minutes. The maze failed to arouse interest and hence was of no assistance—quite the contrary, it was really an annoyance—a distraction unfavorable. During the time I took 100 with eyes closed I was able to inhibit any absorbing interest in anything except the discrimination of tones. I was able to concentrate

very definitely on the work in hand. Following immediately with closed eyes on a test with 0.5 v.d. I grew tired of the uniform method (eyes closed) making two mistakes in the first 50 and six in the second 50. About five of the errors were attended by a feeling of decided uncertainty and the others were caused by some annoyance. Returning to the use of the maze for the last 100, 0.5 v.d. I voluntarily renewed interest in the affair and raised my record making but four errors in the 100 judgments."

J. E. B. "At first the maze troubled me, but after going over it a number of times I could do it rather automatically so that more and more attention was given to the tones. When the eyes were open there were always numerous disturbances that would effectively distract attention. With the eyes closed there was very little to distract attention."

M. C. "Working at too great tension seemed to be my greatest difficulty during the tests, especially with eyes closed and eyes opened. The maze seems to relieve that tension though I rather expected the opposite effect. As a result of the tension I found myself confused at times and made several errors in succession. I could not notice much difference in my own attitude toward the test with eyes closed and that with eyes open. Possibly there was a greater effort to center attention on the two sounds when my eyes were open. In both of these tests there were times when I seemed to notice a difference in pitch, but could not tell which sound was higher. This difficulty seemed to disappear with the maze test."

N. E. G. "I felt that it was much easier to decide with my eyes closed than with the maze. However, my third record shows fewer errors with the maze."

P. H. H. "In the test using the maze it seemed easier to concentrate the mind; that is, the mind was concerned with two definite things: the maze and the tones, as opposed to the free associations. There was less inclination to drift to other things. The decisions in the maze test involved less conscious effort and seem to be 'felt' rather than consciously formed. Errors in the maze test often followed the effort to locate the end of the pencil line, after it was lost through the recording of the introspection. I gained better success by starting the maze line ahead of the trial."

T. F. V. "I found in this experiment that everything depended upon my attitude towards it. If I had my attention in high strain to perceive the difference between the tones and to give a correct judgment my results would be very poor. However, if I fixed my attention upon something else and gave almost passive and indifferent attention to the forks my judgments were far more certain. When my eyes were closed I attempted to focus my attention upon the retinal light and also attempted to complicate matters by means of eye-movement. That is to say, I was endeavoring to center my attention on something other than the forks. In the maze, the more intensely I worked with respect to accuracy and speed, the more clear seemed the distinction between the forks. This focusing of my attention on something else than the thing in hand was very hard to do, especially after I made one or two mistakes in close succession. If I had not been so desirous of getting correct judgments I am sure my discriminative ability would have been better."

L. E. W. "My preference is for the maze, eyes open, next and last of all eyes closed. In the latter case my mind is ever full of visual and auditory imagery, rich and prolific. One moment I am in my room and can hear the clicking of my typewriter, the next I am singing some haunting air, then I see a paper on my table I should have brought with me this morning. Sometimes I recall in auditory imagery just what the order of the last two forks was and I feel sure that I was wrong though I had unconsciously made the wrong reply. The main difficulty with keeping the eyes closed is that in so doing I can't keep a constant image or position before me; my mind refuses to remain a blank. Now, with eyes open I can fixate my eyes on some particular object and as long as this does not waver and my thoughts and attention are on the business at hand I feel secure—am so, in fact. With the maze I direct my attention to one thing continuously."

E. D. S. "Yesterday I was interested in the maze and hence was distracted by it. To-day I felt no such interest in the maze. In the test with eyes closed I became interested in the method of presenting the forks and was thinking about certain possibilities of modifying the method. This became a distraction or rather a constant object of attention and source of error."

The second function of training in pitch discrimination is the acquisition of skill in listening to musical tones. Four factors are involved. First, skill means raising the level of non-voluntary attention. The power to concentrate upon the characteristic acuteness or gravity of the tones without conscious effort usually favors correct judging and is the end to be sought in ear training. Swift found that strained attention results in distraction, and a number of observers make similar statements regarding their own experience in distinguishing tones.

Second, skill means mechanizing the conscious factors in learning to distinguish differences in the pitch of tones. The pupil has learned to image the tones as auditory, auditory-vocal, kinaesthetic, or motor qualities. In this process some one or two qualities have predominated, and the object of ear training is to form habits of listening to, *i.e.*, of thinking musical tones in terms of their dominating imagery.

The third factor in the acquisition of skill is interest. One of the chief aims of ear training should be to enlist the pupil's interest in the appreciation of musical tones and the enlargement of the scope of apperception with reference to isolated tones.

The physiological limit

The physiological limit is undoubtedly considerably lower than is indicated by the threshold which would give 75 per cent. right

cases, as here used. To demonstrate this and, at the same time, to observe the significance of the choice of a particular increment in the homogeneous method, measurements were made on seven good observers whose threshold had been recorded as being in the neighborhood of 1 v.d. Four tests were made on each of the seven observers at 1, 0.5, and 0.25 v.d. with 200 judgments at each unit in double fatigue order, or a total of 800 judgments at each unit. From the per cent. of right judgments the probable threshold with 75 per cent. right cases was computed by the Fullerton-Cattell formula.

Table XIII shows the difference threshold which was required to give 75 per cent. of right judgments for 1, 0.5, and 0.25 v.d. respectively.

TABLE XIII

No.	1 v.d. v.d.	0.5 v.d. v.d.	0.25 v.d. v.d.
1	.42	.44	.49
2	.40	.27	.23
3	1.30	3.33	1.30
4	1.10	1.47	1.10
5	1.75	1.47	1.30
Average	.82	.74	.60
6	1.00	1.00	
7	1.54	1.30	
Average	1.24	1.17	

The first five observers had more than fifty per cent. of right judgments at 0.25 v.d.; hence the threshold is calculated for the three increments of the other cases. Number 6 got only 49 and number 7 only 46 per cent. right cases on the 0.25 increment. But the significant fact is that for both of these persons the number of right judgments on 0.5 v.d. was such as to give practically the same threshold as was found on 1.0 v.d. Only one important inconsistency occurs in the above table. In the case of No. 3. the right judgments at 0.5 give a threshold of 3.33 v.d. while at 1 v.d. and 0.25 v.d. the threshold is exactly the same.

Examination of the table therefore proves that in the region of the average physiological limit the conventional threshold may be computed on the basis of observations considerably below that limit (here in five cases out of seven) and that the actual physiological limit is always considerably below the conventional threshold. This is, of course, analogous to what we find in sight; under exception-

ally favorable circumstances we may see a small, well defined object at a distance which, from the nature of the dioptric system, represents the physiological limit of acuity in vision but average records of acuity would ordinarily designate a point short of that distance.

Correlations

From the standpoint of musical training it is important to know how the ability to distinguish differences of pitch is correlated with other mental characters, as general intelligence and singing ability. In addition to these we wish to know whether brothers and sisters are more closely correlated in ability to distinguish differences of pitch than other children not related. These questions are discussed in their order.

For the purpose of the correlation between pitch discrimination and general intelligence and singing ability, the data for pitch discrimination were obtained from the final days of the practice series. No single absolute measure of general intelligence is possible. For the present purpose the teachers were instructed to mark general intelligence on the basis of two criteria, brightness and reliability, assuming these to be of equal weight. By brightness is meant quickness and accuracy of mental grasp, or, in other words, general wide-awakeness. Reliability is self-explanatory. It is the correlate of a small mean variation for daily work. For convenience of marking, these two factors may be considered as having equal weight and may, therefore, be marked independently on a scale of 10. It was explained that the markings should follow approximately the normal distribution for each age and for both sexes. The mean of the two marks was taken as the mark representative of intelligence.

In order to facilitate correlation the ten units in the series of increments used in pitch discrimination were translated into corresponding values on the scale of 10, thus: 30 v.d. corresponds to 1; 23 to 2; 17 to 3; etc. .... 0.5 v.d. to 10.

The markings on singing ability were also based on the teacher's judgment of the pupil's ability to sing correctly in pitch scale and a melody.

As regards kinship, three correlations were as follows: (1) between younger and older brothers and sisters with practice; (2) the same without practice; and (3) between the younger members

of the second correlation and other children of the same age and sex as the second members, but not related.

The correlations were determined by the Pearson product-moments method. In order to show the relative distribution of individuals for each factor correlated, each group is subdivided into five grades. This is not a quintile subdivision as there is no attempt to have an equal number of persons in each subdivision. The distribution by grades serves the purpose of comparison quite as well as the quintile or quartile method and avoids the necessity of ranking, which is practically impossible on a scale of 10 units. The method of subdivision is very simple. The scale of 10 units is is divided into five equal parts. 1 and 2 = E. 3 and 4 = D. 5 and 6 = C. 7 and 8 = B. 9 and 10 = A. An example will make clear the method. An observer gets 3 in pitch discrimination and 7 in general intelligence. He belongs to Grade D in the first factor and in Grade B in the second factor. The number who are in the same grade in each factor indicates the degree of correlation. The number who are in different grades in the two factors indicates lack of correlation or low correlation.

The results show a relatively high coefficient of correlation between pitch discrimination and general intelligence, singing ability and musical training (Tables XIV and XV). It is higher for boys than for girls and highest for both boys and girls between pitch discrimination and general intelligence.

TABLE XIV. Correlation of pitch discrimination with general intelligence and singing ability

Pitch discrimination with

(1). General intelligence	Boys	r	.70	p.e.	.023
	Girls	r	.63	p.e.	.026
(2) Singing ability	Boys	r	.71	p.e.	.023
	Girls	r	.51	p.e.	.031

TABLE XV. A. Pitch discrimination and general intelligence

Boys — Intelligence

	234	A	B	C	D	E
	A	4	15	16	3	
	B	6	31	29	3	1
Pitch	C	3	25	33	11	
	D	1	8	19	8	
	E	1	4	4	9	

Girls — Intelligence

	274	A	B	C	D	E
	A	6	14	14		
	B	12	51	27	3	
Pitch	C	11	49	37	12	
	D	4	3	17	4	
	E		1	6	3	

B. Pitch discrimination and singing ability

Boys

		Singing				
	234	A	B	C	D	E
	A	5	7	3	2	
	B	17	22	13	8	4
Pitch	C	4	24	23	12	7
	D	2	13	16	8	6
	E	1	4	15	8	11

Girls

		Singing				
	274	A	B	C	D	E
	A	11	21	12	1	
	B	8	46	38	10	2
Pitch	C	8	27	39	16	
	D	3	8	16	2	3
	E					

The fact of a high correlation between pitch discrimination and general intelligence favors the conclusion reached above that pitch discrimination depends partly upon the ability to learn, *i.e,* upon brightness and reliability. If this is a correct view, training in pitch discrimination is essentially mental training. It is more than reproducing tones; it is thinking tones. Another conclusion which is in harmony with what has just been said is that a child may possess a perfect ear for tones, and still be unable to distinguish differences in pitch. Musical training should begin with training in tone quality.

The coefficient of correlation between pitch discrimination and singing ability is technically high. A high correlation between these factors means that the ability to distinguish differences in the pitch of tones is an essential factor in learning to sing.

Table XVI shows that, for the groups compared, girls are superior to boys in pitch discrimination, since there are no girls in Grade E and relatively few in Grade D. But they are not shown to be essentially superior in singing ability.

TABLE XVI. Correlations for blood relationship

Correlation between pitch discrimination of				
(1). Brothers and sisters:				
(a) with practice	r	.48	p.e.	.031
(b) without practice	r	.43	p.e.	.035
(2). Children not related	r	.53	p.e.	.030

The coefficient of correlation between brothers and sisters on the basis of ability in pitch discrimination is not higher than between other children. This is true both for records without practice and records after practice. Although the results are clearly negative, no sweeping conclusion should be drawn because several variables are involved, such as advantage of the knowledge which comes with age, differences in intelligence, the element of competition, etc. This is regrettable since it had been definitely hoped and planned

that this large collection of data might contribute to the solution of this interesting question. Finally, the younger member of each pair in the second correlation was compared with another child of the same age and sex as the second member, but not related. The coefficient of correlation is practically the same for the three groups. (Table XVII). No conclusions can be drawn from these meager results as regards the influence of heredity on tonal hearing.

TABLE XVII. Correlation of pitch discrimination for younger and older brothers and sisters

(1) With practice

		Older				
	129	A	B	C	D	E
	A	7	5	1		1
	B	9	32	12	5	1
Younger	C	4	17	7	4	2
	D		9	2	3	
	E		3	2	2	1

(2) Without practice

		Older				
	275	A	B	C	D	E
	A	2	13	12	7	1
	B	2	6	17	12	1
Younger	C	12	12	54	27	10
	D	7	8	25	32	5
	E	1	1	2	4	2

(3) Children not related and without practice

		Older				
	275	A	B	C	D	E
	A		4	2		3
	B	6	22	28	12	8
Younger	C	6	38	36	15	9
	D	2	15	23	4	2
	E		13	16	4	7

General conclusions

The quantitative statement and analysis of data has been presented in such condensed form that a summary of conclusions from that point of view is scarcely necessary. There is, however, need of a statement of "general conclusions" from the point of view of interpretation and application of the experimental results in the light of the quantitative data, the introspections of the observer, the daily notes of the experimenter, and a general study of the problem with the collaborators in research. Such a statement necessarily involves something of a personal equation and I am glad to acknowledge in this the co-operation of Professor Seashore whose long and varied experience in this field of research makes this interpretation possible.

The psychological limit in pitch discrimination is always below the conventional threshold (75 per cent. right cases). Thus, a person whose threshold is 1 v.d. may, under extraordinarily favorable circumstances, hear as small a difference as .25 v.d.; and it is probable that in the normal unreflective and uncritical appreciation of music the automatic "impression" of tone differences comes freely through this region of increments which are below the conventional threshold. This conventional threshold which can not be further reduced by instruction or training we have called the "approximate" physiological threshold. This is the concept of threshold that must be employed for most purposes of research and in nearly all applications of the test for practical purposes. The three factors which differentiate it from the true physiological threshold are—the convention of counting 75 per cent. right cases, the physical variation in the organ of Corti, and the failure to keep all the conditions of the measurements under control.

Success in making a true measurement on an unexperienced observer in a single sitting varies with the knowledge, keenness, and care of the observer and the many objectively favorable or unfavorable conditions of the test as well as the experimenter; but, everything taken into account, it is safe to say that when an individual test is made under favorable conditions the approximate physiological threshold may be reached in a single sitting of less than an hour for more than half of the cases of adults or children who are bright and old enough to understand the test. Even in group tests by the heterogeneous method one may reach in an hour the approximate physiological threshold of nearly half of the observers who are old enough and bright enough to observe.

A cognitive threshold, always above the approximate physiological threshold, may be due to failure in understanding what is required in the test, lack of information, defect in auditory imagery and memory, lack of application, confusions, objective or subjective disturbances, expectations, inhibitions in writing or speaking, etc. Most of these conditions are such that they may be removed by information, by inducement to use the best effort, or by learning through some experience.

There are means of determining when the approximate physiological threshold has been reached. Chief among these are the mean variation and the character of the distribution of the errors. But in

individual tests many direct observations on the character of the difficulties in judging may be helpful. In general, where a record is low (good) the chances are that the observer has no "cognitive" difficulties. The uncertainty is, of course, always with reference to the poor record. Practical advice or recommendation should therefore be cautious in the case of poor records for fear that the limit reached, although persistent, may be merely cognitive. One can not err on the side of getting too good a record; the danger is always that something has prevented a fair test of actual ability.

The sensitiveness of the ear to pitch difference can not be improved appreciably by practice. There is no evidence of any improvement in sensitiveness to pitch as a result of practice. When a person shows a cognitive threshold practice ordinarly results in a clearing up of the difficulties which in the way of a true measure of discrimination by information, observations, and the development of interest, isolation of the problem in hand, and more consistent application to the task in hand. This is, of course, not improvement in the psycho-physic ear but merely a preliminary to a fair determination of the psycho-physic limit. It follows that instruction in regard to the nature of the test and individual help are all important for the lowering of the cognitive limit and that mere practice for this purpose is a poor and uncertain makeshift. It also follows that a "cognitive" threshold is no measure at all but rather a confession that the measurement has not yet been successfully made.

Training in pitch discrimination is not like the acquisition of skill, as in learning to read or to hear overtones. It is in the last analysis informational and the improvement is immediate in proportion to the effectiveness of the instruction or the ingenuity of the observer and the experimenter in isolating the difficulty.

Reduced to its lowest terms the question of variation with age may be interpreted to mean that we have no evidence of improvement in the psychological limit of pitch discrimination with age; a young child of school age and even younger, can hear pitch fully as keenly as an adult. The amount in favor of the adult shown in all group statistics is amply accounted for by the difficulty in making a reliable test on the young and by their lack of information. This statement is based primarily on two lines of evidence,—the common occurrence of fine, irreducible records among young children, and the character of the conditions which are ordinarily overcome by instruction and training.

Pitch discrimination does not vary with sex to any significant extent. In the records here reported and in the many hundreds of other records in this laboratory in which comparisons may be made for sex, certain tendencies are shown in groups of records, sometimes in favor of one sex and other times in favor of the other sex, but on the whole, it seems certain that such differences, except so far as they are due to grouping, may be accounted for as due to the conditions of the test rather than to the sex difference in the psycho-physic capacity of pitch discrimination. Thus one of the most consistent and striking differences reported above, that of the superiority of elementary schoolgirls over elementary schoolboys may probably be fully accounted for by the prevailing trait of aloofness of the preadolescent boy toward music. These boys often regard music as a sort of frill for girls and, therefore, enter the test with less fervor than do the girls. Such interpretation is supported in part by the fact that in the high school and in the university, where the girls have had far more advantage of training than the boys, the records reveal no appreciable difference for sex.

Not a single case of tonal deafness was isolated in any of the records here reported. This would indicate that if tonal deafness exists at all in a "normal" ear, it is no so common as has usually been supposed.

We have found a high correlation between pitch discrimination and ability in singing, as judged by teachers. In the collective records there is also a high correlation between pitch discrimination and "general intelligence." This is undoubtedly due to the presence of so many "cognitive" as opposed to physiological thresholds.

Under the conditions of this test the records of members of the same family do not correlate more closely than do members of different families.

This test is elemental, *i.e.,* when applied under favorable conditions it calls forth a relatively simple and immediate sensory discrimination which does not improve appreciably with practice. It is like the minimum visible angle in visual space—the limit is set by the sense organ. We say "under favorable conditions" because the cognitive factors which condition a fair test must be recognized. As has been seen in a large per cent. of cases, we can get only a cognitive threshold in the first attempts. As elemental, this test is contrasted with, *e.g.,* a test of ability to isolate overtones

in a violin tone which represents a skill that can only be acquired through practice. It must be recognized that the test is a true and successful test, the results of which may be applied with safety, only as it is actually elemental.

The basal character of pitch discrimination in the appreciation and expression of music has become evident in many ways. Keen recognition of pitch difference is a condition of auditory imagery, auditory memory, singing or playing in true pitch. This is true as well for the affective attitudes with reference both to pitch and to timbre, for timbre is in the last analysis simply a pitch complex. It would therefore seem to be most fundamental of all tests of musical talent, although, of course, no one test by itself can be considered an adequate measure of such talent.

The educational value of this test has been strongly impressed during this work. It is unquestionably the isolation and measuring of one specific, basal factor in musical talent. It may be undertaken individually or in groups and commends itself particularly as one of the tests that should be made in schools for the purpose of vocational guidance in music, in the music studio for the purpose of learning where to place the emphasis in instruction and in adapting the course to the natural capacities of the student, and as a recurrent exercise in the schools and in the studio for the purpose of developing keenness in attention to detail of tone in ear training.

The instruments, *i.e.,* the tuning forks and resonators as here used, and the method, both the heterogeneous and the homogeneous procedure, have proved eminently satisfactory.

THE LOWER LIMIT OF TONALITY

BY

THOMAS FRANKLIN VANCE

An accurate determination of the threshold of the lowest audible tone involves a consideration of the variables which condition it. The area and the amplitude of the wave and the distance of the vibrating body from the ear of the observer are the principal objective variables. Individual differences, due largely to innate capacity, degree of practice, and ability to concentrate attention, and variations within the same individual which may be attributed to changes in physical tone and mental content, are obviously the most influential subjective variables. These variables, both objective and subjective, present particular problems which must be considered in their relation to the general problem of the lower tonal limit before the latter can be accurately determined.

No attempt will be made here to review the history of investigation on this problem. A good summary is found in Titchener's Instructor's Manual, Quantitative.

Mr. Misao Imai made a careful study of this problem in this laboratory in 1907. Inasmuch as his results have not been published and the present study is essentially a repetition of his work for the purpose of verification it is necessary to report his work in brief.

Mr. Imai's first problem was to determine the relation between the threshold and the amplitude of the wave. He produced the tones by an electro-magnetic fork 460 mm. in length and 10 mm. by 20 mm. in cross section of a prong. By differential weights five tones could be produced, namely, 35, 25, 22, 19, and 17 v.d. By varying amount of resistance different amplitudes could be secured. The test in each case consisted in determining the smallest amplitude that would produce an audible tone at a given pitch. The measurements were made on ten laboratory students. With this apparatus, he obtained the results shown in Table I.

TABLE I. The relation of threshold to amplitude

v.d.	ampl. in mm.	m.v.
30	1.30	.15
25	1.75	.30
22	2.20	.45
19	2.95	.50
17	3.45	.50

From these results he drew the conclusion that the threshold varies inversely as the amplitude; *i.e.,* increase of amplitude lowers the threshold.

With the same apparatus he conducted a second series of experiments to learn the relation between the distance from the ear of the vibrating body and the threshold. From these results he concluded that the distance at which the fundamental tone is just perceived, varies with the pitch; *i.e.,* the higher the pitch, the greater the distance may be, within given limits. Below 18 v.d., however, he found the distance uncertain as the overtones were frequently confused with the fundamental.

It then occurred to him that the area of the vibrating body might have an important bearing upon his general problem. He varied the area by means of four pairs of discs 6 mm. in thickness with diamaters of 6, 8, 10, and 12 cm., respectively, which could be attached to the ends of the prongs of a fork similar to the one described above. With the variable of area thus controlled, he learned that it must always be given due consideration in the determination of the lower limit of tonal hearing. Judgments from ten highly practiced observers showed clearly that the threshold varies inversely as the area, within limits.

Investigators previous to Imai had used three different methods in the production of tone; namely, (1) vibrations of tuning forks, pipes, and reeds; (2) difference tones; and (3) interruption tones. Helmholtz, Stumpf, and Preyer favor the use of tuning forks. Where forks have been used the thresholds are, as a rule, noticeably lower than where other means have been employed. Schaefer views with suspicion all thresholds reported under 16 v.d., inclining to the belief that perceptions below that point are conditioned by overtones rather than by fundamentals. He doubts von Bezold's assertion that the fork by means of which he registered a threshold of 11 v.d. was free from overtones. Schaefer is doubtless in the right in his contention that von Bezold has not proved this point conclusively. Von Bezold's statement that the very low tone 11 v.d. was perceived by some observers with normal hearing, cannot be accepted unqualifiedly. In fact, the statement would have been more convincing had he admitted the probability of overtones. Unless an observer realizes the possibility of an overtone and is cautioned to discriminate between it and the fundamental, he will base his judg-

ment upon the first tone perceived which below 18 v.d. will always be an overtone, if the tone is not pure. Preyer reported the very conservative threshold of 18.6 v.d. Wundt's threshold of 14 v.d. with an Appunn reed and 16 v.d. with a wire fork undoubtedly held true with good observers. They compare very favorably with the results of Imai who found the threshold to be about 16 v.d. with the plain forks.

Imai, then, by an increase in area obtained a lower threshold than those recorded by the most reliable previous investigators. His observers who had a threshold of 16 v.d. with the plain fork were enabled to hear 12 v.d. with the most favorable size of disc-forks. This lowering of the conventional threshold of tonality by about 4 v.d. was of such importance as to justify a verification and extension of his experiment under most accurate conditions of control.

In the investigation here reported tuning forks were used for the production of the tone because they are among the best means for producing relatively pure tones, and there seemed to be no other way to change the area of the vibrating surface without at the same time altering the rate of vibration.

Two electro-magnetic forks were arranged tandem. The driver fork was constructed of soft steel and measured 65.5 cm. in length from the base to the tip of the prongs which were 1 cm. by 2 cm. in cross section. A series of eleven holes, 1 cm. in diameter and 2.8 cm. apart, bored in each prong made the fork lighter and more active. The fork hung vertically from a support by means of a large iron hook securely bolted to its base. The driven fork was made of soft steel rod and measured 57 cm. in length. The prongs were 1 cm. in diameter. By means of differential weights, vibration rates ranging from 18 to 12 v.d. could easily be secured. Three pairs of discs of fibre 6 mm. in thickness and 2.5 cm., 5 cm., and 10 cm. in diameter respectively, served a double purpose; they not only afforded the desired variation in area, but also, served as weights for the tuning of the fork. The two forks were tuned to the same pitch. The driving fork was kept at a distance, thus causing the driven fork in the observing room to vibrate without the disturbance of an interruption spark and sound. The amplitude of vibration was varied by means of variation in the strength of the energizing current through an adjustable resistance coil in the experimenting room.

All the experiments were conducted in the sound-proof room. Beginning with 18 v.d., the series followed a descending order until the threshold was reached. Each rate, however, was observed with the three different areas of vibrating surface as controlled by the discs of 10, 5, and 2.5 cm. in diameter. The observations were completed at each step in the order named before the forks were adjusted to the lower rate. The observer sat in a comfortable position with one hand on the switch which controlled the current through the driven fork, and waited attentively for the tone. The experimenter, after a warning signal, presented the fork with the central part of the disc exactly opposite the opening of the more sensitive of the two ears and as close to it as the amplitude of vibration would permit. After a very brief period of observation, with the current either on or off as desired, the observer immediately reported "tone" or "no tone". The observer's judgment thus determined the amplitude of the fork in the next presentation. When very near the threshold of intensity the experimenter recorded the amplitude of vibration upon smoked paper. This method was followed until the least intensity of tone which the observer could possibly hear, for a given rate and area, was reached. Each period of observation lasted one hour; during that time data for each disc at two different vibration rates could usually be obtained. Three complete series from 18 v.d. to the threshold with the three different discs were secured for each observer.

The nature of the experiment demanded that only experienced observers be employed. Ten members of a class in experimental psychology were available. Each of these submitted to an eight hour course of training which was divided into periods of two hours each. From this group of ten, the experimenter chose three whose work in the preliminaries was most satisfactory to act as observers throughout the entire series. The demonstrator of this class and the experimenter brought the total number of observers up to five, all of whom had had considerable training in psychological experimentation.

Before turning to the consideration of the results, a brief statement of the difficulties encountered in a study of this kind should be made. That there are difficulties may be inferred from the fact that different investigators have placed the threshold at points ranging from 8 to 30 v.d. Low tones are intrinsically weak and

require much more energy for their production than do the high tones which are relatively strong and clear. While little effort is required in sensing a tone in the normal range, it becomes a matter of strict attention to perceive the tones near the limen where the predominant overtone may be confusing. It is not easy to procure a perfectly pure tone in the central rigister where the instrument used is small, but it is next to impossible to produce an absolutely pure tone with the large forks of the lower limit. Furthermore, we are not accustomed to pay attention to liminal tones, therefore, when a person is asked to observe them under laboratory conditions, he is at first at a loss to know what to listen for. It takes hours of training before the individual feels the necessary degree of certainty in giving his judgments. But patience and practice tend to make him reasonably certain of his judgments. The development in accuracy of observations may be traced in the characteristic expressions, "I hear something!", later, "I think I hear the fundamental!" and finally, "I am sure I hear the fundamental!"

TABLE II. Average results of M. C. C.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.13	.04	.40	.13	1.33	.51
17	.23	.04	1.07	.31	2.93	1.18
16	2.00	2.00	3.43	1.05	5.50	2.20
15	2.23	1.78	3.43	1.91	5.33	1.98
14	3.07	1.76	3.50	3.33	9.80	4.20
13	5.23	1.96				

TABLE III. Average results of C. B.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.23	.18	.90	.47	2.20	.93
17	.43	.18	1.27	.49	3.27	.18
16	.87	.29	1.97	.38	4.00	.67
15	1.40	.13	2.63	.04	2.57	.42
14	2.33	.58	3.07	.29	5.60	.53
13	2.87	.91	4.53	.49		
12	4.50	.67				

TABLE IV. Average results of F. O. S.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.47	.24	2.47	.78	3.13	.62
17	1.23	.73	3.07	.18	5.07	1.29
16	1.60	.73	4.03	1.24	6.50	.87
15	3.43	3.35	4.60	1.20	7.37	.78
14	3.73	.79	6.00	1.67	9.00	2.33
13	3.70	.53	4.07	.89		
12	3.77	1.34				

TABLE V. Average results of L. E. W.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	1.13	.57	1.77	.38	2.43	.78
17	1.50	.20	2.30	.60	3.47	1.51
16	2.73	.84	3.23	.62	5.33	.58
15	2.70	.20	4.63	.91	7.00	1.00
14	4.17	1.42	4.13	.24	6.40	1.40
13	5.40	2.13	7.07	.76		
12	6.10	.60				

TABLE VI. Average results of T. F. V.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.67	.04	1.13	.24	1.90	.27
17	1.23	.18	1.90	.13	3.13	.58
16	1.27	.18	2.40	.27	3.87	.44
15	1.77	.18	3.60	.27	5.50	.00
14	3.17	1.22	5.53	1.29		
13	5.67	.44				

TABLE VII. Average results of five observers

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.54	.08	1.33	.24	2.20	1.05
17	.93	.09	1.92	.21	3.57	.72
16	1.69	.73	3.01	.58	5.04	.60
15	2.31	.33	3.78	.61	5.55	.52
14	3.29	1.03	4.45	1.16	7.70	.90
13	4.57	.96	6.37	2.68		
12	4.78	.37				

The quantitative results of this series of experiments are expressed in Tables II-VII and Fig. 1. The records show that, within the limits here studied, the larger area offers more favorable conditions for the perception of the lowest audible tone. To be more specific, the threshold varies inversely as the area of the vibrating surface; *i.e.*, the greater the area, the less the frequency of vibration, or the lower the threshold may be. In all of the curves, whether of individual series, or of averages of series, this law is demonstrated. In every case the frequency of vibration is less with the area of 10 cm. than with the area of 2.5 cm. The frequency of vibration with the area of 5 cm. usually falls between that of the area of 10 cm. and the area of 2.5 cm., but occasionally it is equal to one of these, but even then there is a decided difference in amplitude. The results show that the average threshold for the first five observers lies between 15 and 14 v.d. with the smallest area,

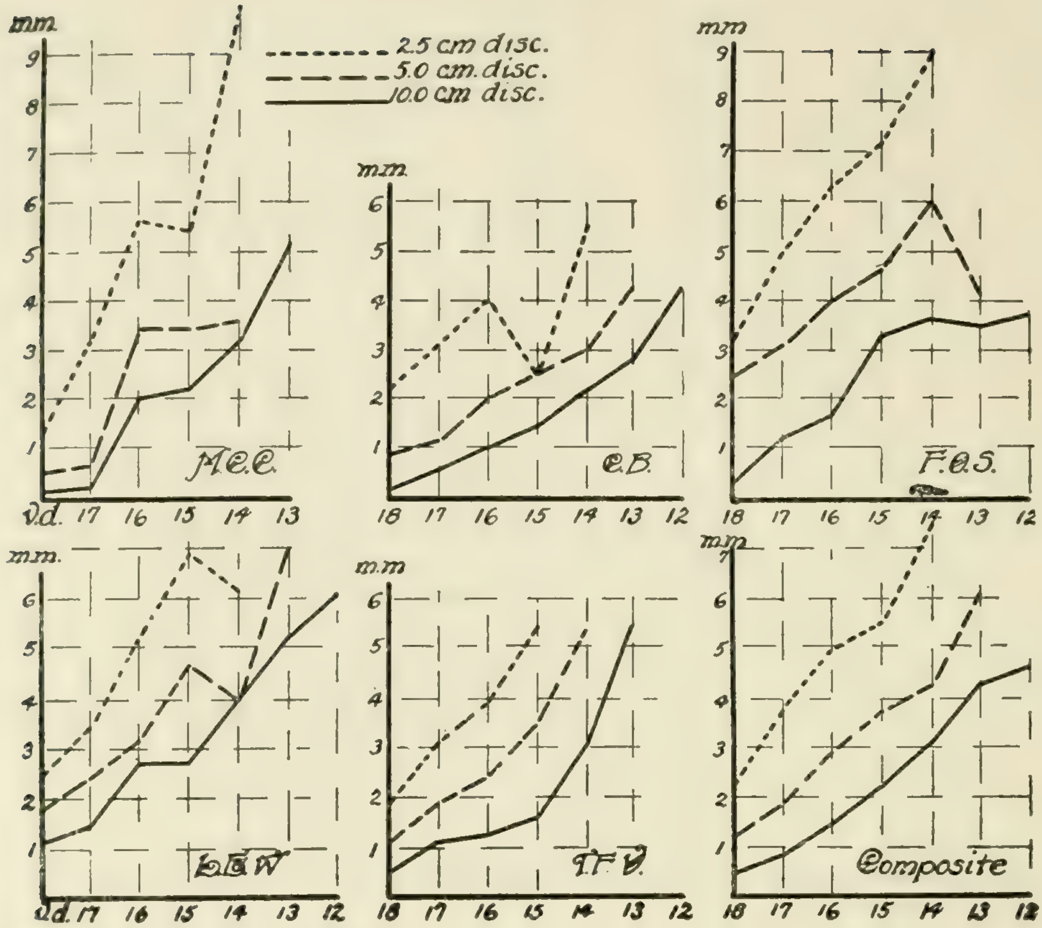


Fig. 1. (Tables. II-VII.)

between 14 and 13 v.d. with the middle area, and between 13 and 12 v.d. with the largest. This is as accurate a determination as can be made from the data secured. There are, obviously, certain judgments which cannot be accepted at their face value. The average results of F. O. S., as shown in Figure I, may be taken as an illustration. In this case there should be much hesitancy in placing the threshold at 13 v.d. for the middle area inasmuch as the amplitude required is considerably less than that required for 14 v.d. The drop in the curve for the smallest area for L. E. W. may be interpreted in a similar way. In either of the instances cited there is, however, the possibility that the observers actually did perceive a tone, the small amplitude being the result of better attention.

Within limits, then, the threshold is lowered by increase in area. This limit, under the conditions of the experiment, is approximately 10 cm. Results secured by Mr. Imai with a disc of 12 cm. in diameter are no better than those secured with a disc of 10 cm. Furthermore, at the beginning of my own investigation, the area was

increased to 20 cm. but the conditions were in no way improved by this still larger area.

Again, the threshold, as measured in terms of frequency of vibration, varies inversely as the amplitude of vibration; in other words, increase in amplitude lowers the threshold, within limits. It requires a greater amplitude to produce an audible tone of 17 v.d. than it does to produce one of 18 v.d. And so the amplitude gradually increases until the threshold is reached. Thus the curves have an almost uniformly ascending trend. Variations in attention offer the most probable explanation for the exceptions that occur in the lower part of the curves, but the confusion of the overtone with the fundamental is the most frequent cause of the irregularities in the upper part.

Mr. Imai conducted two sets of experiments on the variable of distance. From the first of these he deduced the law that, within limits, the maximal distance at which the fundamental tone is just perceptible varies directly as the pitch; *i.e.*, the higher the pitch the greater the distance it can be heard.

In the second series he used two different discs to vary the area, one 12 cm. in diameter and the other 6 cm. The difference in distance which resulted from this difference in area he found very slight,—not more than 1 mm. But this difference, together with incidental observations recorded throughout the course of the whole experimentation, led him to the conclusion that, within given limits, the maximal distance at which a threshold tone could be perceived varies directly as the area of the vibrating surface. Both of the above conclusions have been verified by our own experimental procedure.

The variables of a subjective nature encountered in the attempt to determine accurately the lower limit of tonality are not different in kind from those which are met in experimenting upon any other part of the tonal range. But the fleeting, momentary character of the low tones makes it extremely difficult to hold them in clear consciousness. Certain of the subjective variables, therefore, become more disturbing in the lower register than they do in the middle register. Every precaution must be taken to control them. Individual differences and variations within the individual have a vital bearing upon the problem.

Practice is one of the most significant variables but fortunately

it can be easily controlled. The difficulty of the task set the observer necessitated in every case a rather high degree of practice. When the series of experiments opened the observers had first to learn to distinguish between fundamental and overtone, and later, in the case of very low tones, to distinguish between fundamental and puffs of air with comparative ease and certainty. The records of three of the observers show clearly the effect of practice also in the lowering of the record. Musical education undoubtedly lessened the amount of special training required. Mr. Imai observed this also in his experiment. A few members of the Minneapolis Symphony Orchestra were given a series of these tests and nearly all of them heard the tone of 12 v.d. This superiority of musicians may be due to a selection—to a musical nature rather than to training, although both count.

Fatigue can easily become one of the most vitiating factors. Its effect is both physical and mental. It has been demonstrated in this laboratory that tones in the middle register may be listened to attentively for two hours without a disturbing effect. But low tones fatigue the ear very quickly and when thus incapacitated the power of analysis deteriorates and the determination of the threshold therefore becomes uncertain. Furthermore, it demands the closest attention on the part of the observer to sense the fundamental tone. There is, of course, a strain of attention and when mental fatigue occurs sensations and images of tones may be confused. It is highly essential, therefore, that the experimental series be of short duration and that it should be interrupted by frequent periods of rest.

The observers had a strong tendency to give the tone a definite location in space. One or two placed it in the back part of the head, very near the neck. One said that when he heard both the fundamental and the first overtone, the fundamental had a lower place in space than did the overtone. Another maintained that the first tone heard was usually an overtone and seemed very close to the ears, but the fundamental seemed to come out of the darkness from some place lower than the ears.

The larger area produced by the attachment of the discs caused puffs or whiffs of air to occur in connection with the tone. The larger the area, the more noticeable they are. Very early in the experimentation the puffs confused the observers, but after practice

they were scarcely noticed until the threshold had been reached when nothing but the puffs remained. They bear the same relation to the lowest audible tone that the thud, produced by striking the mallet against the König cylinder, bears to the highest audible tone. The observer ignores the thud until the threshold has been reached.

With three observers the fundamental bore a temporal relation to the overtone. The overtone was nearly always heard first, then the fundamental came out gradually from under the overtone, remained for an instant and then disappeared leaving nothing but the puffs. One observer said, "The fundamental arises as a faint impression in the ear and dies out quickly. It seems to emerge from beneath the overtone while the latter ceases. It does not come to consciousness suddenly, but gradually."

Low tones are intrinsically weak; therefore any factor that would increase the energy of the wave motion would tend to make a low tone more audible. Distance, *i.e.,* nearness to the ear, does this; area does also. In so far as mere audibility of the tone is concerned, amplitude, nearness, and area are the chief factors. But the element under observation is fusion of the individual vibrations into a tone. What has been brought out most clearly in this investigation is that the limit of fusion is set not only by vibration frequency but by the factors just named. Their total effect should be interpreted primarily in terms of the *form* of the wave. Nearness, large amplitude, and large area all unite to form a continuity of the wave. When the waves which impinge upon the tympanum assume the form of the sine curve we have probably the most favorable conditions for that fusion which is the essence of tonality.

In summary, the factors which must be taken into account in the accurate determination of the lower limit of tonality are of two kinds, the objective and the subjective. The objective factors are four: namely, area, amplitude, distance, and timbre. It has been shown (1) that the threshold varies inversely as the area of the vibrating surface; (2) that the threshold varies inversely as the amplitude of vibration; (3) that the amplitude of vibration varies inversely as the area of the vibrating surface; (4) that the maximal distance at which the fundamental tone is just perceptible varies directly as the pitch; and, (5) the maximal distance at which a threshold tone can be perceived varies directly as the vibrating surface. The subjective variables are in general the same as those

encountered in other experiments in audition. They are, differences in innate capacity, in degree of practice, in ability to concentrate attention and in mental content. The wide variation in threshold-values, as determined by the different investigators, may be accounted for by the lack of control of some of these variables.

VARIATION IN PITCH DISCRIMINATION WITHIN THE TONAL RANGE

BY

THOMAS FRANKLIN VANCE

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HISTORICAL STATEMENT

The object in this brief historical survey is to place before the reader only those results which are most closely related to the present problem with respect to the methods used and the aspects investigated. Therefore, investigations made with similar methods but having only a narrow field, as well as those exploring a large range by different means, will not be discussed.

Preyer may be considered the pioneer worker on the problem of pitch discrimination within the tonal range. Earlies investigators concerned themselves with the least perceptible difference at only a single point in the register. Delazenne, (1) in 1827, used a metal string 1147 mm. in length with a vibration rate of 60 v.d., which he divided into equal parts by a bridge. He found that if the bridge was removed only 1 mm. from the central position a difference of pitch could be detected by well trained ears when the two parts of the string were sounded in succession. Weber (20) declared that he was able to determine the pitch of tones so accurately through the ear that he never made an error of more than one vibration on a tone of 200 v.d. He thought it possible that this keen discrimination

might be due to beats. Sauveur (9) with two monochords tuned to the same pitch found that when one string was shortened by 1/2000 of its length a difference in the pitch of the two tones was recognizable, but he leaves no record as to the pitch of the tones. Schleiber (12) recorded a differential threshold of less than .5 v.d. on the tone of b. Seebeck (14) and two superior violin players differentiated without fail between two tuning forks vibrating at 1209 and 1210 v.d.

Preyer.—Preyer (7) used the Appunn tonmesser,—an instrument consisting of reeds which gave the following tones: from 500 to 501 in steps differing by 0.1 v.d., 504, 508, 512, 1000 to 1001 in steps of 0.2 v.d., 1008, 1016, 1024, 2048, and 4096 v.d., respectively. More than a thousand judgments as to whether the two tones compared were equal or different, were secured from twelve practiced observers. They were not required to tell the direction of the difference. To his own results he added those of Delezenne and Seebeck. Transcribing Delezenne's measurement in millimeters into terms of frequency of vibration, he found that the shorter string produced a tone of 120.2 v.d., while the longer one gave a tone of 119.8 v.d., thus making a difference of 0.4 v.d. which skilled observers could detect. Likewise, a simple computation revealed the fact that Seebeck had the very low threshold of 0.36 at 449 v.d. After some verification of Seebeck's results, Preyer inclined to the belief that the threshold might be brought as low as 0.25 v.d. The combined results give differential thresholds for four different places in the tonal range as follows: Delezenne at 120 v.d. found the threshold to be 0.4 v.d., Seebeck at 440 v.d. found it to be 0.4 v.d., Preyer at 500 v.d., 0.3 v.d., and at 1000 v.d., 0.2 v.d. From this study, inadequate as it may seem, Preyer draws the following conclusions:

(1) One-third of a vibration on 500 v.d. and five-tenths of a vibration on 1000 v.d. will always be recognized as different by the best observers, although the most sensitive, the most trained, and the most reliable ears tested could not recognize a difference of 1000 and 1000.25 v.d. nor of 500 and 500.2 v.d. (2) Very high tones and very low tones cannot be discriminated so accurately as tones of the middle region. Capacity for discrimination is keen between c and c^3, keenest for the region from a^1 to c^2, but beyond c^3 it decreases slowly until it becomes very unreliable at c^5. Fis^4 marks a second point of keenness of capacity. (3) The relative difference for pitch is dependent, in a high degree, on the number of vibrations

of the compared tones; and the absolute difference-sensitiveness does not decrease with the pitch. (4) The judgment concerning the place of tones in the tonal line is more uncertain than the judgment as to whether the two tones lie at different points. (5) Practice is an influential factor in pitch-discrimination. Extreme fineness of capacity is peculiar only to those who have much familiarity with tones.

Luft.—In the Leipsic laboratory, during the years 1884 to 1886, Luft (4) experimented upon a range of tones extending from 64 to 2048 v.d. Tuning forks were used for the production of the tones, the variable being mistuned from the standard by means of sliding weights. The forks 64, 128, and 256 v.d. were energized by the stroke of a hammer of India-rubber, forks of 512 v.d., by means of a violin-bow, and forks of 1024 and 2048 v.d., by a wooden hammer, which was padded with felt. Forks of 64 v.d. were held upon large resonator-boxes; 128, 256, and 512 v.d. were brought to the openings of resonator-tubes of paper; while 1024 and 2048 v.d. were permanently attached to resonator-boxes. He used the method of minimal change and employed from four to eight different steps in passing from a large difference to one that was just perceptible, with the following results:

Standard v.d.	64	128	256	512	1024	2048
Difference	.15	.16	.23	.25	.22	.36

"In the field of tonal quality within the region investigated the psychophysical law, according to which the absolute differences of sensation correspond to relative differences of stimulus which must be constant, finds no application. On the contrary the differential threshold within the interval mentioned approaches the constant average of 0.2 vibrations."

The threshold value slowly rises from 64 to 2048 v.d. with the single exception of 1024 v.d. Luft admits that the error here is probably due to an objective circumstance. His results can scarcely be compared with those of Preyer as the methods employed were quite different. It is a point worthy of observation, however, that with Preyer the point of finest discrimination lies at 500 v.d. while with Luft it lies at 64 v.d. Later, Luft found the threshold of 32 v.d. (by the same method employed with 64 v.d.) to be about .44 v.d. His results, therefore, do not contradict Preyer's statement that very high tones and very low tones cannot be distinguished as readily as tones of the middle register.

Luft noted that practice lowers the threshold and that the effect

of practice is not equally distributed in all parts of the range. The influence of practice is of special importance at 64 and 128 v.d. Luft lowered his own record from 0.85 to 0.3 v.d. at 128 v.d., and from 0.42 to 0.15 v.d. at 64 v.d. He believes that the only reason that the initial thresholds for the lower tones is higher than those of the central region, is, that the degree of practice for the former is not so great. The variable of practice was far less noticeable in the higher regions which was due, he believed, to the greater intensity and persistence of these tones. He even ventured to say that the individual variations of the differential threshold are, for the most part, due to practice.

Meyer.—In 1898, Meyer (5) published Professor Stumpf's thresholds for the discrimination of pitch. Tuning forks were used for the tones 100, 200, 400, 600, and 1200 v.d. The variable forks were mistuned by the insertion of a screw in the end of the prong,—a more accurate device than that of sliding weights. After discarding the method of minimal change as practically worthless for the problem in hand, and thereby questioning the validity of Luft's results, Meyer adopted the method of right and wrong cases. Forks of 100 and 200 v.d. were held in the hand and brought to the openings of the resonators, while forks of 400, 600, and 1200 v.d. were mounted on resonator-boxes and were energized by the blow of a hammer. Each individual experiment was performed three times and even more if the observer wished it, before a judgment was required. In this manner Meyer thought to equalize variations of intensity and time-interval, as well as fluctuations of attention. Stumpf's thresholds as determined by means of the Cattell-Fullerton table, from the data given, are as follows:

V. D.	100	200	400	600	1200
Differential threshold	.54	.25	.28	.24	.69

The author concluded his report thus:

"One sees, therefrom, that approximately the same difference of pitch is recognized with equal certainty at 200, 400, and 600 v.d. and with less, but likewise moderately equal certainty, at 100 and 1200 v.d. The differences in these cases are so small that they may be considered accidental. That the certainty of judgments declines in still higher and still lower tones is self-evident."

Stücker.—Stücker's work (16) is the most extensive study published on this particular subject. His observations covered the range between the limits 72 and 35000 v.d., or nine entire octaves. He

employed the following standard tones: d^{-1} (73.4), c^0 (130.5), c^1 (261), all the tones of the major scale up to c^2 (522), a^2 (870), a^3, g^3 (3100), c^5, g^5, c^6, g^6, c^7 and c^8. All of the tones up to and including c^2 were produced with tuning forks, a^2 and a^3 with a monochord, and the remaining ones with a Galton whistle. In each individual series he started with a large difference in the number of vibrations of the two instruments and then made the difference gradually smaller until the threshold was reached. Such a procedure was repeated a few times for the purpose of verification. Whether the observer indicated the direction of the difference or merely the difference is not stated. Given below are the average values of the relative and absolute sensitiveness of discrimination of his fifty observers for eight different levels, with his statement in summary:

Pitch	d^1	c^0	c^1	a^1	a^2	a^3	g^3	g^5
Rel. Disc.	.94	.74	.49	.32	.30	.44	.86	4.91
Abs. "	.7	1.	1.3	1.4	2.5	7.7	26.7	304

(1) Neither the absolute nor the relative sensitiveness to difference of the two tones remains constant in the different tonal regions. (2) The relative difference-sensitiveness is in general the greatest in the first and second accented octaves; in many cases, however, the second maximum lies in the third and fourth accented octaves. (3) With one-third of the entire number of observers the relative sensitiveness to difference in the second half of the first accented octave is nearly equal; namely, 0.2 and 0.3; when one compares the individual curves of sensitiveness with these, the places of greatest sensitiveness lie in the upper half of this region, while with unmusical individuals they occur in general in the lower half. (4) The degree of sensitiveness is subjected to fluctuations within an octave, which is repeated in each octave in the same proportion; it is the greatest for c, slightly less great for g and still less for f and h. (5) A number of persons possess a secondary maximum of sensitivity. (6) An unusually great sensitiveness in high tonal regions is a characteristic of musical persons.

Stücker points out that the discrimination was far more accurate in the lower regions when the second tone was lower, while in the higher region the opposite was true. The inference here is, that judgments are facilitated when the second tone is farther removed from the first and second accented octaves, which are most frequently employed in musical composition; i.e., when the second tone is the farther from this middle register, the judgment seems to be more accurate. He further adds, that the daily variation of non-musical observers is less than for musical ones.

A year later this same author (17) published a report supplementing the one just reviewed. In this he states the results obtained from three different types of observers, professional players of various instruments, singers, and individuals decidedly unmusical. The average values of the absolute differences for the three different sets of observers have been computed for seven levels in the tonal range, as follows:

	d^1	c^0	c^1	a^1	a^2	a^3	g^4
Players	.35	.37	.40	.56	1.20	2.64	13.0
Singers	.46	.48	.44	.71	1.62	3.07	14.0
Unmusical	12.62	2.20	2.80	4.80	9.96	24.00	130.0

Of special interest in this second article is the statement that with tenors and sopranos the finest discrimination is found beneath their voice register, but with bass and alto singers above their voice register; the difference is not between the voices of men and women, but only appears between the relative height and depth of the voice-register of both sexes.

The age difference, he maintains, is more significant than that between musical and non-musical observers. After the age of thirty, sensitiveness to difference declines and the range becomes restricted.

Schaefer.—In 1910, Schaefer (10) submitted a thesis to the Department of Psychology of the State University of Iowa on the subject, "The Curve for the Variation of Pitch Discrimination within the Tonal Range", which has not been published. The apparatus and method were practically the same as those used in the present investigation. For observers, he had fifteen normal individuals varying in musical ability and training. Five hundred tests were given on each of the tones 24, 32, 64, 128, 256, 512, and 2048 v.d. The average threshold for each of these in the order given is as follows: 3.3, 3.4, 2.9, 1.3, 1.5, 1.8, and 6.7 v.d. He summarizes his results thus:

(1) The form of the composite curve indicates that discrimination for the average normal individual is most difficult in the higher and the lower registers and becomes easier in the middle register. (2) The majority of the individual curves are of the same form as the composite. Curves of individuals having high thresholds are of about the same form as the curves of individuals having low thresholds. (3) There are notable individual differences. (4) Musical training does not influence to any large extent, the ability to perceive difference of pitch. (5) It is easier to detect difference in pitch than to name the direction of the difference.

STATEMENT OF THE PROBLEM

The primary purpose of this investigation has been to determine the prevalence of islands or gaps in pitch-discrimination within the tonal range. The pursuit of this aim has taken the form of an attempt to make a comparatively large number of complete individual measurements on pitch-discrimination within the tonal range with as many as possible of the hitherto unknown or disregarded sources of error under control. On the basis of frequently observed defects in the hearing of pitch, found in clinical cases, it is generally believed that such disturbances occur in varying degrees in normal persons. In the curves of two or three of Schaefer's observers, there are places where discrimination of pitch is less keen than the balance of the curves would seem to indicate that it ought to be; in the case of one observer the evidence of a gap was striking. Professor Titchener (19) deems such cases of sufficient importance to bring to the support of the Helmholtz theory of hearing. He says:

"Cases occur in which the range of hearing is normal, but the tonal scale is not continuous; there are tonal gaps, large or small, parts of the scale where the patient is completely deaf to tonal stimuli, though he can perfectly well hear the cases above and below."

The sources of error in a problem of pitch-discrimination are so great and insistent that successive investigators of the same problem are fully justified in a patient struggle to overcome them with progressive insight. In reading the various reports on the subject, one cannot help being impressed with the fact that very few, if any, of the investigators fully realized the significance of the many important variables which could easily—and doubtless did—vitiate the results. The disturbing factors, due to faulty apparatus and inadequate procedure, mentioned by Professor Seashore in his preliminary report (13), suggest the seriousness of the problem. From my own experience I am convinced that his statement in regard to these factors is in no way exaggerated. Rather, it has not been made sufficiently emphatic. The danger of false criteria entering into the judgments of the most conscientious observer, either consciously or unconsciously, can scarcely be realized by one who has not encountered them first-hand. The danger of identification, alone, is sufficient to make the investigator very cautious.

Apparatus and Method.—In this investigation the measurements

were made at six different levels in the register; namely, 64, 128, 256, 1024, and 2048 v.d. The tones were produced by the best grade of Kohl tuning forks. For 128, 256, 512, and 1024 v.d. Helmholtz resonators were used; the forks of 2048 v.d. were mounted on resonator-boxes; while resonance for 64 v.d. was produced by extending the Helmholtz resonator for 128 v.d. For 64 v.d. a second set of forks was found to be more satisfactory at a later stage of the experiment. These were made of round tool steel, 12 mm. in diameter. The prongs were 30 cm. in length and carried hard rubber discs 10 cm. in diameter.

The sounder was a simple device consisting merely of a lead pipe about one inch in diameter with one end bent into the form of a circle for the base, and the other in the shape of a U at right angles to the base. The U-end, when covered with several thicknesses of rubber, made a sounder of the required elasticity and softness. The placing of the sounder on leather sand-bags resting on a heavy metal stand eliminated, in large part, the accessory noise of the blow. The forks of the four central octaves were energized by striking the middle of the prong upon the sounder; the forks of 2048 v.d. were struck as lightly as possible with a felt-hammer; while those of 64 v.d. were set into vibration by striking them on the sand-bags.

To mistune the variable fork, in every case except those of the lower limit, a screw was inserted in the end of each prong and to these were attached nuts, varying in weight, to give the desired pitch. Such a device is a decided improvement over the method of sliding weights, inasmuch as the latter may allow a slight change in position, with a corresponding change in pitch, during the course of the experiment. This is especially true of the smaller forks. At 64 v.d. variation in pitch was secured by shifting the discs, which were firmly attached to the forks by large set screws. At each of the steps the successive differences of one, two, three, five, and eight vibrations were chosen—a range which was found to be sufficient for all but one or two observers. All of the forks were tuned to an accuracy of five-hundredths of a vibration per second.

The mode of procedure followed the plan suggested by Professor Seashore in his preliminary report (13) in almost every respect, in the four central octaves where it was possible to do so. A most careful attempt was made to keep the tones at a constant intensity

without resorting to the uniformity of mechanical devices. The experimenter simply relied on the accuracy of his own hand and ear in presenting the forks in such a way that the tones would be of equal strength. If at any time, through a lapse on the part of the experimenter, the difference of intensity seemed pronounced, the trial was repeated. Mechanical devices are particularly unsatisfactory in that the difference of intensity which is practically certain to occur, be it ever so slight, is constant and might thus become a criterion for identification. In the method of presentation by hand, this source of error is eliminated. The ideal presentation is that in which the tones are just loud enough to be heard without a strain of the attention, and extreme care was taken throughout to guage the tones by this standard. The duration of each tone, as well as the time-interval, was approximately one second. Whether the constant or the variable tone should be presented first, was decided by a key which had been arranged first by chance and then revised to the extent that the same order could be followed no more than three times in succession, and that in one hundred tests the two possible sequences should have the same frequency. The observers in every case were required to render their judgments in terms of "second tone lower", or "second tone higher", in accordance with the method of right and wrong cases. No doubtful judgments were allowed; when the observer felt uncertain after repeated tests he was simply requested to guess. As a rule each individual experiment was given but once, but whenever disturbances of any sort, either objective or subjective, were noted, the experiment was repeated. Observers were instructed to trust the first impression. Except with the lowest tones, where the judgments were given orally and were then recorded by the experimenter, the observers themselves kept the record by simply writing *H* or *L* as an abbreviation of the judgments "higher" or "lower." With one observer, however, the response was oral throughout because attention to the writing caused too much of a distraction. At least one hundred judgments were recorded at each level, but many observers required a considerably larger number before their thresholds could be satisfactorily determined. No series of observations extended long enough to cause any disturbing fatigue. The monotony of the experiment was broken at intervals by the checking of the record and by the adjusting of the forks. Fatigue caused previous to the experiment could

not be very well controlled as the observers had to be taken at times which best suited their convenience. The tests were, however, fairly well distributed throughout the hours of the day and those observers who did come at a late hour were always dismissed if they felt fatigue to a degree which they thought might interfere with their best work. The experiment was conducted in the sound-proof room and in every instance the observers were tested individually so that distractions of an objective character were reduced to conditions connected only with the actual experiment.

The experimental control was naturally most difficult at 64 v.d. The large size of the forks not only made them more difficult to handle but also increased the possibility of overtones. Still another problem was presented in obtaining sufficient resonance for these tones of low intensity. Overtones were especially distracting with the first pair of forks that were used, but it was possible to overcome them to some extent by setting the forks in heavy handles of iron and by putting heavy rubber bands upon the prongs. Yet the increased weight added to the difficulty of handling. Two different methods were tried with these forks; namely, bringing the forks to the openings of the resonators, described above, and presenting them to the ear of the observer without the aid of a resonator. Both of these methods are unsatisfactory. The resonator scarcely makes the tones loud enough to make the judgment one of certainty, and it is difficult for the experimenter to present the tones so that they are of equal intensity. Holding the forks to the ear has the advantage of making the tones louder, but here again the variable of intensity is left uncontrolled, and the possibility is open to the observer for obtaining clues from the position of the fork, from timbre, and from noises caused by movements in presentation. The fact that the tones could be distinctly heard by the second method gave it the preference. But when the results were compared with those obtained for 128 v.d. the thresholds seemed abnormally large. This pair of forks was therefore discarded for the forks with the discs, which were found to answer the purpose much better, for at least three reasons; namely, they were freer from overtones, the tones were louder and clearer because of the increased vibratory surface offered by the discs, and they were neither so heavy nor so long, which facilitated handling very materially. With these forks the method of presentation to the ear was adopted, but on

account of the louder tone it was possible to hold the forks farther from the ear. Being also lighter in weight, they could be energized in a more uniform manner, and it was easier to bring them more nearly to the same point opposite the ear; thus the variable of intensity and direction of source could be more adequately controlled. An opportunity was not offered for the retesting of all individuals whose thresholds had been determined by the first pair of forks, but in most cases where a second was possible, somewhat lower thresholds were obtained.

No particular comment in regard to the forks of 128 and 256 v.d. is necessary. They were energized and presented to the resonators with the conditions of duration, time-interval, and intensity carefully controlled. In each case the tones were perfectly clear and distinct. The forks producing these tones held up long enough to allow five individual experiments without restriking. But the control was not quite so satisfactory at 1024 v.d. The forks at this level would vibrate with sufficient energy for only two tests. A more forceful blow was also required, and it was necessary to bring them very close to the small resonators, indeed so close that they nearly touched it. All this, of course, made it more difficult for the experimenter to maintain a constant intensity. Again, the piercing character of the tone was annoying to some observers. The tones produced by forks of 128, 256, and 512 v.d. were not heard by the observers except when reinforced by the resonators. But the 1024 v.d. forks gave a high piercing tone before being presented to the resonator. The observer, as much as possible, ignored this tone and concentrated his attention on the tones as they were intensified by the resonators.

In the upper limit, the method was necessarily quite different. The small resonator-boxes on which the forks were mounted were held in the hand; the one fork was struck and dampened, and then the second in close succession. So delicate a stroke was necessary to produce a tone that the noise of the blow was but a slight distraction, if any. It was extremely difficult, however, to keep the intensity constant. To eliminate discrimination of the direction of source, the position of the left hand was shifted to bring the forks to exactly the same place before they were energized.

Of the fifty observers who made this study possible by giving it their time and thought, thirty-three were members of the elemen-

TABLE I. *Absolute differential thresholds*

Obs.	64 v.d. T	64 v.d. m.v.	128 v.d. T	128 v.d. m.v.	256 v.d. T	256 v.d. m.v.	512 v.d. T	512 v.d. m.v.	1024 v.d. T	1024 v.d. m.v.	2048 v.d. T	2048 v.d. m.v.
1	2.5	0.9	1.9	0.5	0.8	0.6	0.7	1.1	3.3	.0	5.3	0.4
2	3.5	0.1	0.8	0.6	0.6	0.8	1.0	0.8	2.2	1.1	2.5	3.2
3	3.3	0.1	0.6	0.8	0.7	0.7	0.8	1.0	2.5	.8	5.6	0.1
4	3.0	0.4	1.5	0.1	2.2	0.8	2.3	0.5	1.1	2.2	3.5	2.2
5	4.0	0.6	1.3	0.1	1.1	0.3	0.9	0.9	4.1	.8	7.3	1.6
6			0.7	0.7	0.8	0.6	1.2	0.6	2.4	.9		
7	4.0	0.6	0.7	0.7	1.2	0.2	1.7	0.1	3.5	.2	6.7	1.0
8	1.0	2.4	1.5	0.1	1.4	0.0	1.8	0.0	3.7	.4	6.5	0.8
9	6.4	3.0	2.7	1.3	0.7	0.7	1.7	0.1	5.3	2.0	6.5	0.8
10	1.5	1.9	1.4	0.0	1.1	0.3	1.8	0.0	2.4	.9	4.9	0.8
11	2.0	1.4	0.8	0.6	1.5	0.1	1.0	0.8	3.9	.6	5.6	0.1
12	3.7	0.3	0.7	0.7	1.5	0.1	2.4	0.6	4.3	1.0	3.5	2.2
13	1.5	1.9	1.0	0.4	0.7	0.7	2.0	0.2	3.8	.5	6.7	1.0
14	4.0	0.6	2.7	1.3	1.8	0.4	2.2	0.4	5.0	1.7	10.0	4.3
15	3.0	0.4	1.4	0.0	2.1	0.7	4.4	2.6	6.4	3.1	8.8	3.1
16	3.4	0.0	1.3	0.1	0.6	0.8	0.8	1.0	2.2	1.1	3.0	2.7
17	2.0	1.4	1.1	0.3	2.9	1.5	5.0	3.2	5.1	1.8	7.6	1.9
18	0.7	2.7	1.0	0.4	2.0	0.6	0.6	1.2	0.8	2.5	5.8	0.1
19	5.2	1.8	0.6	0.8	1.1	0.3	0.8	1.0	4.1	0.8	9.7	4.0
20	1.0	2.4	1.0	0.4	0.7	0.7	1.2	0.6	3.2	.1	6.1	0.4
21	3.8	0.4	2.3	0.9	1.6	0.2	1.7	0.1	3.6	.3	5.5	0.2
22	2.5	0.9	0.8	0.6	1.0	0.4	1.3	0.5	2.0	1.3	3.0	2.7
23			2.0	0.6	1.3	0.1	1.4	0.4	6.8	3.5	5.5	0.2
24	6.4	3.0	1.5	0.1	1.1	0.3	1.5	0.3	2.0	1.3		
25	4.0	0.6	2.0	0.6	2.5	1.1	2.5	0.7	2.4	.9	4.4	1.3
26	3.0	0.4	0.7	0.7	1.0	0.4	1.9	0.1	1.8	1.5	5.8	0.1
27	4.7	1.3	0.7	0.7	1.5	0.1	1.7	0.1	2.1	1.2	3.6	2.1
28	3.4	0.0	0.6	0.8	0.8	0.6	1.1	0.7	2.3	1.0	3.2	2.5
29	2.4	1.0	2.1	0.7	1.5	0.1	1.6	0.2	8.4	5.1	10.2	4.5
30	2.5	0.9	2.4	1.0	1.3	0.1	1.4	0.4	4.1	.8	5.7	0.0
31	6.4	3.0	2.4	1.0	2.0	0.6	1.5	0.3	3.9	.6	3.0	2.7
32	2.4	1.0	1.0	0.4	1.0	0.4	1.1	0.7	2.2	1.1	4.6	1.1
33			1.7	0.3	1.4	0.0	2.7	0.9	3.2	.1	8.8	3.1
34	3.3	0.1	1.5	0.1	1.5	0.1	1.4	0.4	2.5	0.8	4.9	0.8
35	6.4	3.0	1.1	0.3	1.7	0.3	1.0	0.8	6.4	3.1	9.9	4.2
36	8.8	5.4	1.1	0.3	0.8	0.6	2.2	0.4	3.5	.2	4.8	0.9
37	3.0	0.4	1.5	0.1	1.4	0.0	2.6	0.8	4.4	1.1	10.2	4.5
38	0.7	2.7	0.6	0.8	0.7	0.7	2.1	0.3	1.6	1.7	3.7	2.0
39	7.2	3.8	4.1	2.7	3.2	1.8	1.6	0.2	7.6	4.3		
40			0.9	0.5	1.2	0.2	0.9	0.9	0.7	2.6	4.3	1.4
41	2.4	1.0	0.8	0.6	0.7	0.7	2.1	0.3	1.8	1.5	3.5	2.2
42	4.0	0.6	2.0	0.6	3.2	1.8	4.9	3.1		7.0	7.0	1.3
43	1.3	2.1	0.8	0.6	0.7	0.7	1.1	0.7	1.8	1.5	5.3	0.4
44	3.0	0.4	1.1	0.3	0.4	1.0	1.2	0.6	2.6	.7	3.0	2.7
45	3.1	0.3	1.4	0.0	1.5	0.1	1.4	0.4	3.2	.1	8.0	2.3
46			1.4	0.0	1.3	0.1	2.6	0.8	2.7	.6		
47	3.0	0.4	1.2	0.2	1.6	0.2	1.7	0.1	2.4	0.9		
48	3.0	0.4	1.4	0.0	1.1	0.3	0.8	1.0	2.7	.6		
49	5.0	1.6	4.0	2.6	2.5	1.1	6.1	4.3				
50	1.3	2.1	1.0	0.4	0.7	0.7	1.0	0.8	0.9	2.4	1.2	4.5
Mean	3.4	1.5	1.4	.57	1.4	.5	1.8	.76	3.3	1.31	5.7	1.56
Median	3.00		1.2		1.3		1.5		3.0		5.5	

TABLE II. Relative differential thresholds

Obs.	64 v.d.	128 v.d.	256 v.d.	512 v.d.	1024 v.d.	2048 v.d.
1	.31	.12	.03	.01	.03	.02
2	.44	.05	.02	.02	.02	.01
3	.41	.04	.02	.02	.02	.02
4	.38	.09	.07	.04	.01	.01
5	.50	.08	.03	.01	.03	.03
6		.04	.03	.02	.02	
7	.50	.04	.04	.03	.03	.03
8	.13	.09	.04	.03	.03	.03
9	.80	.17	.02	.03	.04	.03
10	.19	.09	.03	.03	.02	.02
11	.25	.05	.05	.02	.03	.02
12	.46	.04	.05	.04	.03	.01
13	.19	.06	.02	.03	.03	.03
14	.50	.17	.06	.03	.04	.04
15	.38	.09	.07	.07	.05	.03
16	.43	.08	.02	.01	.02	.01
17	.25	.07	.09	.08	.04	.03
18	.09	.06	.06	.01	.01	.02
19	.65	.04	.03	.01	.03	.04
20	.13	.02	.02	.02	.03	.02
21	.48	.14	.05	.03	.03	.02
22	.31	.05	.03	.02	.02	.01
23		.13	.04	.02	.05	.02
24	.80	.09	.03	.02	.02	.02
25	.50	.13	.08	.04	.02	.02
26	.38	.04	.03	.03	.01	.02
27	.59	.04	.05	.03	.02	.01
28	.43	.04	.03	.02	.02	.01
29	.30	.13	.05	.03	.06	.04
30	.31	.15	.04	.02	.03	.02
31	.80	.15	.06	.02	.03	.01
32	.30	.06	.03	.02	.01	.02
33		.11	.04	.04	.03	.03
34	.41	.09	.05	.02	.02	.02
35	.80	.07	.05	.02	.05	.04
36	.11	.07	.03	.03	.03	.02
37	.38	.09	.04	.04	.03	.04
38	.09	.04	.02	.03	.02	.01
39	.90	.26	.10	.03	.06	
40		.06	.05	.01	.01	.02
41	.30	.05	.02	.03	.01	.01
42	.50	.13	.10	.08		.03
43	.16	.05	.02	.02	.01	.02
44	.38	.07	.01	.02	.02	.01
45	.39	.09	.05	.02	.03	.03
46		.09	.04	.02	.02	
47	.38	.08	.05	.03	.02	
48	.36	.09	.03	.01	.02	
49	.63	.25	.08	.10		
50	.16	.06	.02	.02	.01	.01
Average	.4	.09	.04	.03	.03	.02

tary class in psychology in the University, sixteen others were advanced students in psychology, and one other a member of the staff in psychology. It is important to note that the fifty represent a selected group. The thirty-three from the elementary class were chosen from a class of one hundred or more because their differential thresholds at 435 v.d. were less than 8 v.d., as determined from a test given to the class for purposes of demonstration. The advanced students had likewise shown in previous tests that their thresholds for discrimination of pitch were easily less than 8 v.d. Their closer association with the work in the department of psychology also tended to make them slightly better as a group than the elementary students. This basis of selection must be borne in mind in the consideration of the results, for our composite curve is not an average curve; it is superior to the average. It was gratifying to find that all of the observers took keen interest in the problem and made a sincere effort to give the work their best attention. Their knowledge of the fact that they were chosen because of their former good record helped them to maintain an interest.

RESULTS

The Composite Curves.—Table I includes the individual thresholds in terms of the absolute difference of vibrations for the six points in the range. The odd numbers of the observers refer to women, and the even, to the men. The thresholds are given in column T, and the mean variation in column m.v. At the foot of the table are the mean, the median, and the mean variation of the group. In Table II the same records are reduced to the relative threshold expressed in terms of the fractional part of a whole tone, at the respective levels. The figures in italics at the head show the number of vibrations in a whole tone at each of these respective levels. The record of Table I is shown graphically in Fig. 1 and that of Table II in Fig. 2. By an error the decimal point was left out before each of the numbers 1, 2, 3, and 4, in Fig. 2.

There is evidently no essential difference between the mean and the median curves; they run practically parallel throughout their course, coming a little closer together at 256 v.d. than at any other point. But inasmuch as the mean allows the extremes an influence out of proportion to their importance, the median must be considered the truer representative figure.

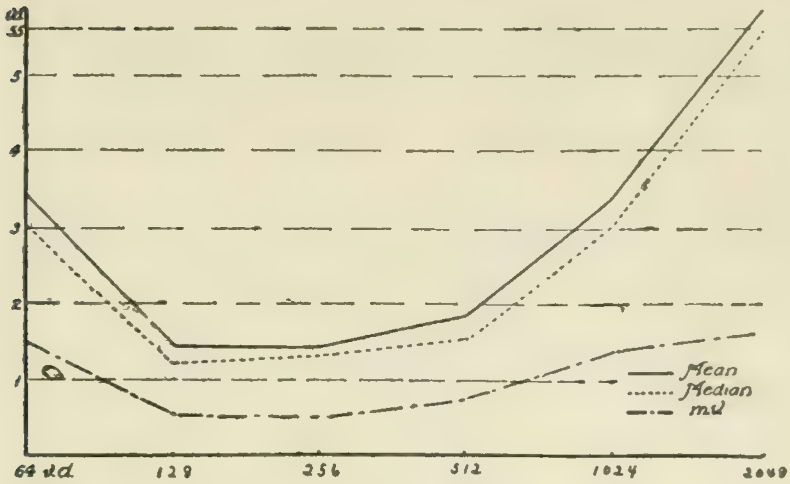

Fig. 1. Mean, median, and mean variation—absolute (Table I).

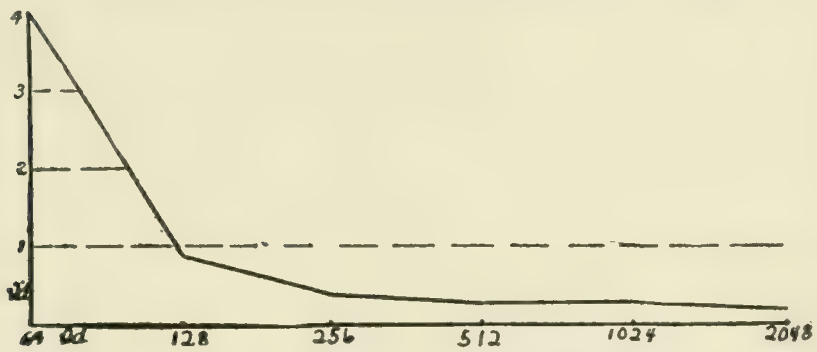

Fig. 2. Mean—relative (Table II).

The average capacity of discrimination, as measured in terms of absolute difference, is practically the same for 128 and 256 v.d. From this central register the curves rise slowly to 512 v.d., from which begins a rapid rise that becomes even more rapid from 1024 on up to 2048 v.d. The curve of mean variation (Fig. 1) follows the general trend of the composite, which means that the thresholds for this group of fifty form a more compact grouping in the central register, while at both the upper and the lower limits they are more widely separated.

The relative curve declines rapidly at first and then very gradually, reaching its lowest point at 2048 v.d. In other words discrimination of pitch, as measured in fractional parts of a whole tone, decreases somewhat abruptly from 64 to 128 v.d., but very slowly from that point to the upper limit of the range studied. In the relative curve the minimal value is at 2048 v.d. which is the region of the maximal value in the curve of absolute difference.

The absolute difference of vibration frequency has been adopted as the vehicle of expression in this report, for the reason that it is slightly more concrete and brings out the individual differences more strikingly. Its true relation to the relative must, however, be kept in mind.

The curves represent with a high degree of accuracy, it is believed, the average capacity of a group of observers such as have had a part in this study. But there is little doubt that the form of the curves has been influenced, to some extent, by certain factors other than those of actual discrimination of pitch. There are objective factors which could not be perfectly controlled and which in some cases have led to confusion, but in other cases have resulted in identification. The former necessarily raised the threshold, while the latter lowered it. The subjective variables of attention and practice are important inasmuch as attention is seldom at is best and then only for short duration, and the degree of practice might always be greater. The thresholds are therefore not quite as low as they would be under the most ideal conditions.

With tuning forks, it is impossible to produce as satisfactory a tone at the extremes as in the central register. Discrimination of pitch at 64 and at 2048 v.d. is thus made most difficult and the observer has a tendency to pick up other criteria than pitch upon which to base his judgments. Differences of intensity, change in the direction of the source of sound, and noises accompanying the control of the experiment are the chief factors which cause disturbance. They lead to confusion, rather than to identification, because the method used necessitated their approximately equal distribution between the higher and the lower tones; that is to say, that they occurred in a chance order, were therefore unpredictable, and consequently could not be used as safe criteria for accurate judgments; for example, if an observer was inclined to judge the more intense tone the higher, there would be an increased probability of error whenever the lower tone happened to be more intense. Had the forks been energized by a mechanical device, rather than by the free hand, these variables would have been constant and would have become a means of identification, rather than a source of confusion. At the higher limit it was difficult to keep the tones of equal loudness. The tones produced by the small forks are very fine and persistent, and a slight variation in the forces of the blow produced

a perceptible change in the intensity of the tones, which was often confusing. Whether or not the greater intensity favored a judgment of higher or lower varied with the individual. For some, the pitch being nearly equal, the louder tone was considered the higher, while for others the reverse experience was true.

It is in the lower limit, however, that the most abrupt rise in the threshold is to be found. As has been previously mentioned, various methods of presentation were given a trial, but none of them, excepting with a very few observers, gave results which were comparable with those obtained at 128 v.d. Only two observers had a lower threshold for 64 and for 128 v.d., (Nos. 8 and 18). For observer No. 20 the thresholds for the two tones were the same, while No's. 10, 13, 29, 30, 38, and 50 were the only remaining ones whose thresholds for 64 v.d. did not exceed that of 128 v.d. by more than 0.5 v.d. In other words, forty observers have a threshold for 64 which is more than one-half of a vibration higher than for 128 v.d. That this difference would have been less had it been possible to rule out all the factors of confusion is probable.

But not all of the variables cause confusion. Those which are constant soon come to be associated with one of the two possible judgments and this, in time, brings about a lowering of the threshold. Just what is seized upon as a means of identification one cannot always say. The auditory capacity of analysis is very keen and often the slightest variable which occurs in a particular setting is selected as a clue for the proper response. Slight variations in timbre are among the most frequent sources of identification. It is impossible to make two forks exactly alike and the unavoidable structural difference may be perceived in the nature and composition of the overtones. The forks of the lower limit are particularly susceptible to variation in timbre. If these differences are perceptible, the error of identification is sure to appear. Even with presentation by hand there is the possibility of the experimenter's falling into some characteristic habit of presenting the forks, which may be identified eventually. He may form the habit unconsciously of striking one fork at a different angle from that of the other, or the time-order may have some constant peculiarity which gives a clue.

The errors due to identification are without a doubt the most serious with which the experimenter has to contend. But in an experiment such as this the error of identification is usually discover-

able by comparing the thresholds of one level with those of the other levels. Whenever an observer has a threshold at any particular level considerably lower than the tentative norm would warrant, the chances are, that the error of identification has had a part to play. The record of No. 20 is wanting in the table for 2048 v.d. because he had discovered some criterion other than pitch upon which to base his judgments. In fact he made nearly a perfect record with a difference of one vibration,—a lower threshold than his records at the other levels would warrant. Two other observers had a similar experience in the upper limit, but when the method was slightly changed, they lost their clue and were forced to rely on pitch. In the lowest level Nos. 28 and 40 were influenced in some way by criteria other than pitch, the latter to such an extent that his results were worthless. As has been said, just what criteria were selected by these observers is not known. Their introspections fail to reveal them, the observers contending throughout—and with undoubted conviction—that they were judging on pitch alone. Such illustrations show that the experimenter connot be too careful in his attempt to keep the judgments confined to pitch.

The subjective variables of attention and practice also play more important rôles at the extremes than in the central register. To secure a low threshold at these levels closer attention is necessary and, as these tones are rarely heard, the degree of practice is much less than for tones of the central register. Practice for these tones is only to be had in the laboratory as they are seldom used in musical compositions. Observers Nos. 2, 28, 47, and 50 were the only ones who had the advantage of practice for these extreme tones and their thresholds at these levels are all below the average.

Summing up, we have found that the curve of pitch-discrimination shows the threshold of absolute difference to be keenest from 128 to 256 v.d.; from 256 to 512 v.d. it takes a gradual rise; and from 512 to 2048 v.d., a rapid rise. On the lower side, from 128 to 64 v.d., the rise is very sudden. As expressed by the curve of relative difference, there is a continual decline from the lower to the higher limit; this decline, however, is very rapid from 64 to 128 v.d., much less pronounced from 128 to 256 v.d., and from 256 to 2048 v.d. the curve becomes very nearly a straight line. It will be of interest now to compare the above results with those of other investigators.

Comparative Curves.—Figure 3 represents the composite results

Fig. 3. Composite curves for six different investigators—absolute.

of the six different investigators who have approached the problem by the same general method. The curves of Stücker (16), Schaefer (10), and the writer show considerably higher thresholds than those of Luft, Preyer and Meyer. This difference may be explained. In a pioneer work, such as Preyer's (7), numerous sources of error as yet undiscovered must have had much influence upon the results. His low thresholds can be attributed, in some degree at least, to the error of identification. Since this error may creep in when the best grade of tuning forks is used, there is little doubt but that it must have played an important rôle with the tonmesser. Furthermore this instrument, the reed, is not reliable for fine pitch differences. Luft's (4) values, especially at the extremes, must likewise be questioned. Whenever such low results are obtained at 64 and at 2048 v.d., there must be conclusive evidence that they are not due primarily to discrimination of pitch, but to some other factor which permits of identification. Luft has given no such proof. Furthermore, in a problem of this kind, the method of minimal change, which he used is unreliable, as Meyer has well pointed out, in that it introduces factors other than those of pitch and the threshold value is not quite comparable to the threshold value in our method of constant stimuli. Professor Stumpf's curve, drawn by Meyer (5), shows exceptional ability and is probably accurate. The low thresholds can be adequately explained by the less extended range, by extraordinary natural capacity, and by a high degree of training in

experimental work. On the other hand it should be kept in mind that the curves of Stücker, Schaefer, and the writer, represent the results of a much larger number of observers, many of whom do not have exceptionally fine capacity for discrimination of pitch. The curves are therefore on a much higher level than if they were drawn exclusively from the results of observers who had unusually fine ability.

In the curves of Preyer, Schaefer, Meyer, and the writer the minimal threshold lies somewhere near the central region; but in the other two discrimination seems to be the best in the lowest level. With Preyer the finest capacity is at 500, with Luft at 64, with Meyer at 600 (although the thresholds for 200, 400, and 600 are practically equal), with Stücker at 73.4, with Schaefer at 128, and with the writer at 256 v.d. The maximal threshold is to be found in the highest part of the range in every case. The second maximum lies with Preyer, Meyer, Schaefer and the writer at 64 v.d.

An examination of these curves raises the question as to the cause of the variations. Individual differences are, of course, the principal cause but the nature of the objective control is undoubtedly a very important factor, especially at the extremes. If the apparatus and the method of the three investigators, who had a large number of observers, had been equally refined at the different steps, these grosser differences would probably not have occurred. As it is, they are most pronounced at the extremes where the control was the most difficult. The experimental control at 128, 256, and 512 v.d. can be made so perfect that no observer will be able to pass consistent judgments on any criterion other than pitch. For this reason the results of Stücker, Schaefer, and the writer agree, approximately, within this region.

Inasmuch as the curves take the same general direction, the variations in the upper limit are about what would be expected when one considers the difficulties to be encountered, together with the fact that one of the experimenters used an entirely different apparatus. But from 130.5 to 73.4 v.d., Stücker's curve continues in the same general direction which it has had throughout the entire course, while the other two curves have changed their direction. In other words, Stücker found the absolute difference for 73.4 v.d. to be less than for any other point in the line, while both Schaefer and I found at 64 v.d. the second maximum which is noticeably

greater than for any other point except at 2048 v.d. Luft's results seem to confirm those of Stücker, but Meyer's curve, as well as Preyer's, shows a rise at the lower limit. Indeed the ratio between the thresholds of Meyer for 100 and 200 v.d. is very similar to the ratio between the thresholds for 64 and 128 v.d. obtained by Schaefer and myself. I have no hesitancy in concluding, therefore, that sensitiveness in the great octave is, in general, not so keen as in the small octave. But for reasons already given, it does not follow that the difference is actually as great as the numerical results of this study would seem to indicate. In the light of the experience of the present study, however, Stücker's findings in the lower limit must be held in question. It seems more probable that his observers had learned to make judgments on some criterion other than pitch. Just what that may have been cannot be stated definitely as that author has failed to give any detailed account either in regard to method or to apparatus. It is only known that the tone in question was produced by a tuning fork. The possibilities of error with the large tuning forks are, however, sufficiently great to warrant the statement that Stücker's low record is due, not altogether to discrimination of pitch, but that secondary criteria have been operative in giving the low thresholds.

Individual Differences.—An examination of Table I discloses the fact that the observers may be classified in two general divisions. In the first there are thirty-seven whose curves follow the course of the composite curve in that the smallest values are to be found in the central register on either side of which a slow or a rapid rise is evident. In the second division, are thirteen whose curves do not conform to any general type. In the irregularity of the curves of this second division lies the only possible evidence of gaps which this study has developed.

The curves of the first division may, in a general way, be given a three-fold classification; namely, (1) those which show a relatively low threshold at some point in the central region and relatively high thresholds at the extremes, (2) those in which the thresholds are fairly uniform throughout the entire range, and (3) those curves in which the threshold for 64 is lower than for 2048 v.d.

Division I.—In Figure 4 are the five curves of the first group which show, the most strikingly, the relatively low thresholds in the central register and the higher thresholds at the extremes. These

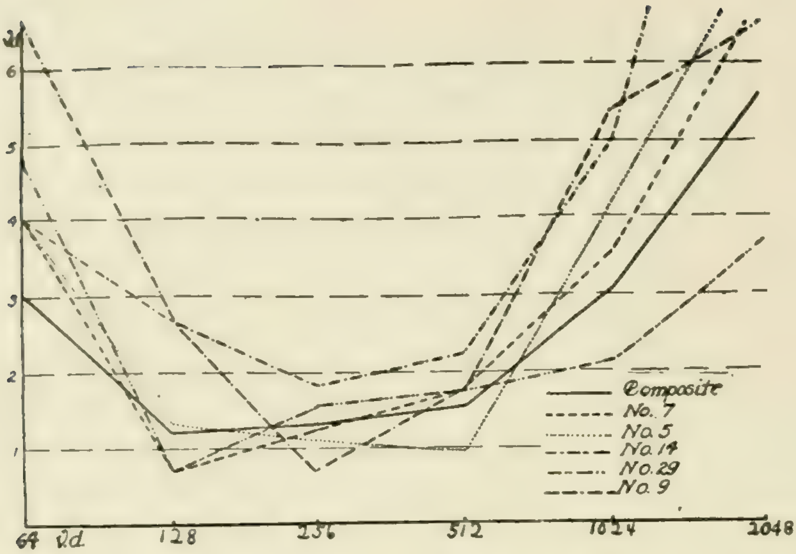

Fig. 4. Individual curves of observers Nos. 5, 7, 9, 14, and 27, which show relatively low values in the central region and high values at the extremes. The solid line is the composite curve of the fifty observers.

curves all resemble the composite more or less closely. At the extremes, however, all excepting that of No. 27 rise above the composite, but in the central region, at 128, 256, and 512 v.d., one-third of the fifteen thresholds pass beneath it. The normal variation of the point of keenest discrimination is well illustrated in this figure. Nos. 7 and 27 made the best record at 128, Nos. 9 and 14 at 256, and No. 5 at 512 v.d. In fact, all but one of the entire number of observers made their lowest record at one of these central levels.

These curves represent the results of observers who were the most unreliable. Very few of these values indicate the physiological threshold. One could not say that the high values at the extremes should be interpreted to mean that all of the observers in question were unable to perceive smaller differences on account of physiological incapacity. It is much more probable that the difficulty is psychological. Individuals of this type do not adapt themselves so readily to new situations under experimental control. When new adjustments must be made their work is relatively poor and continues on a low plane until time has been given for the proper adjustment after which their work may be on a par with that of individuals who adapt themselves more quickly to new situations.

Figure 5 represents the results of the six individuals who are most typical of the second group. All of these observers are men, but they are not of equal rank in previous work in discrimination of

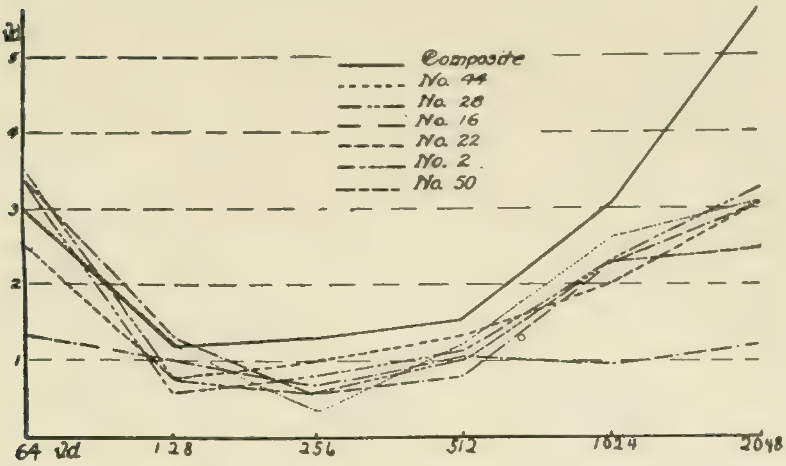

Fig. 5. The composite curve of the fifty observers and the individual curves of Nos. 2, 16, 22, 28, 44, and 50, which are characterized by a high degree of uniformity in the threshold values throughout the range.

pitch. Nos. 2, 32, and 48 were graduate students in psychology and were trained in other tests of discrimination; with the remaining three, however, previous training was very limited. The striking difference between these curves and those of the former group is, as would be expected, with reference to the extremes. The values for 64 and especially for 1024 and 2048 v.d. are lower than in curves of the first type; they approach, therefore, a more uniform level,—a goal which is most nearly approximated by No. 48. In contrast to the former group, these curves fall below the composite at practically every point; only four values are actually higher than the composite, while two more are equal, and these are at the lower limit. From the standpoint of consistency, the curves of Class II can easily be judged the better. Observers who give such results are reliable. With a state of secondary passive attention, they are able to meet the new situation in an easy and natural manner and are little disturbed by unusual difficulties which may be presented. In addition, exceptional ability in analyzing a problem enables them to select the proper element or elements upon which to base their judgments, even though there be disturbing factors. They are so consistent that the experimenter can feel a high degree of assurance that their records represent a close approach to the physiological threshold.

The curves of the five individuals who are most representative of the third group are shown in Figure 6. The peculiar character of these curves, in contrast to those already considered, lies in the lower limit. Here the thresholds are very much lower than for Class I

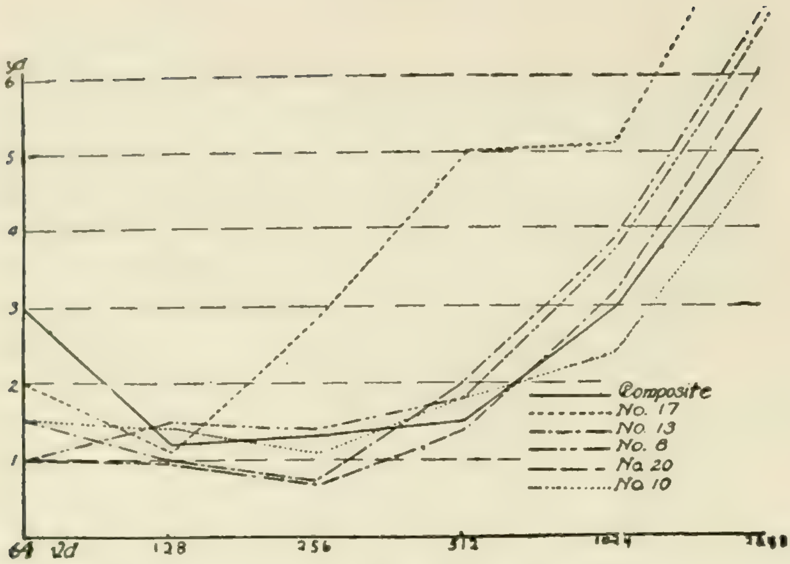

Fig. 6. The composite curve of the fifty observers and the individual curves of 8, 10, 13, 17, and 20, which show relatively low values at the lower limit and high values at the upper limit.

and considerably lower than they are for Class II. But in the upper extreme the curves are similar to those of the first group, with one exception,—the curve of No. 17. Indeed the average results of these five observers form a curve which closely approximates Stücker's curve, the essential difference being, that the latter is tilted at a slightly different angle, due to the fact that Stücker's thresholds at 73.4 v.d. are lower than ours and higher in the vicinity of 2048 v.d.

The similarity of our results to those of Stücker in the lower extreme might invite the same criticism which we advanced against him. It might be said that our low threshold at 64 v.d. was due to the discovery of some variable other than pitch upon which the judgment was based. There is, of course, the possibility that this occurred, but reference to Figure 7, in which the composite curves of the three groups may be compared, leads to the belief that such a criticism does not have much weight with respect to these particular observers. It is to be observed that the minimal thresholds of the first two groups lie at 256 v.d. with a gradual rise on either side of this point. The point of keenest discrimination for Class III, however, lies at 128 v.d. with here again a rise on either side proportional to that which we find in the other two groups. In other words, the form of the latter curve from 64 to 256 v.d. is similar to the form of the other two from 128 to 512 v.d. We should expect to find a higher threshold for 64 when the minimum is

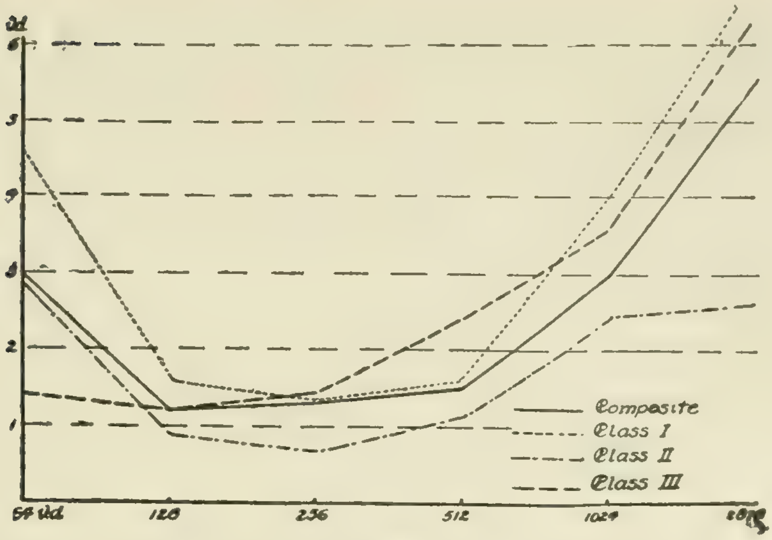

Fig. 7. The composite curves of the fifty observers and the three different classes.

at 256 than when it is at 128 v.d., and this is what occurs. With conditions such as they are, we are inclined to regard the results of this class of observers, at 64 v.d., as fairly accurate. The observers in question naturally do better work on the low tones. If such an interpretation is true, it would not be just to say that the curve of Class III is less consistent than the curve of Class II; each represents a different type and is consistent with itself throughout.

There are still to be considered the results of the thirteen observers which are not exactly comparable to any of the classes described above, because of certain irregularities occurring in their curves. It must be determined whether or not these ridges or elevations may be explained on the basis of daily fluctuations, in which case a sufficiently large number of observations would result in smooth curves, or whether the variations are due to natural weaknesses for the regions where they are found. The extent to which this latter explanation must be invoked, indicates an answer to the question of the frequency of gaps in the registers of individuals who have apparently normal hearing.

In Table I, the observers who have such curves are Nos. 4, 8, 11, 12, 18, 19, 25, 38, 40, 41, and 45. It will be noted, however, that at no place are the deviations from the normal very pronounced. All of them, excepting perhaps one, are doubtless due to certain factors which would have been eliminated by a large number of judgments. Daily variations, the relative amount of practice, and the accuracy with which the increments used corresponded to the true

thresholds, are the most important factors which have contributed toward the irregularities. With No. 5, for example, the experiment was begun at 256 v.d., so that the greater amount of practice would give the neighboring tones the advantage. With No. 11, the tone of 256 v.d. was first tried with a difference of 2 v.d., which was too large, resulting in an almost perfect series. Had the order in which these differences were presented been reversed, the threshold would probably have been very close to 1 v.d. Indeed, a number of these observers were given additional tests to determine whether or not these variations from the normal would hold. In each case as Table III will show, the curves became fairly smooth.

The curve of No. 4 is abnormal at 1024 v.d. in its relatively low threshold of 1.1 v.d. During an experimentation of one hour he made a record of eighty-eight per cent. on two hundred judgments with a difference of 2 v.d. But on the following day, a difference of 1 v.d. gave only fifty-two per cent. of the right cases. A larger number of observations would doubtless have resulted in a threshold more equal to that of the tone an octave lower.

But there is one observer, No. 18, who gives some evidence of a

TABLE III. Irregular results which additional observations have corrected

Observer	*64*	*128*	*256*	*512*	*1024*	*2048*
19	5.2	0.6	1.1	0.8	4.1	9.7
		0.6	0.6	1.1		
41	2.4	0.8	0.7	2.1	1.8	3.5
				1.8	3.5	
35	6.4	1.1	1.5	1.1	6.4	9.9
		1.8	2.2	2.6		
38	0.7	0.6	0.7	2.1	1.6	3.7
			1.1	.9	1.3	
12	3.7	0.7	1.5	2.4	4.3	3.5
				1.8	3.3	

slight weakness in the region of 256 v.d. His threshold at this point was derived from four hundred judgments. The first half of the number with a difference of 3 v.d. gave a threshold of 2.9 v.d., while the second half with a difference of 2 v.d. gave a threshold of 2.3 v.d. At no time was he able to approach a threshold of 1 v.d. On the other hand, with the tones above and below, he made low and consistent thresholds. It is difficult to account for this high threshold at 256 v.d.; the observer himself could offer nothing as a basis for explanation. The affective element, association and imagery, and inherent characteristics of volume and intensity may have played varying rôles in causing the discrepancy. At any rate the differ-

ences are not sufficiently great to be regarded as representing gaps.

We have found, then, from this study of the curves of discrimination of pitch of fifty normal observers no clear evidence of tonal gaps. The grosser irregularities which might arouse the suspicion of a gap are due to certain factors which have not been perfectly controlled. It is highly probable that with more extended observations the irregularities would have been eliminated. It must be kept in mind, however, that this conclusion has reference only to observers with apparently normal auditory capacity; with respect to individuals whose audition is unquestionably recognized as pathological, this study has nothing to offer.

Relation of Musical Training and Expression to Discrimination of Pitch.—The question naturally occurs in a study of this kind as to the nature and extent of the correlation between musical education and pitch-discrimination. It seemed obvious that if a correlation existed it would be between discrimination and musical expression rather than between discrimination and mere technical training. The Pearson method of rank difference was used to determine the correlation. The mean of the six levels in the range for each of thirty-eight observers gave a value for the ranking of the individuals according to their capacity for the discrimination of pitch. The records of the remaining twelve were not included as most of them were advanced students whose greater experience in work in the laboratory might possibly put them in a slightly better class, while with one or two others, information regarding their musical training was not at the time available. The ranking according to expression was not quite so simple. For this purpose an evaluation was made of the answers to the questionnaire, which was an exact duplicate of the one published by Professor Seashore in his Preliminary Report (13). To recall, there are three questions under the topic "Musical Expression": namely, (1) Favorite selections you can sing (by ear? by note?), (2) Favorite selections you can play (by ear? by note?), (3) Singing or playing in public (parts, occasions, etc.). The individuals were instructed to give as specific information as possible. But the comparison of the two functions showed no correlation whatever.

It was still believed, however, that there must be some difference between the discriminating capacity of those who seemed to be the

most musical and those who appeared to be the least, as far as previous experience was concerned. Again the questionnaire, to which reference has been made was resorted to, but this time the questions were designed to reveal the amount of training. They were as follows: (1) Musical training in public schools, (2) Private vocal lessons (when, where, how long, etc.), (3) Private instrumental lessons (when, where, how long, etc.). The observers were then equally grouped in two divisions, the first group consisting of the better ones in training and expression and the second of the poorer ones. The mean threshold for each group for the different levels is recorded below:

Table IV

V.d.	*64*	*126*	*256*	*512*	*1024*	*2048*
Group I	3.8	1.2	1.2	1.8	3.8	6.8
Group II	3.1	1.5	1.5	1.6	3.1	4.9

We find, then, that the group whose members have had greater musical education and more practice excel in capacity for discriminat-

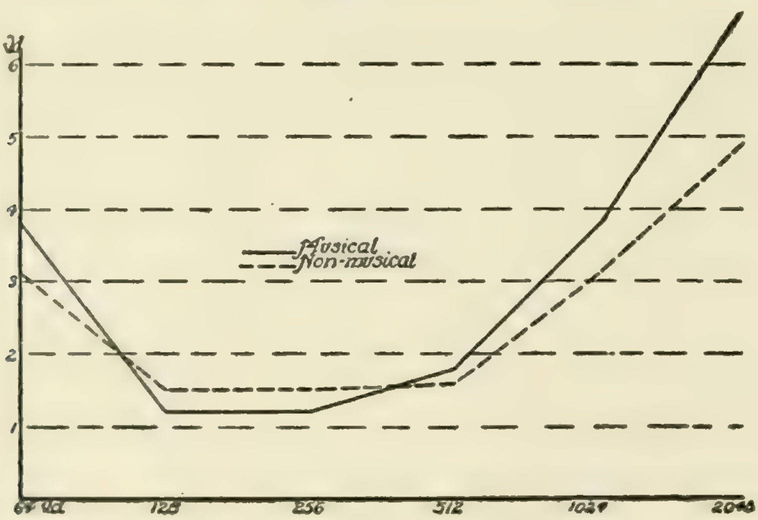

Fig. 8. Comparison of the musical and the non-musical.

ing pitch only at 128 and 256 v.d. But it is in this region that the above factors would have the most influence. Their effect upon the differential threshold of either extreme would be small because these tones are seldom used either in singing or playing. There is, then, some correlation between musical ability and discrimination of pitch in the central register. This is in general agreement with the conclusions of both Mount (6) and Smith (15) who found a fair degree of correlation between musical expression of pitch and dis-

crimination of pitch at 435 v.d. But we cannot agree with Stücker (17) in his assertion that musical observers, in general, show keener discrimination in the upper limit than the non-musical ones. Much depends upon the standard of classification for musical observers. Stücker may have reference only to observers of unusually fine ability in music. For such, his statement may be true, but for observers whose ability is not so exceptional it scarcely holds.

The frequency and distribution of the false judgments.—From the 39700 judgments it has been possible to determine definitely not only the frequency of the false judgments but also the way in which they have been distributed in the various levels. So large an amount of data should show whether or not there is a preference for one or the other order, and if so what relation this preference bears to sex, voice-register, and pitch.

In computing the number of errors, the results of sixty-two individuals, thirty-two women and thirty men, for at least three different tonal regions were available. The nature of the error in the wrong judgments at 64 v.d. was not recorded; the observer sat with closed eyes and gave oral judgments and the experimenter merely recorded the number of the errors. At the other levels, with one or two exceptions, the observer recorded *H* or *L* for each pair of tones. For 128, 256, 512, 1024, and 2048 v.d., then, the distribution of errors could be accurately studied. Just one-half of the sixty-two observers had a record for each of these five steps, for the other half discriminations were made at from three to four levels. Two different computations were therefore made, the first including the results of the entire number of observers and the second, only those which are complete for the five different levels. The total of the complete results could thus be used as a check upon the total of the incomplete results. Reference shall be made to the first, however, only in so far as it differs from the second.

TABLE V. Distribution of errors

Section 1

Computed from the results of sixty-two observers

A	B	C	D	E
128	8300	23.40	10.78	12.62
256	8600	25.13	13.31	11.82
512	10100	26.03	13.79	12.24
1024	8300	25.42	13.61	11.81
2048	4400	25.41	14.00	11.41
Total	39700	25.09	13.04	12.05

Section 2

Computed from the results of thirty men

128	4700	23.21	10.34	12.87
256	4800	25.00	12.83	12.17
512	6100	27.46	13.84	13.62
1024	4500	24.67	12.50	12.17
2048	2100	23.52	11.61	11.91
Total	22200	25.11	12.39	12.72

Section 3

Computed from the results of thirty-two women

128	3600	23.67	11.36	12.11
256	3800	25.29	13.92	11.37
512	4000	23.78	13.48	10.30
1024	3800	26.32	14.95	11.37
2048	2300	27.13	16.13	11.00
Total	17500	25.07	13.86	11.21

Table V is a record of the errors computed from the results of the total number of the observers. Column A represents the vibration-rate of the fork; B, the total number of judgments; C, the total percentage of error; D, the percentage of error when the second tone was lower; and E, the percentage of error when the second tone was higher. The greater number of judgments in the central register is due to the fact that irregularities occurring here necessitated further experimentation to determine whether they were due to subjective factors which were permanent or merely transient, or possibly to objective factors.

The final average of the percentage of right cases approaches to within .09 per cent. of the ideal of 75 per cent. When the different levels are considered collectively, the false judgments amount to 13.04 per cent. when the second tone is lower, and to 12.05 per cent. when the order of succession is reversed. There seems then to be a slight though not significant preference for the order in which the second tone is higher.

A difference is observable in the distribution of error at the various levels. At 128 v.d. more errors by 1.84 per cent. occur when the second tone is higher, but at the other levels there is a greater percentage of error with the opposite order. As shown in Table V the differences between Column D and E increase gradually from 256 to 2048 v.d. In the first computation, however, made from the complete results of a smaller number of observers, the order of second tone higher gives the smaller per cent. of error at each level. At 128 v.d., the difference in favor of this order is only .48 per cent., but

at 256 it amounts to 2.03 per cent., at 512. to 3.65 per cent., at 1024 to 2.68 per cent., and at 2048 v.d. to 2.46 per cent. On the average, then, judgments of difference in pitch are more accurate when the second tone is higher, *i.e.* given two successive tones of the same pitch, there is a slight tendency to hear the second as the higher, excepting at 128 v.d., where fewer errors are made when the reverse order is followed.

Difference of sex.—When the results are studied with respect to sex it is found that the above conclusion would not be valid for a group of individuals in which there was a much larger percentage of men than women.

In the study of differences of sex it is found that the women on the average, show a decided preference for the order in which the second tone is higher at every step except at 128 v.d., where the difference seems to be slightly in favor of the second tone lower. But it is at this latter level that the men show a very strong preference for the second tone lower, while in the other levels the difference in favor of either order is insignificant. This variation of sex affords additional evidence that normal illusions are greater with women than with men.

An arrangement of results according to voice registers of the observers brought out nothing new. The difference seems to be essentially between the voices of men and women. Had our observers been highly specialized singers, there might have been some difference showing itself in the different voice registers.

TABLE VI. Variation with sex

V.d.	*64*	*128*	*256*	*512*	*1024*	*2048*
20 women	3.2	1.2	1.4	1.7	3.8	6.6
16 men	2.7	1.1	1.0	1.6	2.4	4.8

The foregoing table and the accompanying figure show the results for the twenty women and the sixteen men who had a similar amount of training in experimental procedure. At first sight there seems to be a decided difference between the sexes, inasmuch as the thresholds for men are lower throughout the whole range than those for women. While there are differences in favor of the men, care must be taken not to attach too much significance to them. The differences at 1024 v.d., of 1.4 and at 2048 v.d. of 1.8, seem to be considerable, yet they are not much greater than should be expected when the total results are considered. At 256 v.d. the varia-

tion of .4 appears high when compared with the difference of .1 in the octave just above and just below. With such noticeable variation in the central region it is not so surprising to find much larger differences at the extremes where objective factors are not so well controlled. Smith (15) reports practically the same difference of sex as is shown in these results. He finds that at the ages of 17 to 20 and at maturity, the men surpass the women by an average of 0.3 v.d. at 435 v.d. It is evident that the men's curve presents a more satisfactory form than does that of the women, in that there is not so high a variation between the points of keenest discrimination and the

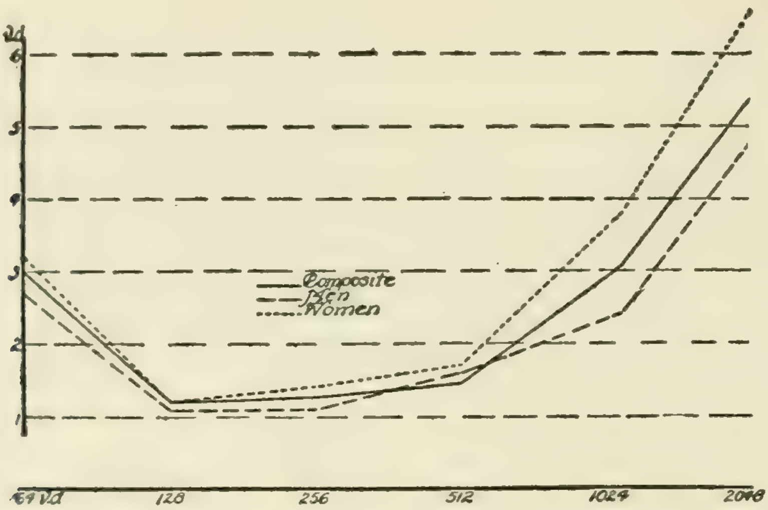

Fig. 9. The comparative curves of twenty women and sixteen men together with the composite of the fifty observers. (Table VI).

extremes. It may be that the cause of this arises from a possible inherent difference between the sexes in the method of meeting new situations. Or it may be that the men adapt themselves more quickly to experimental conditions and for this reason it has been easier to reach their physiological threshold.

Stücker (17) contends that the greatest sensitiveness to small differences of pitch lies with tenors and sopranos in the lower half of their voice registers, but with singers of bass and of alto parts, as a rule, in the upper half. In other words, the differences is not between the voices of men and the voices of women but between the relative height and depth of the voice register of both sexes. As none of my observers could be classed as professional singers, the results have little to offer either positively or negatively, in regard to Stücker's statement. Table VII indicates that only the results of

the soprano singers can be harmonized with the conclusion of Stücker. The finest sensitivity of the tenors is in the central part of their register and not in the lower, as he finds it to be; the basses made the best record in the lower part of their register, rather than in the upper; the baritones have done better in their register; and finally, the altos do better in the lower register and not in the upper. But these facts are not necessarily contradictory to Stücker's, inasmuch as the observers in this experiment represent only average ability as singers. One should have plotted the curves of a relatively large number of highly practiced singers before he would be able to add a conclusive word in answer to the problem which Stücker has suggested.

TABLE VII. Average thresholds classified according to voice register

Soprano	(16)	3.8	1.5	1.6	2.1	4.1	7.1
Tenor	(4)	4.1	1.5	1.2	1.2	2.3	4.3
Baritone	(15)	3.2	1.4	1.3	2.1	2.4	4.7
Alto	(8)	3.5	1.5	1.2	1.3	4.3	6.0
Bass	(7)	2.5	1.1	1.3	1.6	2.8	4.3

SUMMARY

(1) For individuals selected because of a slight superiority at 435 v.d., the composite absolute curve of pitch-discrimination within the limits of 64 and 2048 v.d. shows the keenest discrimination at 128 and 256 v.d. On either side of this central register, there is a rise in the curve which is relatively abrupt toward the lower limit but much more gradual toward the higher extreme.

(2) The relative curve takes the form of a continual decline from the lower to the higher limit. From 64 to 128 v.d. the decline is comparatively steep, but from 128 to 2048 v.d., it is very gradual, approaching approximately a horizontal line in the upper half of the register.

(3) Individual differences, factors which lead to confusion and to identification, and variation in practice and in attention are the principal conditions upon which the form of the curve depends. The variations in the curves of the different investigators are explainable on the basis of the varying degrees of influence of these conditions.

(4) Most of the individual curves conform more or less closely to one of the following types of curves; namely, (a) a curve in

which there is a relatively low value at some point in the central register and relatively high values at the extremes, (b) a curve in which the thresholds are fairly uniform throughout the entire range, and (c) one in which the threshold for 64 is considerably less than for 2048 v.d.

(5) There is very little evidence of tonal gaps. The grosser irregularities in a few curves, which at first seemed to indicate the presence of a gap, disappeared with more extended observations.

(6) A correlation between musical ability and discrimination of pitch occurs only in the central register.

(7) The women make more accurate judgments when the second tone is higher; their preference for this order increases in direct proportion to the pitch, within limits, excepting at 128 v.d. where the reverse order is slightly preferred. The men make fewer mistakes at 128 v.d. when the second tone is lower, but at the other levels no particular preference for either order of succession is observable.

(8) The men surpass the women in discrimination of pitch at every level in the register; this variation between the sexes is the greatest at the extremes.

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THE DURATION OF TONES, THE TIME INTERVAL, THE DIRECTION OF SOUND, DARKNESS AND QUIET, AND THE ORDER OF STIMULI IN PITCH DISCRIMINATION

BY

DAVID ALLEN ANDERSON

I. Most favorable duration of the tones

In this investigation to ascertain the relative favorableness of different durations of tone in pitch discrimination, the tones were produced by tuning forks from the "standard pitch discrimination set" as described by Professor Seashore (1) reenforced by Koenig adjustable resonators suspended behind a revolving slit-disc which was driven by a synchronous motor (2,3).

The tuning forks were tuned to an accuracy of ± .015 v.d. They were held firmly by the fingers near the end of the stem and energized by striking the middle of the prong lightly against a sounder made of ¾ in. lead pipe covered with a soft rubber tubing and resting on a leather cushion filled with sand. When they had been set in motion the forks were held directly in front of the mouths of the resonators during the passage of the open slits in the intervening revolving disc. Revolving discs made from cardboard, in which were slits cut in appropriate sectors, regulated the duration of tones and the interval between them. The disc proper prevented the passage of the vibrations from the forks to the resonators while the slits admitted of their free passage. The length of the slit determined the duration of the tone and the size of the sector between governed the length of the interval. When a slit passed a fork the resonator would take up the vibrations. The result was a clear and pure tone, clean cut at beginning and end. The intensity was kept as regular as possible without maintaining an identifiable uniformity. An effort was made to change the forks from hand to hand and to govern the duration of time between the energizing of the forks and the hearing of the tones in such a way that the observers could get no clue regarding the order in which the tones were to be given. Whether the higher or lower tone was to be given last was regulated by a key prepared beforehand according to chance, except that not more than three consecutive cases of one kind were allowed.

The two resonators were fastened side by side immediately behind the revolving disc.

The speed of the synchronous motor used in driving the revolving discs is controlled by a tuning fork and gives an accuracy in the time element far beyond the requirements of this experiment.

A time interval of ⅛ second was chosen arbitrarily and was kept constant throughout. Ten judgments constituted a group and ten groups or columns (100 trials) confined to one duration made up a set. When a group of ten judgments had been taken and recorded in a column, another group followed, and so on until the set was completed. When one set had been given it was followed by a set of another duration and so on throughout the series. Generally about four sets were given at a sitting.

The observers were Professor C. E. Seashore, and three graduate students, namely, G. H. Mount, L. E. Widen and W. R. Miles; all of whom were at the time pursuing experimental problems in the laboratory and had quite extended experience in observing tones. Each of them had a threshold of 1 v.d. or less on the basis of 75 per cent. correct judgments; hence 1 v.d. was used as the pitch interval throughout the tests. The observers were permitted to choose a location in the room which seemed favorable, and comfortable with the understanding that it was to be kept unchanged.[1] They listened to the two tones, judged the latter as higher or lower than the former, and recorded the decision (H for higher and L for lower).

TABLE I. Effect of differences in duration of tones
(Time interval ⅛ second; pitch interval 1 v.d.)

	S.			*Mo.*			*W.*			*Mi.*			*Ave.*
Duration	%	m.v.	n.	%	m.v.	n.	%	m.v.	n.	%	m.v.	n.	
⅛ sec.							72	2	500				71.6
¼ sec.	58	5	500	73	9	500	76	4	500	67	2	500	68.4
⅜ sec.	68	6	500	81	5	500	78	3	500	76	5	500	76.0
½ sec.	75	3	500	84	4	500	76	3	700	74	4	500	77.5
1 sec.	76	8	500	82	4	500	79	3	500	84	4	500	80.4
2 sec.	87	5	300							89	2	300	88.3
1st tone 2, 2nd, ½ sec.	77	1	200							86	2	200	82.0

%, per cent. of right cases; *n*, number of trials; *m.v.* mean variation for successive hundreds of trials.

[1] Two observers each made one change in position but their records in both positions were so distributed throughout the series as not to interfere with the results. The records of both were materially improved by the shift. The influence of position of the observer with reference to the origin of the tone is discussed later.

Table I shows a general tendency in favor of the shorter durations. There is practical uniformity in this general tendency among the several observers, there being but three steps that are exceptions: Mo.'s average at ½ second is higher than his average at 1 second, and W and Mi. make higher averages with a ⅜ second duration than when it is ½ second in length. However, the increase in ability with increase in duration of tone is comparatively small.

Introspections indicate that the most favorable feeling attending the hearing of tones of a certain duration may or may not parallel the percentage of correct judgments. It is also noted that when tones are of any long duration, judgment is usually made as soon as the essential character of the second tone is perceived without waiting for its cessation. Some tests were thereupon made on S and Mi. using durations of 2 seconds and ½ second in each couplet. This experiment consisted of two hundred judgments by each of the observers and resulted in a general average of 82 per cent. correct judgments. (See last line of Table I.). There seems to be therefore no advantage in making the second tone more than ½ second in duration.

Taking all things into consideration, it appears that the initial tone should have a duration of about 1 second, while the second tone need not exceed ½ second in duration. The demands for economy justify these limits even though longer intervals result in a slight increase in efficiency.

II. Most favorable time-interval between tones

The problem was to determine whether the time-interval between tones should be of a definite length and, if so, what it should be. In this problem the laboratory conditions, dates of experimenting, observers, apparatus, and methods were the same as in the preceding section. The discs were made so as to vary the interval and keep the duration uniform. The time intervals tested were 0, 1/16, ⅛, ¼, ½, 1, 2, 3, and 4 seconds. The duration of tones used was kept constant at ½ second.

Upon examination of Table II we notice that the gross averages for the several intervals are fairly uniform while the records of each individual vary widely at each step in the series. For this group the intervals tested are practically equally favorable, with possibly a slight tendency in favor of the shorter. Introspections

TABLE II. Effect of difference in time interval between tones

(Duration ½ sec.; pitch interval 1 v.d.)

Interval in seconds	*S.* %	m.v.	n.	*Mo.* %	m.v.	n.	*W.* %	m.v.	n.	*Mi.* %	m.v.	n.	Ave.
1/16 sec.	78	9	300				95	1	300	87	4	300	86.6
1/16 sec.	73		100	80	5	500	87	8	500	89	7	500	84.6
⅛ sec.	68	10	200	74	7	500	87	7	500	92	1	500	82.5
¼ sec.	63		100	81	5	500	87	6	500	90	2	500	84.7
½ sec.	61		100	78	5	500	87	6	500	91	4	500	83.4
1 sec.	74		100	75	5	500	83	7	500	84	7	500	80.4
2 sec.	67	4	200							86	1	200	76.7
3 sec.	71		100							80		100	80.5
4 sec.	78		100							78		100	78

%, per cent. of right cases; *n*, number of trials; *m.v.* mean variation for successive hundreds of trials.

indicate that conditions of stress, strain, annoyance and fatigue are experienced when long intervals are used. It is advisable to economize time and energy by adopting a short interval or by excluding it altogether.

The results here reported are in accord with, and supplement in so far as they cover the same ground, the studies of Wolf (4), Angell and Harwood (5), and Whipple (6), though these investigators were concerned chiefly with the influence of the time interval upon tone memory and imagery. The shortest interval tested by any of these experimenters was one second and comparatively few records were taken where they used an interval of less than two seconds. Their results are fairly uniform in showing the advantage in a short interval.

III. Most favorable direction of the sound

The problem was to ascertain whether any one direction of the source of the sound was most favorable in discrimination of pitch. The experiments were performed in July 1911 in the open air away from every influence of limiting walls or surfaces aside from the ground and the grass. The sounds were produced by tuning forks with resonators.

The observers were Professor Seashore, Jessica E. Strawbridge, T. F. Vance and E. T. Walker. The three mentioned were chosen from among the graduate students in the University of Iowa. All but one of the observers were practiced in pitch discrimination. A pitch interval of 1 v.d. was used with *S* and *St.* and 2 v.d. with *V* and *Wa.*

In the experimenting the observers were seated about ten feet from the source of the tone. Beginning in a position directly facing the source of the stimulus the observers turned to the left ninety degrees at a time so as to hear the tones exactly from the front, the right, the back and the left in successive series of trials. Making twenty judgments in each position, they continued to turn until one hundred judgments were made from each of the four directions. Table III gives the results in percentage of correct judgments of each individual in each of the four positions and the averages for the group, showing that in the open air there seems to be no significant effect of direction of the sound upon ability in pitch discrimination.

TABLE III. Effect of direction of sound

	S.	*St.*	*V.*	*Wa.*	*Ave.*
Front	71	64	56	72	65.7
Right	66	55	58	72	62.7
Back	64	53	63	69	62.2
Left	76	62	62	69	62.2

A similar set of experiments was then tried in a room 15 x 18 feet square with the observer in a selected series of positions with reference to direction of sound, relation to walls, and distance. On the whole the same conclusion was reached as for out-of-doors. However, strong individual preferences were expressed. There was no clear evidence of correlation between feeling of favorableness and actual ability.

IV. Darkness and quiet

The experiments herein reported were designed to ascertain the influence of occupying the "dark room" upon accuracy in judgments in pitch discrimination. The work was done in July 1911 in room No. 210. L. A. which is a "measurement room", (15 x 18 x 13 feet, well lighted and occupied by apparatus and laboratory furniture, quite resonant), and in the light, sound, and jar proof room described in Vol. III of these Studies (7).

The tuning forks with resonators were used as before, without the discs. The observers were Professor Seashore and graduate students F. O. Smith and E. T. Walker. The position of the observers with reference to the origin of the tones was kept uniform in the two rooms throughout the experimenting. With *S* a pitch-interval of 1 v.d. was used, with *Sm.* and *Wa.* 2 v.d. *Wa.* was an unpracticed observer.

Each observer made two hundred judgments in each of the two rooms. One hundred in the "dark room" were followed immediately by the same number in "the measurement room".

TABLE IV. Effect of darkness and quiet

	S.	*Sm.*	*Wa.*	*Ave.*
"Dark Room"	80	54	64	
	73	55	81	67.8
"Measurement Room"	75	51	56	
	75	62	75	65.7

The results indicate that accuracy in judgment is about equally favored in the two rooms. Introspection shows that while the silence and freedom from distraction of the dark room are soothing, they also make the observer more critical about the stimulus. As one observer said, "Any irregularity in the tones was extremely annoying in the dark room, but in room 210 the accessory sounds made the stimulus seem smooth and soft."

In view of these results, including introspections and observations, it appears that accuracy in judgment of pitch of clearly audible tones will be as high in an ordinary laboratory-room as in a quiet room. The freedom from distractions in the dark and quiet room has a soothing effect upon the observer but, owing to the absence of distracting influences, the observer in the quiet room detects minor qualities and characteristics of tones and methods of procedure which would not come to consciousness in the ordinary laboratory. The normal noises and lights of an ordinary room seem to soften and smooth the stimulus.

V. The order of stimuli

During the progress of the experiments reported in the preceding sections one observer noted that his first judgment in a column (group of ten made in rapid succession) nearly always designated the second tone as higher and that such judgment was frequently in error. In order to ascertain whether or not this was a general tendency, the experimenter examined his records of more than 15000 judgments made by eight observers and written down in 1517 columns of ten judgments each.[2] The recording of judgments in successive columns corresponded to the method of presenting tones in successive groups of ten trials each. The trials in the individual

[2] A few columns include twenty judgments each.

groups followed each other in rapid succession, but there was a slight pause between groups. Therefore the first judgment in each of the 1517 columns or groups is the only one now under consideration. Table V shows the record made by each of the eight observers.

TABLE V. Effect of order of high and low

Observers	Number of judgments	Percentage of errors when judging High	Low
S.	362	77	23
Sm.	20	86	12
St.	20	100	0
Mo.	450	50	50
W.	310	31	69
Mi.	295	64	36
Wa.	40	57	43
V.	20	33	67

Now by actual count of all errors made in the list of over 15000 judgments, there is found no important constant tendency, 50.8 per cent. of the errors falling on the side of low. Without any constant tendency the errors should be about equally distributed between high and low in Table V. But such is not the case with observers *S., Mo., W.* and *Mi.,* for whom the record is extensive; *S.* and *Mi.* tend to judge "high", *W.* to judge "low" and *Mo.* happens to be exactly neutral. For the other observers the records are too few to be of much significance. We must therefore conclude that there are probably fixed individual tendencies to judge high or low but there is no constant group tendency. The order of tones therefore remains a factor which must be kept under control in experimenting.

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THE EFFECT OF INTENSITY AND ORDER ON THE APPARENT PITCH OF TONES IN THE MIDDLE RANGE

BY

ROLLAND M. STEWART

In the study of various phases of pitch discrimination in this laboratory the problem of controlling the intensity of sound has proved persistent. The writer therefore undertook to ascertain some of the principal features of this tendency. The work was begun with two differential forks, standard 128 v.d., mounted on tripods and energized electrically. Before these, a Helmholtz resonator was suspended which could be swung freely into position in front of either fork. The intensity was judged subjectively by the experimenter and was varied by the distance of the resonator from a point of greatest efficiency in front of the fork. After some preliminary work this apparatus was abandoned on account of the disturbance coming from the interruption of the current. The forks were remounted on handles easy of manipulation and were struck on a sounder and presented alternately in front of the same resonator but these forks proved to be unwieldy and it was found practically impossible to avoid the error of identification. They were therefore abandoned again and it was decided to attempt the test with a set of standard forks at 435 v.d.[1]

The real difficulty with 128 v.d. forks lay in the fact that one fork was tuned by sliding weights. The final test was therefore made with a series of differential forks, standard 435, which were tuned permanently to given fixed increments in the making and had no adjustable parts. The forks were presented to the resonator at a uniform rate according to a pre-determined scheme of distribution of weak and strong sounds according to prepared keys. The keys used were known only to the experimenter and were prepared by determining the order of the weak and strong sounds by chance

[1] This incident has proved in the light of more recent work to be of far greater importance than was known at the time, since, as is shown in the accompanying article by Miss Hancock, the law of intensity effect is quite different at 128 v.d. from what it is at 435 v.d.

except that a sufficient number of substitutions were made to get the same number of trials of each kind.

Two keys were used. Key I called for 150 observations: 50 cases of the same pitch and varied intensity; 50 cases with a pitch increment and constant intensity; and 50 cases with a pitch increment and varied intensity—thus high and strong, high and weak, low and strong, low and weak.

Key II called for 192 observations, 32 of which were introduced to exclude or to determine a measure of the so-called time error, *i.e.*, a tendency to call a tone either high or low simply because it is the second tone of a pair. For these trials the same fork was sounded with the constant intensity in both presentations. The remaining 160 observations were divided equally among the four variations mentioned in Key II; namely, high strong, high weak, low strong, low weak.

All observations were made in the light, sound, and jar proof room. Trained observers were used for the most part. Three of these, *D, F* and *H,* had engaged in extensive practice in the study of the problem of pitch discrimination. Four, *A, C, E* and *G,* were working in related problems at this time. Only one observer, *B,* was inexperienced in this problem, but she proved to have a fine ear for pitch discrimination where the intensity was constant.

TABLE I. The effect of the intensity and the order of the tones

	Key I		Key II			
Obs.	Intensity Constant	Intensity Varied	Intensity Constant	Intensity Varied	Weak	Strong
A	77	76	90	86	83 L	67 H
B	95	76	96	81	74 H	70 H
C	80	80	100	77	76 H	57 L
D	75	74	73	76	61 L	63 L
E	75	60	100	66	89 H	96 L
F	63	68	90	80	60 L	58 H
G	84	71	90	57	67 L	67 L
H	77	62	100	87	67 H	52 H
Ave.	78	71	92	76		

In all about 7500 observations were taken as a basis for these records. Table I shows the effect of the variation of intensity on the judgment of pitch. The records are given separately for Key I and Key II for each observer; and, in the last two columns of the table the same records for the two keys are combined and distributed with reference to weak and strong. In the former the

numbers indicate the per cent. of right cases under the four conditions named and, in the latter, what per cent. of the weak and strong respectively were called high (H) or low (L) when actually equal in pitch.

Examination of these figures shows that the varying of the intensity causes confusion which results in a poorer record than when the intensity is constant. This is true in the records both under Key I and Key II. The ratio of the right cases for "intensity constant" as compared with "intensity varied" is on the average 78: 71 in Key I, and this tendency is true of all individuals except one (F) for Key I. For Key II the corresponding ratio of the per cent. of the right cases is 92: 76.

The analysis of the results with reference to weak and strong proves that, for these observers, there is no constant tendency to identify weak or strong and high or low. However, examination of the individual records shows that this is not due to the absence of constant tendency in individuals but rather to the balancing of opposite tendencies in different individuals of the group. The percentages in the table show these individual tendencies to be quite marked. On the whole four (A, B, D and F) have a constant tendency to call the weak low and the strong high; while the other four (C, E, G and H) show the reverse tendency.

Table II contains the distribution of the same records, showing other details in regard to the intensity-pitch illusion. The notation of the table is self-explanatory; the abbreviations HS, HW,

TABLE II. Redistribution of same data as in Table I

	Observers	A	B	C	D	E	F	G	H	Av.
Key I Intensity Constant	High called low	25	6	11	37	25	38	11	32	23
	Low called high	26	4	31	13	25	35	23	11	21
Key I Intensity Varied	HS called low	11	2	27	34	73	23	16	50	29
	HW called low	21	44	12	20	12	33	17	17	22
	LS called high	43	31	21	24	7	43	11	36	27
	LW called high	18	12	59	22	73	25	59	50	40
Key II Intensity Varied	HS called low	5	4	27	15	55	15	62	7	24
	HW called low	22	47	5	27	22	32	15	12	23
	LS called high	20	23	5	40	22	22	25	15	22
	LW called high	7	1	45	15	35	10	67	20	24
	Tendency of second tone	53H	74L	50	66H	69H	81L	62H	53H	

HS, HW, LS and LW stand for High Strong, High Weak, Low Strong, and Low Weak respectively.

LS, LW, standing respectively for high strong, high weak, low strong, low weak. A comparison of the strength of the illusion for each individual in the second and third horizontal sections of this table with the distribution of errors in the first horizontal section (for "intensity constant") shows that these personal equations are sufficiently large to be recognized as fairly prominent individual tendencies that must be taken into account in any comparison of the pitch of two tones.

The above named conclusions are fully substantiated by preliminary observations on 22 observers whose records are not included in the above table because they were taken under somewhat varying conditions. Of these 22 observers 9 called the weak high and 10 low; 10 called the strong low and 12 the strong high. Although the introspections were studied quite carefully, no satisfactory explanation could be found to show why these errors occur. It was first thought that the primary tendency was to identify strong with high and to assume that when the opposite tendency appeared this was due to a reaction, conscious or unconscious, as a correction to this tendency which the observer might expect in himself. But this interpretation is probably not true since it is shown in the article referred to above that for low tones the tendency is just the reverse. We are therefore left without any satisfactory interpretation of the phenomenon and with the impossibility of knowing what direction the illusion will take in a given individual.

The order of the sound was so distributed as to eliminate that source of error for the main purpose of this experiment. The bottom section of Table II shows that there is no constant tendency in the time-error for these observers as a group: there is about as strong tendency to call the second tone low as to call it high. It is, however, clear that quite marked individual tendencies exist as in the 74 per cent. low for *B*, the 81 per cent. low for *F*, the 66 per cent. high for *D* and the 69 per cent. high for *E*. The conclusions on this point in the foregoing article by Anderson (Section V) are thus sustained, both as to the divergence in the direction of the tendency and the characteristic magnitude of the error.

THE EFFECT OF THE INTENSITY OF SOUND UPON THE PITCH OF LOW TONES

BY

CLARA HANCOCK

In pitch discrimination tests, a difference of intensity in the sounds compared has proved so important a source of error as to require investigation. A series of experiments were conducted by Stewart, as reported in the foregoing article, (1), with forks of 435 v.d. for the purpose of discovering the effect of intensity variation. It was on his tests that Professor Seashore based the following statements in his preliminary report on, "The Measurement of Pitch Discrimination." (2).

"Extensive experiments show (1) that both trained and untrained observers may be influenced by intensity in their pitch judgment; (2) that, although there is a tendency among the untrained, especially the ignorant, to judge the loud tone the higher, it may work either way; (3) that the same individual may show one tendency at one time and the reverse at another; (4) that for trained observers the two tendencies are almost equal; and (5) that the tendency is more serious for large than for small intensity differences."

Later, during a series of experiments on accuracy in singing the tones of forks of from 109 to 308 v.d., Miles (3) found results that differed materially from those of Stewart. He found that an increase in intensity of the standard tone regularly caused a lowering in the pitch of the reproduction when that was of medium intensity, and that when the standard tone was presented with medium intensity, if it was reproduced loudly, it was sharped. This indicated that the effect of intensity on pitch discrimination might not be the same for low tones as for high ones, and that the conclusions of Stewart's experiments might not be applicable to tones of low pitch.

The following series of experiments were undertaken to determine whether or not, in the sounds of tuning forks of 128 v.d., a difference of intensity produces any more constant illusion as to pitch than in the higher tones.

A set of forks was tuned from 128 v.d. upward with intervals of 1, 2, 3, 5, 8, 12, 17, 23, 30, 38 and 47 v.d. Later it was found necessary to tune a few lower than the standard for corresponding increments. The range is from 123 to 175 v.d. In the first series of tests no resonators were used. The difference in intensity was controlled by the force with which the forks were struck on the sounder and by the distance they were held from the ear. The aim was to have the faint sound just loud enough to be distinctly heard as a tone, and the loud one as loud as possible without interfering with its quality as a tone. The method of limits was used. The standard, 128 v.d., was always made faint, and the variables loud. The sounds were presented in two orders in alternating series: (1) a series with the standard first, followed by the variable; and, (2) a series with the variable first, followed by the standard. With the last observers several series were given also with no difference in intensity, for the purpose of comparison. In these the order was variable.

The point in each series at which the observer's judgment changed from "high" to "low", or "low" to "high" was taken as the tone which, when loud, was perceived as equal to the standard 128 v.d. when faint. Many of the observers' judgments changed several times through a range of several vibrations, and in these cases the mean between the highest and the lowest change of judgments was used. As the range of uncertain judgments varied considerably among the different observers, that, as well as the amount of the error, is stated in the tables.

The results of this series of tests are shown in Table Ia. The average error made in comparing the loud sounds with the faint ones, and the average range through which the judgments were not constant or certain are indicated for each observer. When the faint sound (128 v.d.) was given first, a loud sound actually several vibrations higher was selected as of the same pitch. When the loud sound was given first, in five cases the average error was zero, and in one case a sound 1 v.d. lower than the standard was selected; in the case of the thirteen other observers the error was in the same direction as before. In both series, the average of the errors is 6 v.d. Most of the observers found it more difficult to judge when the faint sound was second than when it was first, and there is greater variation among the judgments.

TABLE I.

(a)

	Intensity Equal		Intensity Different			
			Standard first		Standard second	
Obs.	Error	(Range)	Error	(Range)	Error	(Range)
Mc.G	0	(3)	3	(9)	2	(7)
Go.	1	(3)	2	(6)	0	(2)
Ch.			6	(0)	7	(0)
Cu.	2	(5)	6	(5)	9	(8)
Bo.			9	(1)	7	(5)
Ar.			14	(4)	9	(2)
Ge.			13	(4)	11	(5)
On.			7	(12)	29	(10)
Va.			5	(3)	8	(3)
Th.			3	(1)	0	(2)
Sa.			8	(1)	6	(1)
Le.			1	(4)	1	(2)
Ba.			13	(8)	18	(8)
Pi.	0	(2)	4	(2)	2	(3)
Ma.	0	(2)	2	(0)	0	(1)
Gr.	0	(1)	4	(2)	0	(5)
So.	1	(3)	13	(7)	9	(5)
Dm.	0	(5)	8	(3)	1	(5)
Li.	0	(1)	5	(1)	0	(1)
S.			8	(0)	3	(0)

(b)

Li.	0	(3)	1	(7)	0	(1.5)
Cu.	0	(4)	3	(6)	4	(8)
Ma.	0	(0)	4	(0)	2	(0)
Mc.G.	0	(0)	4	(2)	2	(3)
Ge.	2	(2)	7	(0)	7	(0)
So.			9	(1)	9	(1)

(c)

Cu.	5
Ge.	1.5
Ma.	2
So.	3

There seemed to be a possibility that the disturbance caused by having the loud sound close to the ear affected the judgment of its pitch, and for that reason further tests under other conditions were given to several observers. The same forks were used, but a glass funnel was held to the ear to prevent any effect on the sound that might result from the shape of the ear lobe. The results are shown in Section b. While the error in judgment was considerably reduced for several observers, it was still in the same direction as before.

Further tests were given, using a resonator with the forks, so placed that the sound was in the median plane instead of close to

one ear. The method was changed in this series. Pairs of forks were selected of different intervals from .5 v.d. to 8 v.d. according to the judgments of the observer. Small intervals were used first, and these were increased until the judgments were given correctly and with certainty. In these tests, the observer knew that the same pair of forks was being used for several times in succession, the weak one being sometimes first and sometimes second; in this way he had an opportunity to verify or contradict a judgment in the following one. He did not know what forks were used, nor the results of the former experiments. The interval at which the judgments were "equal" or were about equally divided between "high" and "low", was taken as the amount of the observer's error. This varied from 1.5 to 5 v.d., and was in the same direction as the previous results. Further, to test the fact of the existence of the illusion, the same fork was sounded strong and weak; and also the pairs of forks used before were reversed, the lower being sounded louder. With the combinations reversed, the judgments were always correct, even with as small an interval as .5 v.d., indicating an apparent increase of the interval; when the same fork was used for the two sounds, the louder was judged to be low, except that one observer, Ma., in the case of one fork, called the sounds equal in pitch. Ma. also reported an apparent rise in the pitch of the loud fork as it was taken from the resonator.

These tests show that the illusion of the earlier tests was not due to a disturbance at the ear. A repetition of the test with one observer, Cu., in which the last method was used, but without the resonator, shows an illusion of 6 v.d., about the same as that in the first series of tests.

Both experienced and inexperienced observers were used, some with special ability and some without. There seems to be no correlation between the amount of the error and either practice or musical ability. Their only effect is to make the judgments more constant.

The illusion of pitch which is due to strength of tone is then clearly established for relatively low tones and found to be different from that of tones of the middle range. But what is the situation for high tones? Preliminary tests were made of eleven observers with 512 v.d. forks. They show, on the part of six observers, a decided illusion in the same direction as with 128 v.d. forks; one, Ma. (as in the 128 v.d. test) reported a change in pitch in the

same direction when the fork was brought to the resonator and removed from it. With three others there appears to be no illusion, and with one it is reversed, the strong tone being called higher. Similar tests on thirteen observers at 1024 v.d. show the same direction of the illusion as low tones in one case, a reversal of it in three cases, and no illusion in the other nine cases.

The conclusions of this investigation may therefore be summed up as follows:

(1). With forks of 128 v.d., a difference of intensity causes an illusion in pitch which is constant in direction, though variable in amount, a louder sound being judged lower than a faint one of the same pitch. The magnitude of the error seems to increase with increasing strength of tone, the average illusion for the differences in strength here used being about 6 v.d. Many observers show considerable confusion on account of the difficulty of comparing tones of different intensity. There is no constant relation between this confusion and the amount of the illusion. Musical ability, and experience seem to lessen the confusion, but not the amount of the error.

(2). At 512 v.d. the tendencies seem to be approximately as Stewart found them at 435 v.d., with a somewhat stronger tendency to judge the loud tone the lower. At 1024 v.d. the effect of intensity difference seems to be less disturbing than in lower tones.

(3). In general, difference in the intensity of tone is always a disturbing factor in pitch: the illusion is strong and constant in direction for relatively low tones; with rising pitch, it decreases and may vary in direction.

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THE MEASUREMENT OF TIME-SENSE AS AN ELEMENT IN THE SENSE OF RHYTHM

BY

FELIX BRUENE ROSS

The object of this series of experiments was to make an accurate measurement of the time sense as an element in the sense of rhythm, primarily for the purpose of standardizing a test for the measurement of individual differences.

It was necessary to procure some means of producing regularly recurrent sound stimuli and of varying the interval under control. Hitherto kymographs have been mostly used for the running of some sort of "time-sense apparatus", such *e.g.*, as that of Meumann. But it can be easily shown that a clock-work is not sufficiently accurate for the finer measurements of this kind.

We are fortunate in having in the laboratory a synchronous motor which very well fills the needs of this experiment. This motor has been described by Lorenz (1) and Seashore (2).

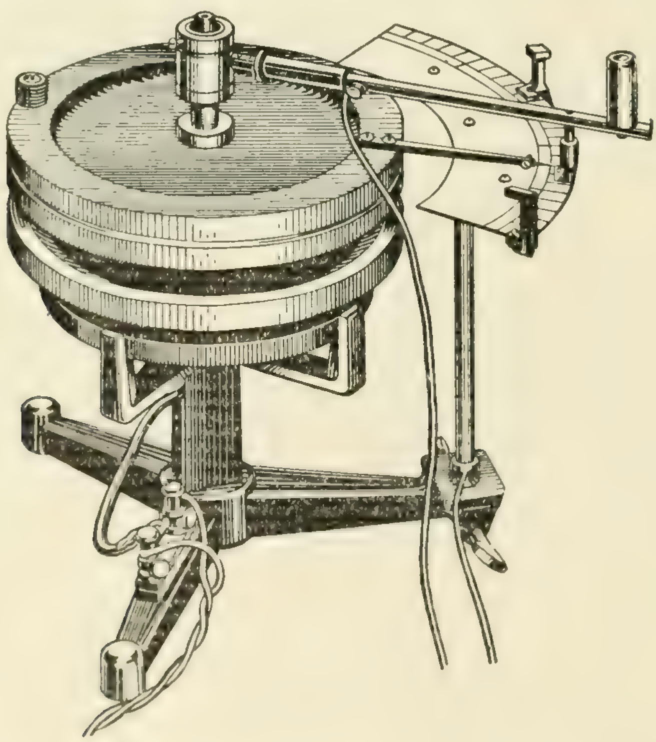


Fig. 1. The horizontal type of synchronous motor with rhythm attachment.

Fig. 1. shows the motor with the accessaries as here used. The top wheel is a balance wheel set on the axle of the motor proper. The pointer attached to this wheel makes contact with the knife-edge on the long movable and insulated arm. This arm may be set by hand for any point on the scale. To secure accuracy the two clamps on the scale plate are used as stops.

A telephone receiver was placed in the circuit of the above make-and-break contrivance in circuit with a battery. The momentary make-and-break of the current produced a distinctly audible and clear click which was used as the stimulus.

The balance wheel carrying the contact arm revolved clockwise at the rate of one revolution per second. If the long arm were held stationary the time interval would be constant,—one second. To change the interval it was only necessary to swing the arm through the required distance as indicated on the scale.

The motor with all the above mentioned accessaries was tested for accuracy by the spark method of recording and it was found that the limit of accuracy for the apparatus as thus operated is .0008 seconds.

The motor was located in a distant room and the telephone receiver connected with it suspended eight feet from the floor in the center of the room in which the measurement was made. A few preliminary trials were given in order that the observer, or observers, as the case might be, might have a clear understanding of the nature of the experiment. This done, a signal was given and the test began. A single test consisted in sounding ten clicks in succession with the understanding that the first five marked equal intervals but that in the last five there would be one short interval: and it was the task of the observer to detect this one. Seven different steps of change were used in successive groups of trials: namely, .02, .03, .05, .08, .12, .17 and .23 seconds. Twenty tests were given on each increment beginning with the largest and taking them in order. The right and wrong cases were counted and the records checked accordingly. The amount of deviation which would yield 75 per cent. correct judgments was computed, using only records between 65 and 90 per cent. right judgments. The average of the thresholds thus computed was taken for as many of the above steps as yielded records within the limits of 65 and 90 per cent.

The results here reported are based on four group measurements and a series of individual measurements. The group measurements were made in a large room into which noises from the halls and streets penetrated freely and proved a disturbing element. In the first three group measurements such disturbances as are characteristic of a large group were present. But in taking the fourth group measurement these objectionable features were eliminated by limiting the observers to a small number in this group. All individual measurements were taken in the dark-and-quiet room of the laboratory.

The results of the first group measurement taken on 200 observers, mostly sophomores, in two divisions of 100 each, are shown in Fig. 2. Those of the second and third, which were taken on 256

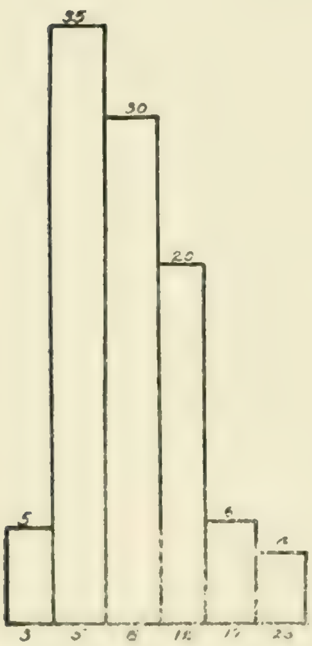


Fig. 2.

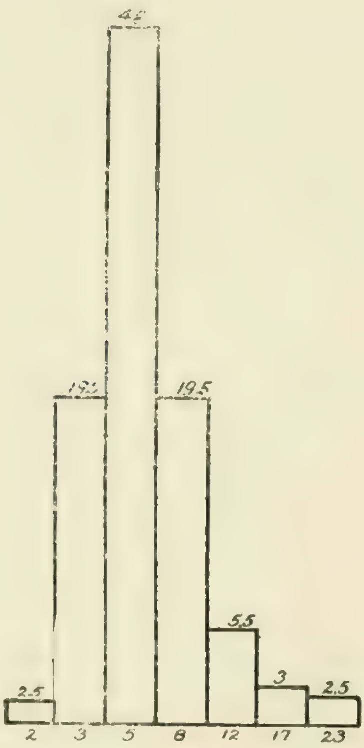


Fig. 3.

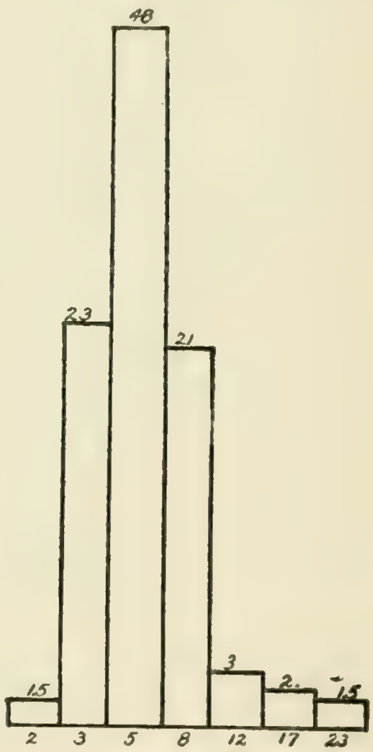
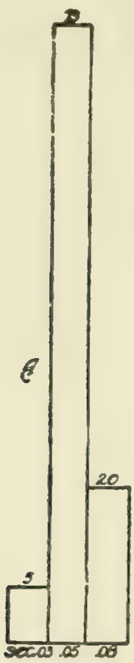


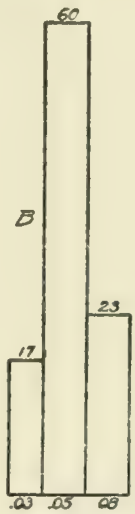
Fig. 4.

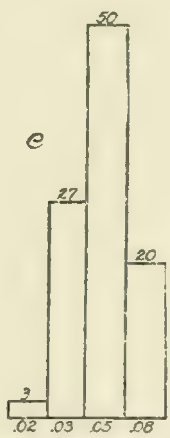
observers each, mostly sophomores, in two divisions each, with approximately an equal number in each division, are shown in Figs. 3 and 4 respectively. The distributions for these large groups of one hundred or more are all similar. This would indicate that this distribution is characteristic for different groups and for the same group at different times.

The third group measurement is a repetition of the second taken on the same persons as accurately as possible, and by the same method and under similar conditions, except those of practice, the object being to determine to what extent the distribution for a group is stable—the coefficient of correlation was computed and was found to be .69, P. E. r .027. This is a fairly satisfactory correlation considering that there are factors in the test not yet under control.

In order to determine the effect of the disturbances due to large groups, the same test was made on a group of thirteen in a small and relatively quiet class room. The result is shown in Fig. 5a.







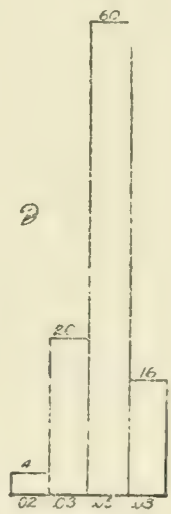


Fig. 5a. Fig. 5b. Fig. 5c. Fig. 5d.

Here the mode is the same (.05) as before but the extremes are eliminated. This group was an advanced class made up mostly of graduate students. The absence of poor records must therefore be attributed to two factors, namely, adaptation and skill in observing as a result of laboratory training and the more favorable conditions due to the smallness of the group. This points to the conclusion that in a large class of inexperienced observers most of the records poorer than .08 second may be due to lack of adaptation to the test or to disturbances in the room. The fine records on .02 second in a few cases of a large group may be due to the fact that in the larger group there are more chances of finding cases of exceptional ability.

To determine whether or not the distribution would be altered under even more rigorous and favorable conditions the same 13 observers were taken individually into the dark-and-quiet room where the test was made with most rigorous experimental control of the environment. The result is shown in Fig. 5b. which takes the same general form as 5a. The average is practically the same, .055 for the group and .054 for the individual tests. The Pearson coefficient of correlation between their individual records and those made in the group is .85, P. E. r .058, which would seem to indicate that both measurements are reliable.

To determine further the validity of the large group measurements 50 from the 200 in the first large group were taken for very careful individual measurements in the light and sound-proof room. The distribution for these is shown in Fig. 5c. The average threshold for these same observers in the group test was .07 second whereas their average in the individual tests was .05 second. This proves that the individual test is more reliable than the large group test because it gives finer records. The coefficient correlation between the two sets of measurements is however low, being only .23, P. E. r .07; that means that not all cases show improvement in the individual test and that there is relative instability in the records. That this instability is greater for the poorer records is proved by the fact that the average record for the best 20 in the above group of 50 was .054 second in the group and .047 second in the individual tests—a very small difference—and the correlation of the group and the individual records for these 20 best cases is .50 P. E. r .10 or more than twice as great as for the whole group of 50 cases. The significant thing in this is that it proves that ordinarily we may trust the finer records, whether in group or individual tests; the uncertainty is largely in the apparently poor records. This view is further supported by the distribution in that the better half of the surfaces of frequency is relatively stable for all the cases here considered.

It is still further supported by the results obtained from taking individual measurements of the 40 whose records were the very poorest in the last large group measurement. These results are shown in Fig. 5 d. The average threshold being .055, whereas in the group test, for the same 40 observers, it is .106. The correlation between the two sets of measurements is only .24, P. E. r, .085.

It is therefore probable that the poor records in the large group measurement are due to some cause other than a lack of appreciation of time on the part of the individual, and that this cause lies in the fact that with the large group there are within the group and peculiar to it sources of error that are not present in a small group or with the individual alone.

To determine the constancy of the record of an individual and the effect of practice, eight observers were given eight successive daily individual tests. After the first day in this series, only the two steps which were closest to the observers threshold of the day previous were used and in each of these 50 trials were made. The results are shown in Table I.

TABLE I. The effect of practice

Obs.	1	2	3	4	5	6	7	8	Ave.	% Gain
1	.051	.039	.039	.027	.030	.023	.024	.028	.032	45
2	.060	.063	.055	.032	.032	.032	.030	.030	.040	50
3	.052	.046	.042	.040	.040	.035	.037	.036	.041	29
4	.045	.047	.039	.032	.032	.028	.028	.025	.031	45
5	.039	.038	.039	.030	.040	.038	.039	.035	.037	11
6	.073	.064	.057	.050	.044	.043	.045	.044	.052	40
7	.040	.037	.040	.040	.034	.034	.039	.030	.037	25
8	.040	.038	.038	.037	.034	.034	.030	.029	.034	27
Ave.	.050	.047	.044	.036	.036	.033	.034	.031	.038	34

These eight observers were of approximately average ability, as may be seen by comparing the records for the first day with the form of distribution in the norms established for groups as shown above. In every case there was improvement as a result of the practice. The amount of gain for each individual, the average gain for the group for each successive day, and the daily fluctuations in the individual records may be seen in the Table.

This unquestionable gain with practice proves that the test was not elemental. The introspections also confirm this view, showing that the real difficulty is in the point of view, the method of imagery, the strain of attention, the method of counting, or some other such feature not essential to the simple experience of time sense. It is factors of this sort that form the basis for improvement by practice. To the extent that they are present this measurement, as a psychophysic test, is vitiated. Our aim is to make the test elemental, so that it shall be adapted for use in the measurements of individual difference. To this end the records and the observations in this series of measurements show that we must eliminate group distur-

bances and simplify the required mental attitude and strain to such extent that a test approximating the physiological limit shall be attainable without practice. As has been demonstrated in the above experiments, the first requirement may be complied with by making the test by individuals or in small groups in favorable surroundings. The second requirement must be met chiefly by simplifying the attitude of observation and the method of replying or recording. To do this our next step will be to take an individual test in which the interval-making sounds are not broken up into sets of ten, as here, or any other small group, but continued for a period of about five minutes each. This will secure better adaptation and will do away with the need of counting. Then the simplest sort of signal such as a motion of the hand may be used to designate those intervals which are recognized as short. Such simplifying of procedure should make this test approximately elemental. Norms are now being worked out on that basis.

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SOME STANDARDIZING TESTS ON STERN'S TONE VARIATOR[1]

BY

REUL H. SYLVESTER

The tone variator is a tone producing instrument of the blown bottle type. It was designed especially for use in gradual changes in pitch, and is made in different sizes. The one here used is of medium size and range, the bottle being 8 cm. in diameter and 17 cm. in length, with a mouth 1 cm. in diameter, giving a pitch of 200 v.d. to 400 v.d. The bottom of the bottle is a piston which moves upward and downward under the control of a gear and parobolic curve mechanism and can be set for different pitches according to a graduated scale. The tone is produced by a stream of air directed across the mouth of the bottle through a tube set at such an angle as to produce a whistling sound. The instrument is fully described by Stern,[2] who calls attention to some possible limitations to its accuracy, but offers no measure of reliability.

The experiments here reported were tests of the reliability of the tone variator, the intention being to use it, if it proved reliable, in studies of pitch discrimination. The constant air pressure was suppled by Whipple tanks.[3] Their pressure was regulated by weights and by a screw clamp applied to the soft rubber tube leading from the tanks to the variator. A water manometer, attached to this tube, gave the pressure readings. Where used in this report, these readings have been translated into grams per square centimeter. For reasons which will be given later, settings of the variator were always made in such a way that the movement of the piston was downward to the desired point, and by means of a special attachment the setting shaft control was firmly clamped each time. The pitch of the tone emitted was read by means of the

[1] These measurements were made in 1909. A synopsis of a report of them is given in the Proceedings of the Iowa Academy of Science for 1910, p. 195.

[2] Der Tonvariator. *Zeitschr. Physiol. u. Psychol. d. Sinn.*, 1902, XXX; 422-432.

[3] Whipple, G. M. A compressed Air Device of Acoustic and General Laboratory Work. Amer. Jour. of Psych. 1902, XIV p. 107 ff.

tonoscope.[4] In order to eliminate the interference at the mouth of the bottle by the funnel of the manometric capsule on the tonoscope, the electrical form of transmission, the phonette,[5] was used. Clamped with its lower edge in contact with the shoulder of the variator body, the phonette receiver faced the mouth of the variator in a position at right angles to the direction of the stream of air.

This apparatus, though complicated, was easily managed by one operator. With the tonoscope running, batteries turned on and the variator set at the desired pitch, the Whipple tanks were started and the taking of records began. After twenty readings had been recorded, the variator was set at another pitch and the record taking continued. With the exception of the resetting of the variator and the shifting of the tank weights, the apparatus ran continuously during two hours of experimentation. The pitch was read in tenths of a vibration, with accuracy. Every effort was made to secure the most favorable conditions for accuracy. It is not probable that the variator would be operated with such care in ordinary use.

Preliminary tests indicated that besides the changes in pitch which can be made in the intended way by moving the piston, variations of more than 15 v.d. may be made by varying the pressure of the air stream, and 10 v.d. variations may be made by changing the position of the mouth-piece. The results of experiments planned for a study of these two features are given in Table I, which shows the pitch variations for four scale readings, three pressures, and three mouth-piece positions. The limits of pressure and mouth-piece gap were determined by preliminary tests in which it was found that the extremes used here are the maximum ones at which the variator gives a fairly steady tone. Each pitch record given in this table is the average of between 40 and 50 readings. To this rather small amount of data, statistical checks of variability cannot be closely applied, but the mean variations given in the table are of considerable value. Their irregularity is largely due to the varying behavior of air currents in different settings, pressure, and mouth-piece gap combinations, some combinations causing more fluctuations than others. No records are given for the 400 v.d. setting because with the mouth-piece in the "flush" position there is a hissing sound which partly obscures the real tone and makes

[4] Seashore, The Tonoscope. (In this Volume.)

[5] General Acousticon Company, New York.

its pitch fluctuate, and because when used at low air stream pressure and wide gap mouth-piece position no sound is produced.

TABLE I. Tonoscope readings for the various combinations of pressures, settings, and mouth-piece gaps.

		3.8 mm. gap		1.9 mm. gap		"flush"		Average	
Pressure	Setting	Pitch	m.v.	Pitch	m.v.	Pitch	m.v.	Pitch	m.v.
2 gm.	200	187.9	.1	192.2	.2	193.8	.5		
	250	239.6	.1	242.4	.3	244.3	.3		
	300	288.0	.3	290.1	.3	292.4	.7		
	350	334.4	.4	340.9	.4	341.1	.7		
Average		262	.2	266	.3	268	.5	265	.3
3 gm.	200	191.7	.1	197.2	.3	202.1	1.2		
	250	243.5	.1	248.1	.3	251.1	.5		
	300	293.4	.3	296.9	.2	299.2	.6		
	350	345.4	.3	349.1	.6	345.3	.5		
Average		268	.2	273	.4	274	.7	272	.4
4 gm.	200	192.3	.1	201.3	.5	209.0	1.3		
	250	244.2	.1	250.9	.3	255.9	.4		
	300	294.5	.1	298.3	.4	302.4	.6		
	350	345.3	.2	352.0	.1	350.5	.8		
Average		269	.1	276	.3	279	.8	275	.5
Grand average		266	.2	272	.4	274	.7		

Inspection of this table indicates that the nine combinations of different pressures and mouth-piece positions, the 4 gm. pressure and a 1.9 mm. mouth-piece gap gives tones with pitches that are nearest the setting scale readings. The pitches, 201.3, 250.9, 298.3, and 352.0 vary from their settings of 200, 250, 300, and 350 an average of less than 1.5 v.d., while the averages of the other eight combinations vary from 2.2 v.d. to 10.1 v.d. from their settings. Therefore the intention of the designer of this instrument must have been that it be used with somewhere near 4 gm. pressure and a 1.9 mm. mouth-piece gap. If however, one disregards closeness to the setting scale pitch, he finds a lower pressure and a wider mouth-piece gap to be more desirable. With this 4 gm. pressure and 1.9 mm. gap the tone sounds forced and there is a prominent hissing in it, especially at the higher pitch settings. The tonoscope shows that under these conditions the pitch continually fluctuates. Obviously this is responsible for some of the larger m.v.'s in the table, it being impossible to catch the readings on the same phase of the fluctuations. When the mouth-piece is in the widest gap position, the pitch fluctuates least, the m.v.'s are least and to the ear the tones are the most clear, smooth and pure. Therefore this wide gap is more reliable than the 1.9 mm. gap. Evidence in favor of the lower pres-

sures is not found in the m.v.'s but to the ear the tones are clearer and purer.

Careful study of the table reveals no important tendencies that are obscured by condensing the data into averages. Hence the use of averages in the following consideration. They show first that an increase of pressure causes a rise in pitch. This is more marked in the lighter pressures, an average rise of 7 v.d. (265 v.d. to 272 v.d.) resulting from changing the pressure from 2 gm. to 3 gm. while, from changing the pressure from 3 gm. to 4 gm. the rise in pitch is but 3 v.d. (272 v.d. to 275 v.d). Averages in the table also show that when the mouth-piece is at the widest gap position at which a sound can be produced, the tone is comparatively low and changing the mouth-piece toward the "flush" position raises the pitch. This is more marked in the wider gap positions, an average rise of 8 v.d. (266 v.d. to 272 v.d.) resulting from changing it from a 3.8 mm. gap to a 1.9 mm. gap, while for changing it from a 1.9 mm. gap to "flush", the rise in pitch is but 2 v.d. (272 v.d. to 274 v.d.).

Mention has been made of the varying behavior of air currents at different settings. For this size bottle there seems to be the least disturbance at the 250 v.d. setting. Again using averages (and these averages as well do not smooth out or bury any important tendencies) one finds that for the 250 v.d. setting, the average of all records is 247 v.d. with a m.v. of .3 v.d. and that for the settings 200, 300, and 350, the averages of all records are 196 (m.v. .5), 295 (m.v. .4) and 345 (m.v. .5) respectively. In other words the pitch varies less from the scale reading than do the pitches produced at the other settings, and the mean variation is less. Hence the conclusion that a variator of this size is most reliable with the length of air column which gives a pitch near 250 v.d.

So much for conclusions from quantitative results. Certain general observations should be added. The manner in which the instrument's mouth-piece is attached is very unsatisfactory. It should be absolutely firm, accurately adjustable, and provided with a setting scale showing the width of the gap, or in some other way indicating the position. The piston sometimes settles downward. For the above experiments it was necessary to fit it with a clamping device. The securing of a steady stream of air is a serious problem. As shown in the tables, a slight pressure change causes a consider-

able change in pitch. The Whipple tanks are perhaps the best contrivance available, but they demand close care and are at best an occasional hindrance to the experimenting.

From the results of these tests, it is obvious that the tone variator can be relied upon only as an instrument of approximate pitch and relative intervals, and that it is not suitable for use in research which requires accuracy in pitch. The fact that its pitch varies with pressure and mouth-piece position, and probably with temperature, humidity, and other conditions makes an absolute reading impossible. A variation in one of these conditions would throw any scale out of proportion. But even if it cannot be relied upon for careful quantitative work, it is a desirable piece of apparatus for the psychological laboratory. Its loud clear tone and its ready manner of changing pitch make it especially valuable for general class experiments and demonstrations of consonance, beats and combination tones.

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CONTENTS

DESCRIPTION OF AN UNUSUAL CASE OF PARTIAL COLOR BLINDNESS

BY

MABEL CLARE WILLIAMS

CONTENTS

Introduction

The subject of this study will be referred to as J. H. He was a student in the University of Iowa during the year 1914-1915. His matriculation card gives as the place and date of his birth: Edinburgh, Scotland, April 27, 1890. He is of medium height and weight, in good physical health, and shows no evidence of disease or condition that would in any way influence his vision. He does not use tobacco, alcohol, or other drugs. His eyes are light blue in color with slightly bluish sclera, well set and wholly "normal" in appearance. For constant use he wears spectacles with peritoric lenses, spheres +1.25 dioptres over each eye, fitted in London without cycloplegic. Dr. Heard, physician in charge of the ophthalmic clinic in the College of Medicine in the University of Iowa, re-examined J. H.'s eyes, also without cycloplegic, and reports "vision right eye 6/3, left eye 6/3, showing him to be a hyperope since he reads at 6

meters what he should read at 3, and he reads this equally well with the +1.25 spheres. His accommodation is evidently very active. Since he refused a cycloplegic, this is only a measure of his manifest, not his latent, hyperopia. Ophthalmic examination reveals no astigmatic error. Fundi those of the typical hyperope. Retina granular in appearance due to pigment. Arteries not tortuous. Media clear."

Little family history of a satisfactory sort is available. His father, who died when J. H. was fifteen years old, was known to be color blind (letter from his widow, the mother of J. H. and herself not color blind) but there is nothing to indicate the type. Of the three brothers of J. H., two had probably normal color vision and in general appearance are said to have resembled the mother; while the third brother, A. H., and J. H. himself, resembled strongly the (color blind) father and were themselves color blind. The brother A. H., recently dead, apparently exhibited the same sort of color defect as J. H. As boys and young men together they often compared colors of objects and agreed always with each other but not with the other two brothers, only one of whom is now living. The two sisters are not color blind so far as J. H. knows. J. H. is the only member of the family who has been in this country.

J. H. has at all times been a most satisfactory observer. He was very careful and painstaking and unusually accurate, due in part perhaps to extensive training in biology and chemistry. He has done much microscopic work. Considering the fact that he has never been shown or told any of the results of the experiments even to the present time, his coöperation and willingness are all the more praiseworthy. It is therefore most pleasing to the writer to make public acknowledgement of indebtedness to him and to thank him. He began the experiments with the pre-established conviction that he was blind to blue and yellow, and was allowed to continue in that belief without discussion.

Attention was first called to the case in March 1915 through a mutual friend, Mr. C. J. Knock, fellow in the department of psychology at Iowa. It was noticed that only two color names

were used, "red" and "green", and that "red" was applied indifferently to reds and yellows and "green" to greens and blues. There has never been any noteworthy variation from this throughout the whole period of experimentation. In connection with hospital work, he once had to diagnose with the attending physician a case of "yellow jaundice". The patient looked "red" to him and very queer. He thought she must be an American Indian, but the features were not right and she had light hair. "Her skin and eyes were red and she was a funny looking woman."

Experimental work of a preliminary sort was begun in the Iowa Psychological Laboratory on March 5th and continued at frequent intervals until June, when J. H. had suddenly to leave. Fortunately he was again available for study two weeks in December, 1915. Since the resources of the Iowa Laboratories had been practically exhausted and it was now advisable to work with spectral light, the writer went to Chicago to canvass the situation there. Professor Carr at that time suggested that the best opportunity for continuing the work was to be found in the Nela Research Laboratory of the National Lamp Works of the General Electric Company at Cleveland, Ohio. Accordingly, invitation was sought from the Director of the Laboratory, Dr. E. P. Hyde, and most cordially extended by him. The writer went to Cleveland early in December to prepare in advance for J. H. who arrived from Missouri on December 19th and remained until the night of January 1st, 1916. Traveling expenses for both were generously provided from the research funds of the Iowa Graduate College.

At the Nela Research Laboratory little could have been accomplished in so short a time without the considerate aid of Dr. H. M. Johnson and Mr. M. Luckiesh, to both of whom the writer is most deeply indebted. Dr. Cobb, Dr. Troland and other members of the staff contributed in less extensive but very important ways. Every facility possessed by the Laboratory was lent unreservedly. A valuable instrument (Hilger constant deviation wave-length spectrometer) was borrowed

from Dr. Dunlap of Johns Hopkins and filled an immediate need.

The report of this case, though apparently extensive, should be regarded as a preliminary survey; and it is hoped that at some future time the study may be continued at the Nela Research Laboratory. The color names as used by J. H. have been quite disregarded. Control observers, usually three in number, were used in all tests where relative values only were obtainable. For the most part the several experiments are reported in their chronological order. The tests have all been conducted by the writer and are reliable to the extent claimed.

Color matching. (Colored worsteds)

A large well-mixed assortment of worsted skeins of many colors was spread out in good day light, on a table, over a large square of grey cloth. These skeins were not numbered and will be referred to by color only. No color names were used by examiner or subject.

(A) *Red* standard. A rich fairly dark red skein was handed to J. H. and he was asked to select from the pile all those skeins which resembled it in color but not necessarily in brightness. He selected out all reds and yellows, some sixteen skeins in all. He was then asked to re-sort these selected skeins according to two samples, the original red and a good yellow which he had included as a match for the red. He tried this, but gave it up as impossible.

(B) *Green* standard. Following the same procedure he selected to match the green sample thirty-two blues and greens of varying brightness and saturation. No red or reddish skeins were chosen. Given two samples, green and blue, and asked to re-sort his selections into two piles he again failed.

(C) *Yellow* standard. J. H. selected to match the yellow skein, the same skeins which he had sorted out to match the red and which had been returned to the main assortment. He added however a few more reds and yellows.

(D) *Blue* standard. The matches for the blue were identical with those for the green sample.

(E) *Grey.* Without sample, he was asked to select some "greys". Fifteen skeins were chosen, five of which were nearly true greys, eight were tans, and two were brown.

Color recognition. (*Nagel cards. Fifth edition*)

The available set of cards was old and somewhat soiled and perhaps faded. The results of the test should not be too rigidly interpreted. In the first four experiments (A, B, C, D) the regular procedure was followed.

(A) Select the card with red or reddish dots: A 12, A 6, A 1 were selected in the order given.

(B) Only red spots: A 3, and possibly A 15. He was not sure about A 15.

(C) Only green spots: A 5, A 13. He called the greyish spots on A 13 "green."

(D) Only grey spots: A 6. He also accepted A 9 after his attention was called to it and indicated the true grey spots on A 6 when asked to do so.

(E) For J. H., card B 1 consists of "plain red" (the shades of yellow) and "dark red or brownish".

(F) On card B 2 all the spots are "red, but the bright ones are redder; the others are brownish".

(G) Point out all the A cards with red spots: 6, 15, 12, 14, 3, 1, 11 (the yellow spots), 7, 16, 8, 10. Of the remaining A cards, "5 is green, 4 has green spots (correctly indicated), 13 is green with the greys showing a trace of green; 9 has shades of very dark green."

Color equations. (*Color mixing by rotation*)

Disks of two sizes, 5.8 and 9.6 cm. radii, were cut from Hering colored papers: red (old No. 1), green (old No. 7, new No. 9), blue (old No. 10, new No. 13), yellow (old No. 5, new No. 5), and black and white. Care was taken to guard against fatigue and an exact match was insisted upon. A small disk of single color was mounted over large disks in combination and the following equations were secured.

(A) Red 360° = yellow 270° + black 90°

(B) Green $360° =$ blue $226° +$ white $134°$
(C) Blue $360° =$ green $132° +$ white $21° +$ black $207°$
(D) Yellow $360° =$ red $257° +$ white $45° +$ black $58°$

The four colors were then combined in pairs to match grey (black and white).

(E) Blue $279° +$ red $81° =$ white $51° +$ black $309°$
(F) Green $222° +$ red $138° =$ white $111° +$ black $249°$
(G) Yellow $173° +$ blue $187° =$ white $150° +$ black $210°$
(H) Yellow $167° +$ green $193° =$ white $129° +$ black $231°$

In the three experiments thus far described, the results of the Nagel test seem not to be very decisive. In the conditions under which they were conducted, their chief significance is in indicating that some defect in color vision is obviously present. Too much stress is placed upon the color names which J. H. can apply successfully to red and green. The worsted test is suggestive and quite corroborated by the color equation tests. J. H. evidently has his color system reduced to two processes: that which conditions what he calls "red" and applies consistently to both red and yellow; and that which conditions what he calls "green" and applies consistently to green and blue. Real red and green are not confused nor are yellow and blue. These color processes are apparently physiologically antagonistic since a match for grey may be produced by proper combinations on the color wheel. The darkest grey is matched by blue (called "green") and red (called "red"); the next lighter grey is matched by green ("green") and red ("red"); the next lighter by yellow ("red") and green ("green"); and the lightest grey by yellow ("red") and blue ("green"). One would naturally expect the combination of yellow and green to require the most luminous grey as its equivalent. This discrepancy is probably in part due to the fact that only one trial was made and, in spite of frequent rests, fatigue may have entered, as all four of the grey equivalents were made at one rather tedious sitting and the yellow-green combination was tried last. Subsequent experiments (luminousity curves) show that for day-light adaptation, green is much nearer to spectral yellow in luminousity than is blue.

Color sensitivity. (*Colored rings on white rotating disks*)

This is essentially the same test described by Hayes, (1). The disks, 9.6 cm. in radius, were somewhat larger than those made by Hayes. They were cut from white bond paper, several thicknesses of which were used to avoid transparency. The rings of color (Hering papers red, green, yellow and blue as previously described and grey No. 9 from the Hering 50 greys) were 1 cm. wide, and were pasted smoothly midway between the center and the periphery of the disks. It was not possible to keep all the four colors and grey on the wheel at once[1] so each color was paired with the same grey and the observer was asked to decide whether a given color or grey was exposed. More foreknowledge on the part of the observer was implied here than in Hayes' tests and the threshold values are probably on that account lowered. Chance determined whether a portion of a colored ring or of the grey should be exposed. Records were obtained also from three control observers, Dr. R. H. Sylvester, Mr. C. J. Knock, and Mr. P. J. Sodergren, all members of the psychology department at Iowa. The results which represent the minimal exposure of color necessary for recognition, are stated in degrees. Ten determinations were made on each color paired with grey for J. H. and five for the controls. The averages and mean variations are presented in Table I.

TABLE I

	J.H.		R.H.S.		C.J.K.		P.J.S.	
	Ave.	m. v.	Ave.	m. v.	Ave.	m. v.	Ave.	m. v.
Red or	11.2°	1.8°	2.7°	0.2°	2.0°	0.2°	3.0°	0.8°
Grey	12.0	0.8	2.6	0.7	4.0	0.6	3.4	0.5
Green	9.1	0.9	2.5	0.0	3.2	1.4	3.6	0.5
Grey	11.2	1.6	2.7	0.2	2.1	0.7	3.6	0.5
Blue	12.3	2.5	23.0	5.6	2.3	0.4	6.2	1.8
Grey	18.3	5.0	6.8	2.7	4.0	1.6	4.6	1.6
Yellow	36.2	4.2	4.2	0.9	4.1	1.4	5.6	1.3
Grey	14.0	1.4	4.0	0.4	3.7	1.2	4.1	1.1

[1] This was actually done for J. H. however, and when he did not know what to expect, red was recognized at 13°, green at 21°, yellow ("red") at 31°, and blue ("green") at 33°. Grey was called "dirty white" at 10°.

The results of this test, which should be regarded as having relative value only, indicate a lesser degree of sensitiveness for the color stimuli in the case of J. H., as compared with the three control observers. The only exception to this is with the blue for R. H. S., whose readings for this color scattered widely and were very uncertain.

Color discrimination. (*The Lovibond Tintometer,* 2)

This instrument offers a ready means for measuring color sensitivity and discrimination when pure color stimuli are not demanded. The colored glass ray filters which are provided with the instrument are carefully standardized according to an arbitrary scale, and the units are recoverable, convenient and fairly permanent. The filters are not even approximately monochromatic, however, although to the eye the transmitted hues appear to be fairly pure.

The tintometer was set up where north daylight fell directly upon its windows. A ground-glass strip was placed between the slips and the source of light as an aid in securing a homogeneous surface. It was necessary to compound the glass slips in order to secure the proper increments from the available assortment. Four or even five slips could be bound together quickly with a rubber band and handled as easily as one. Each slip doubtless cut down the intensity of the transmitted light, for even clear glass will do this to the extent of 8%. A clear or white glass was always used in connection with the standard to help equalize this effect. Lateral light was excluded from the observers' eyes by means of the hood of the instrument. The colors were viewed with both eyes open.

A simple form of the method of constant stimuli was found satisfactory. Two slips, say red 10, were placed in two adjacent windows of the instrument, then, after a few minutes of skirmishing, the increment was found which when added to one of the windows containing red 10, made it just perceptibly "redder" than the other. A difference which yielded 80% of correct judgments was regarded as the approximate threshold. This increment was then fixed to one of the red standard slips

and one hundred comparison judgments were made. The right and left positions of the two stimuli were determined in accordance with a chance key already prepared. The observer either closed his eyes or looked away while the slips were being shifted and always pronounced judgment quickly without staring at the colors. From the number of correct judgments and the size of the increment, the discrimination threshold which probably would yield 75% of correct judgments was computed by means of the well known Fullerton-Cattell table.

In Table II are given: the initials of the observers, J. H. and three controls; the composition of the two stimuli compared, St. 1 and St. 2; the threshold increment in terms of arbitrary tintometer units; the percentage of correct judgments in one hundred trials; and the probable threshold for each of the four colors. It should be remembered that J. H. identifies yellow with red and blue with green.

TABLE II

Observer	St. 1	St. 2	Incr.	%	75%
		RED			
J.H.	R 10 + clear	R 10 + 5	5.0	85	3.20
R.H.S.	R 10 + clear	R 10 + .2 + .1	.3	83	.21
C.J.K.	R 10 + clear	R 10 + .1 + .1	.2	83	.14
P.J.S.	R 10 + clear	R 10 + .1	.1	83	.07
		GREEN			
J.H.	B 10 + Y 10 + B5 + Y5	B10 + Y10 + B10 + Y10	5.0	72	5.80
R.H.S.	B 10 + Y 10	B10 + Y10 + B1 + Y1	1.0	89	.60
C.J.K.	B 10 + Y 10 + clear	B10 + Y10 + B.3 + Y.3	.3	88	.17
P.J.S.	B 10 + Y 10 + clear	B10 + Y10 + B.2 + Y.2	.2	83	.14
		BLUE			
J.H.	B 10 + clear	B 10 + 1 + 1 + 1	3.0	75	3.00
R.H.S.	B 10 + clear	B 10 + .5	.5	81	.39
C.J.K.	B 10 + clear	B 10 + .5	.5	86	.30
P.J.S.	B 10 + clear	B 10 + .1	.1	76	.10
		YELLOW			
J.H.	Y 10 + 1 + 1	Y 10 + 5 + 1	4.0	79	3.30
R.H.S.	Y 10 + clear	Y 10 + .3 + .2	.5	81	.39
C.J.K.	Y 10 + clear	Y 10 + .1 + .1	.2	64	.37
P.J.S.	Y 10 + clear	Y 10 + .3 + .2	.5	87	.30

In the case of J. H., St. 1 is slightly different from that of the control observers for the colors yellow and green. An attempt was made to use for him the yellow which could not be distinguished from red 10, which was yellow 10 + 1 + 1. Green

was somewhat difficult to secure, since no green slips were available, and blue and yellow had to be combined for it. However, a very good green resulted from this combination; and the green which matched blue 10 was compounded from Y 10 + B 10 + Y 5 + B 5 + Y 1 + B 1. These six slips were too thick when bound together to fit the receiving stalls of the tintometer, so the last two were omitted, and the four designated in the table retained.

These tests with the tintometer indicate that J. H. has a decidedly higher discrimination threshold than the normal observers. It is however questionable whether he is really judging depth of color at all; the probability is that some other factor, perhaps luminosity, served as a criterion. To what extent this is also true of the other observers is uncertain; they are at any rate, more sensitive to color than J. H. and, under identical conditions, make better discrimination records. The results, even as they stand, are too favorable for J. H., (see his hue discrimination records, p. 20 f).

Color fields

A recent model of the Meyerowitz self-recording perimeter was secured and fitted with new stimulus patches 9 cm. square, cut from the four Hering colored papers and white. The self-recording device was discarded and the records read in degrees directly from the arms. The color carriers were moved at such a rate that approximately ten seconds were required for one determination. The unused eye was comfortably bandaged. A color, not known to the observer, was moved inward from the periphery and the position noted where it first was named correctly. Two determinations for each of the five stimulus patches were made in each of the four cardinal meridians for each eye, with J. H. and the three regular control observers. J. H., of course, reported only two colors and white. His records again have relative value only, and achieve their significance when compared with those made upon observers with normal[2] color

[2] C.J.K. has complete central scotoma in his right eye and perhaps slight color weakness.

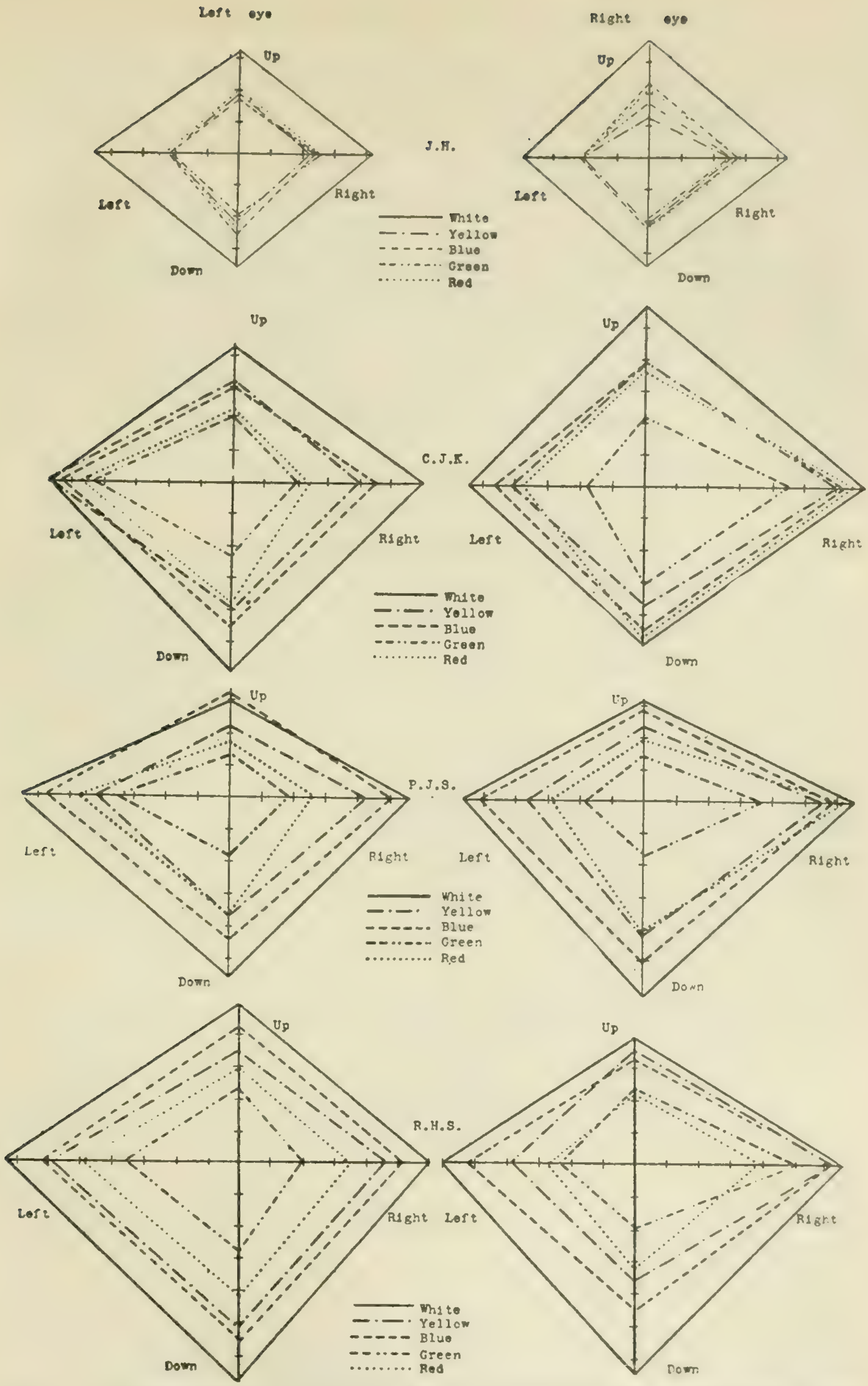


FIG. 1. Visual fields of J.H. and three control observers. Drawn to same scale. Units on axes = 10°.

vision under identical conditions. The results are presented here only in graphic form,[3] Fig. 1, but the curves are drawn to the same scale and may be directly compared. The curves show very close general agreement among the three control observers; all are rather typical. J. H., on the contrary, shows marked contraction of both his color fields and his field for white. The fields are so small that the familiar limiting in certain meridians by the interference of the nose, brow and cheek does not occur, and even the boundaries for white are almost symmetrical. The color fields are very small and practically coincident.

Relative stimulating values of the different wave lengths[4]
(*Spectral luminosity*)

The relative stimulating values of the different wave lengths were sought for by means of a Hilger spectroscope of the fixed arm, constant deviation type, combined with a Whitman disk so as to form a flicker photometer or a direct comparison heterochromatic photometer as desired. The essential feature of the apparatus was recently described by Ives (3) and others. The reflecting surface of the Whitman disk was covered with magnesium oxide, so that the distribution of energy among the wave-lengths of the visible spectrum approximated that of the source of illumination on the disk. In the tests made under light adaptation, this source was a tungsten lamp having a "color temperature"[5] of 2327° Absolute. In the tests under dark adaptation, the source was a similar but smaller lamp giving

[3] In view of the great length of many of the tables of values in this report, it has been deemed unnecessary to present the records in both graphic and tabular forms. The numerical values are systematically tabulated and on file and may be obtained from the writer by any one who may perchance wish them.

[4] The descriptions of the apparatus used in the succeeding experiments are substantially those very kindly furnished by Dr. H. M. Johnson of the Nela Research Laboratory, at which place most of these experiments were conducted.

[5] The color temperature of an incandescent filament is the temperature of a black body giving the same distribution of energy among the different wave-lengths as does the filament under the given conditions. This datum being specified, the spectral character of the radiation emitted by the source may be readily ascertained from well known formulae.

a color temperature of 2355° Absolute. The brightness of the disk was determined by the position of the lamp and was kept constant at 32.8 candles per square meter in the work under light adaptation, and at 0.086 candles per square meter in the work under dark adaptation.

The observed field, as limited by a suitable diaphragm, was circular in form, and subtended a solid visual angle of 2.4°. It was observed through an opening 1 mm. in diameter, placed in the plane of the image of the collimator-slit formed in the minimally deviated wave-length. The observer's eye was about 1 cm. from the aperture, which is as close as his eye-lashes would permit. In the tests by the method of direct comparison, the Whitman disk was stationary, and turned so that one of its radial edges bisected the field vertically. The left half of the field as observed was a portion of the disk, illuminated by tungsten light. The right half was a portion of the prism illuminated in monochromatic light, the wave-length of which was determined by the angular position of the prism. The limiting diaphragm was in all cases completely filled with light. In the tests by the flicker method, the entire circular field was illuminated successively by colorless and monochromatic light. The surroundings of the field were dark except for light scattered in the optical system and in the refractive media of the eye.

The source of homogeneous light was a tungsten lamp. In the tests by both the flicker and direct comparison methods, the spectrometer lamp occupied a fixed position with reference to the collimator-slit. In the direct comparison method, the observer sought an equation of the brightness of the colored portion of the field with that of the disk, by himself adjusting the width of the collimator-slit, controlled by a micrometer screw, with scale reading directly to the 0.01 mm. The distance between the lamp and the slit was increased to secure lower illumination for the tests under dark adaptation and a ground-glass surface was placed between the filament and the collimator-slit, to give uniformity of brightness over the colored portion of the field and further to reduce illumination.

In the experiments according to the flicker method, the speed of the disk was controlled, within limits, by means of resistance in the motor circuit.

The entire system was covered with black felt, and the eyes of the observer were protected from stray light from the lamps. Day-light was excluded from the room. In the tests under light adaptation, the general illumination of the room was from four 150 watt "Mazda C-2" lamps in an overhead fixture. These lamps have tungsten filaments in a gas-filled bulb, the selective absorption of which is such as to transmit light approximating that of afternoon sunlight in spectral character. In the work under dark adaptation, the general illumination was eliminated and fair dark-room conditions were obtained. The observer remained in the darkened room about twenty minutes before beginning the experiments and kept his eyes closed except during the short interval when making the comparisons.

The flicker settings under dark adaptation became very uncertain except for the middle regions of the spectrum, perhaps because the speed of the motor was not variable within sufficiently wide limits.

In making the settings, a definite procedure was followed. The observer himself turned the micrometer which varied the slit-width which controlled the amount of light admitted to the prism of the spectrometer. The scale readings were all made and recorded by the experimenter. Beginning at 720 $\mu\mu$, five settings were made at regular intervals of 20 $\mu\mu$ until 420 $\mu\mu$ was reached. Then the spectrum was similarly surveyed in the reverse order. Of the ten readings thus secured for each wave-length selected, five were made with the luminosity varying from less than that of the colorless comparison field, to equality, and the other five varied from greater luminosity to apparent equality. These two directions of approach alternated regularly in both the direct comparison and the flicker methods, which themselves alternated, five direct comparison readings being made, then five flicker readings. Ten readings, taken in the double fatigue order, were thus secured for each 20 $\mu\mu$ interval

of the spectrum within the limits indicated, for both the direct comparison and flicker methods. With otherwise identical procedure, readings were obtained for both photopic and scotopic vision for each eye of J. H. separately, and for the right eye of three control observers, Dr. L. T. Troland, Mr. Everett Schmidt (a graduate of one of the Cleveland technical high schools), and from the writer.

(A). *Photopic luminosity.* (*Daylight adaptation*)

The records are given in graphic form in Figs. 2, 3, 4, 5 and 6, and represent the reciprocals of the slit-widths in mm. plotted against the ordinates, and the wave-lengths as abscissae. The result is a luminosity curve for a prismatic spectrum of tungsten at a temperature of 2385° Absolute.

J. H., for whom such work was new, had some difficulty at first with the direct comparison method and the results for this method never became so constant as for the flicker method. The mean variations (not presented) show this very clearly. His curves for the flicker method bear a close general resemblance to those of the control observers and also those which have been published by Ives and other investigators. In his direct comparison curves, there is a noticeable break at 560 $\mu\mu$; here the slit was opened much wider than for the wave-lengths immediately preceding and following, 540 $\mu\mu$ and 580 $\mu\mu$ respectively. This is probably due to the fact that there is an achromatic or neutral band in this region of his spectrum, near 560 $\mu\mu$, and he apparently opens the slit wider in the effort to admit color. For the flicker method this does not occur, as the whole field then becomes homogeneous and he is not directly conscious of equating two separate fields.

J. H.'s descriptive comments about the various colored lights are interesting and are given here in his own wording as they were recorded during the progress of the experiment.

720 $\mu\mu$: "Deep red."
700 $\mu\mu$: "Red; good deep bright red."
680 $\mu\mu$: "Nice bright red."
660 $\mu\mu$: "Good bright red."

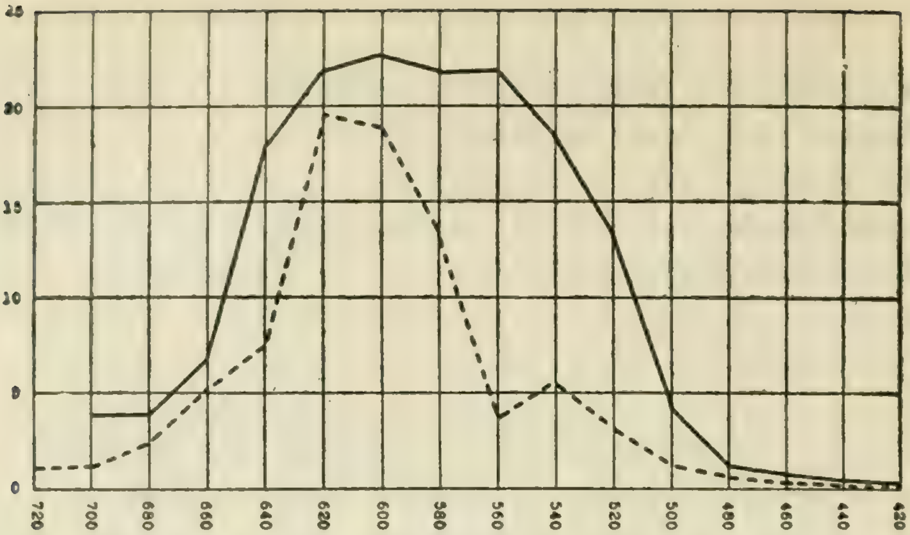


FIG. 2. Photopic luminosity curve for J. H. Right eye. - - - - - = direct comparison, ——— = flicker method.

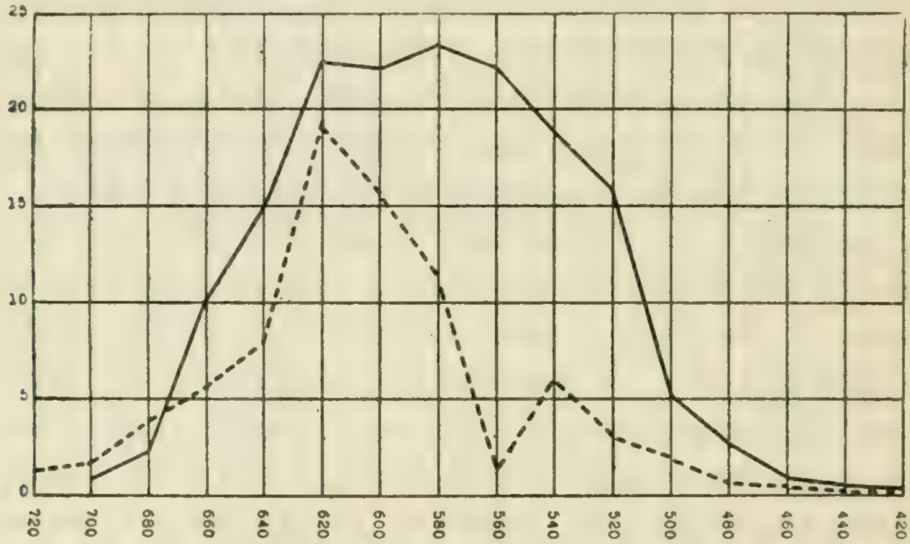


FIG. 3. Photopic luminosity curve for J. H. Left eye. - - - - - = direct comparison, ——— = flicker method.

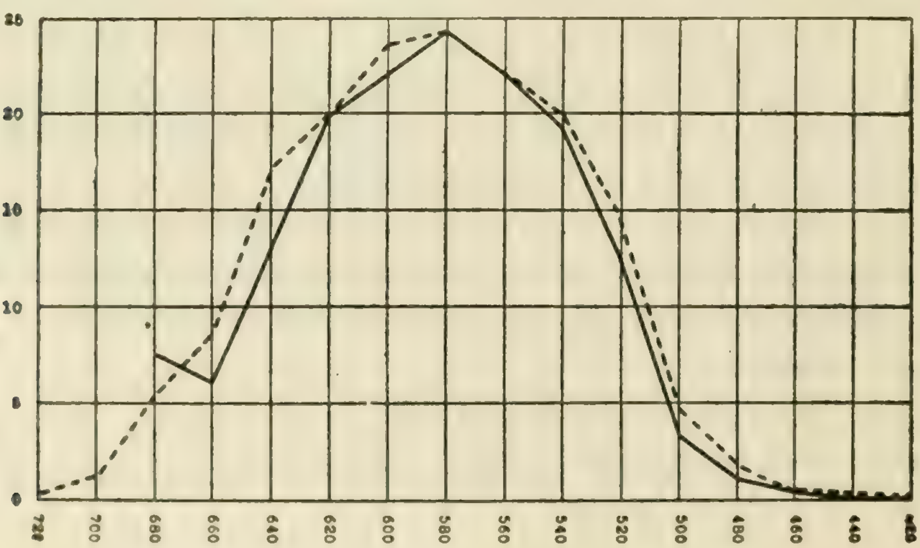
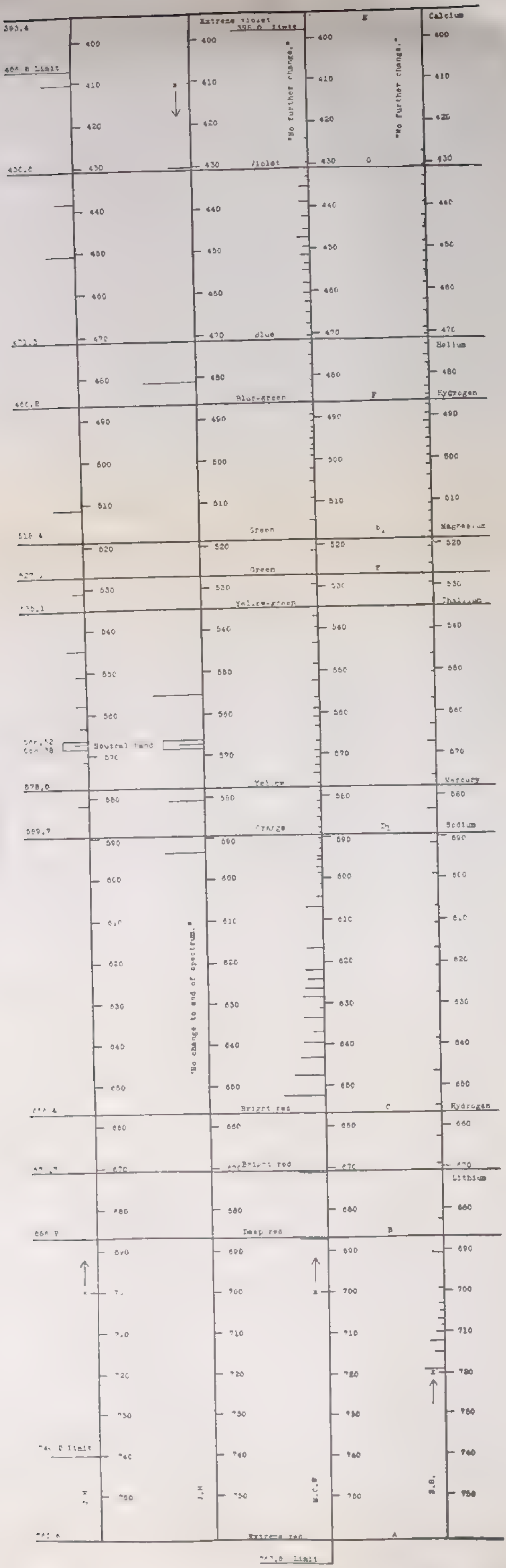


FIG. 4. Photopic luminosity curve for L. T. T. Right eye. - - - - - = direct comparison, ——— = flicker method.



Extreme violet
396.0 Limit
Calcium
"No further change."
"No further change."
Violet
Blue
Helium
Blue-green
Hydrogen
Green
Magnesium
Green
Yellow-green
Neutral band
Yellow
Mercury
Orange
Sodium
"No change to end of spectrum."
Bright red
Hydrogen
Bright red
Lithium
Deep red
Extreme red
J.H
J.H
W.C.W
S.S.

640 $\mu\mu$: "Red; lighter but not particularly light."
620 $\mu\mu$: "Pale red, even when reduced to just visible intensity."
600 $\mu\mu$: "Light red."
580 $\mu\mu$: "Faint reddish; lots of light in it."
560 $\mu\mu$: "Pale red; very pale indeed." On another occasion, "Light green."
540 $\mu\mu$: "Light green; not the same as before; quite a bit of green in it."
520 $\mu\mu$: "Green; an unsaturated green."
500 $\mu\mu$: "Deep green."
480 $\mu\mu$: "Good saturated green."
460 $\mu\mu$: "More saturated green than ever. A good deal more so than the last one."
440 $\mu\mu$: "Dark saturated green; a little darker than the last one."
420 $\mu\mu$: "Much the same as the other one; a dark saturated green; does not seem to be different."

(B). *Scotopic luminosity.* (*Dark adaptation*)

The results are presented graphically in Figs. 7, 8, 9 and 10. The curves represent the luminosity of a prismatic spectrum at a color temperature of 2100° Absolute. The notation is the same as that for the photopic curves, but they are drawn on a slightly different scale; had the same scale been used the curves would have been too flat. All the observers, except perhaps L. T. T., who alone had had extensive practice in photometry, had much difficulty with both the direct comparison and the flicker determinations. It is doubtful whether adaptation was complete enough to show the Purkinje effect.[6] With both the direct comparison method and the flicker method, the spectrum was not so luminous for J. H. as for the controls; he kept the slit opened much more widely. His achromatic band, near 560 $\mu\mu$, was for him the most luminous part of the spectrum, the place where he most nearly closed the slit. This is just the reverse of the situation in the photopic adaptation curve with

[6] L.T.T. exhibits to a slight degree with the flicker method the "reversed Purkinje effect" described by Ives (4). The same effect is suggested in the case of J.H., and to a lesser degree with E.S.

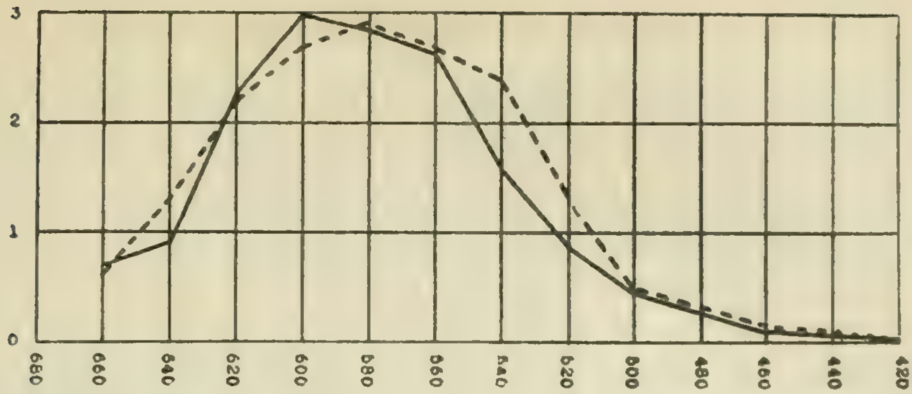


FIG. 8. Scotopic luminosity curve for L. T. T. Right eye. - - - - - = direct comparison, ——— = flicker method.

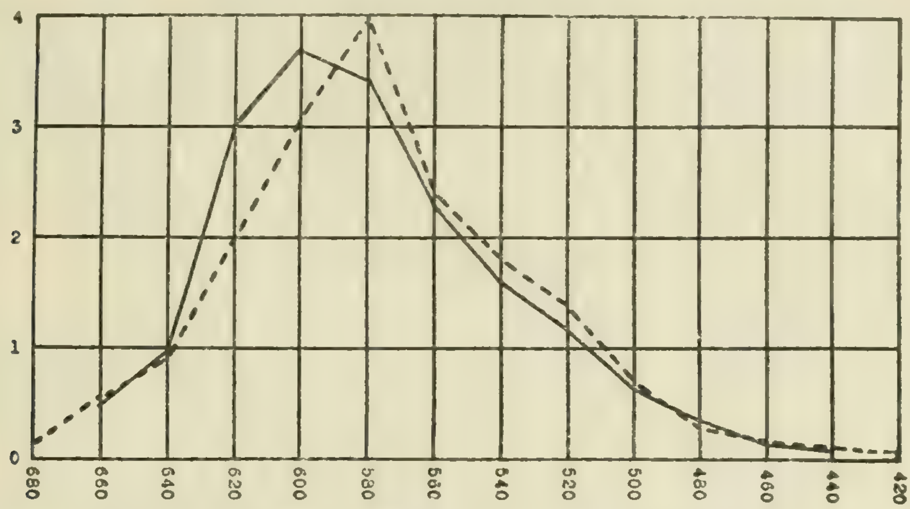


FIG. 9. Scotopic luminosity curve for E. S. Right eye. - - - - - = direct comparison, ——— = flicker method.

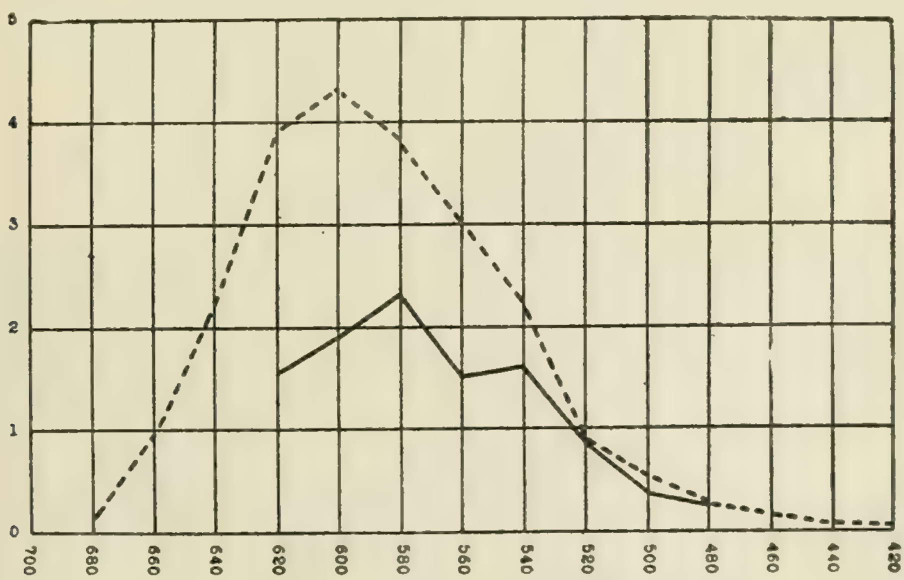


FIG. 10. Scotopic luminosity curve for M. C. W. Right eye. - - - - - = direct comparison, ——— = flicker method.

the method of direct comparison. It is probable that he saw very little if any color in any part of the spectrum with this low illumination, so the suggested explanation for the wide slit-width for 560 $\mu\mu$ in light adaptation does not apply here.

Spectral limits

No very extensive measurements were made since it was necessary to know only in a general way whether J. H.'s spectrum was shortened, especially at the long wave end. It may be said that, under the most favorable conditions, he reported color approximately between 750 $\mu\mu$ and 400 $\mu\mu$. In Table III some records are presented; those with the carbon arc and the Nernst filament were made by Mr. Helmick of the department of physics at Iowa.

TABLE III—*Spectral limits*

	Red-end		Violet end		
	Ave.	m. v.	Ave.	m. v.	Source
Obs.	$\mu\mu$	$\mu\mu$	$\mu\mu$	$\mu\mu$	
J.H.	740.0	3.7	406.8	.9	Tungsten lamp, Hilger spectrometer, ruby Schott Jena glass over slit for red.
M.C.W.	767.5	4.3			Same.
J.H.	661.3	6.0	461.9	7.7	Carbon arc and ordinary prism.
R.H.S.	693.0	5.0	428.0	4.0	Same.
C.J.K.	701.0	8.0	411.0	4.0	Nernst filament.
P.J.S.	682.0	8.0	430.0	4.0	Carbon arc.
M.C.W.	712.0		404.0		Nernst filament.
M.C.W.	704.0		384.0		Carbon arc.
Norm	723.0		397.0		(As stated by Parsons.)
Norm	760.0		396.0		(As stated by Watson.)
Norm	800		400		(As stated by Köllner.)

Threshold differences for wave-length (*Hue discrimination*)

These tests were made by means of two Hilger wave-length spectrometers of the fixed arm, constant deviation type, set up as monochromatic illuminators with a Koenig-Marten polarization photometer. The telescope and collimator of one spectrometer were interchanged and the two spectrometers were so mounted that the axes of the two collimators intersected at an angle of 90° in the plane of the filament of the lamp which served as a common source, and the axes of the two telescopes similarly intersected a short distance in front of the slit of the

Koenig-Marten photometer. The source was a tungsten filament in a gas-filled bulb, operated at 0.5 watts per candle and having a color temperature of approximately 3000° Absolute. An image of the filament was formed on the collimator slit of each instrument by means of a simple lens suitably placed to fill the prism, and the width of the collimator slit was kept constant at 0.3 mm. The ocular slits were removed from the two spectrometers, and the photometer so placed that its slit served in their stead. The image of one collimator slit in the minimally deviated wave-length was formed directly on the upper half of the photometer slit; and, by reflection at a right angle prism, a similar image of the other collimator slit was formed on the lower half of the photometer slit. A piece of colorless ground-glass was placed over this slit to give a field of uniform brightness. After passing through the slit of the photometer, the light passed through a collimating lens, a Wollaston polarizing prism, a bi-prism, an objective lens, a Nicol analyzer, and an eye-piece. The field, as observed through a circular opening 1 mm. in diameter and 1 cm. in front of the eye, was circular, subtending a solid visual angle of about 3°, and bisected horizontally. The upper half was filled with homogeneous light from one illuminator, and the lower half by homogeneous light from the other illuminator. The wave-lengths selected at the photometer slit were determined by proper rotation of the prisms of the spectrometers. The two halves of the photometer field were kept equal in brightness during the settings by proper rotation of the Nicol. The experimenter set the prism of one spectrometer so as to illuminate the standard, or upper, half of the field with light of a known wave-length, and the observer adjusted the drum of the other prism table with one hand, and the Nicol with the other hand, until the variable, or lower, half-field matched the standard in both hue and brightness. Except in the extreme blue and violet, in which there was a slight disproportionality in the amount of diffuse white light superposed on the compared spectra, there was no difficulty in setting so that the entire field appeared uniform. The field was of a high

brightness between the extremes of the spectrum. It was not feasible, owing to the stringency of time, to keep the standard half-field constant in brightness for all wave-lengths. The results given by the several observers were obtained under identical visual conditions and the results are strictly intercomparable. The observing room was in semi-darkness.

The procedure was as follows: Beginning at the extreme red, 700 $\mu\mu$, the two spectrometers were set so as to illuminate both halves of the field in the same wave-lengths. The observer was required to keep the two halves equal in brightness, and to turn the prism of the instrument illuminating the variable half-field until it appeared just noticeably different in hue from the standard. The reading was recorded by the experimenter; the difference was increased, and the observer required to turn the prism until the difference in hue just disappeared. Five paired settings were taken in this way, and the wave-length illuminating the standard half-field was changed to the mean of the ten readings to serve for the next standard. In this manner, the whole spectrum was worked through in short sittings, with frequent rest periods, for three observers, J. H., and two controls, E. S. (who also served in the luminosity experiments) and the writer. In the case of J. H., a second set of readings was taken, working from the violet, 410 $\mu\mu$, to the red.

The results are presented graphically in Fig. 11, the long, folded chart. Along the base lines the 10 $\mu\mu$ divisions are indicated. Normal spectral limits are marked by the heavy lines (Fraunhofer, A and K) at either end. Since the Fraunhofer lines afford fixed points for reference, they are drawn in, with their wave-lengths and characteristic hues indicated. The small crosses give the initial starting points for each observer, and the arrows the direction followed. The short lines, perpendicular to and above the base, represent, in position, the discrimination intervals, each one marking the hue that is just perceptibly different from the preceding one under the conditions imposed. The height of these lines above the base shows the mean variation characterizing the average of the 'difference' and 'equality'

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OR MA

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settings; a height of 1.5 mm. being approximately equal to 1 $\mu\mu$ of variation as the chart appears after reduction. The initials at the left indicate the three observers.

Inspection of the curve reveals at once the vast difference between J. H. and the two control observers; for him hue discrimination practically does not exist. In his first record there are twelve recorded changes and in the second there are only six.[7] M. C. W. has under exactly similar conditions eighty-eight hue changes, and E. S. seventy-two. For J. H. there is no change from 700 $\mu\mu$ to 583 $\mu\mu$; "red" therefore is constant and unchanging in hue until well into the yellow. The few changes that then occur mass themselves for the most part within the next forty or fifty $\mu\mu$. At about 566 $\mu\mu$ a very narrow colorless or achromatic band appears, the limits for which have been very carefully determined. Passing on through the green which he calls "green" and into the blue which he also calls "green", changes are infrequent, until the violet, remarked as "green with some red in it," begins to appear, and continuing, color is finally lost, and "just light" remains. Could his spectrum be bent around into a circle, it would then show two achromatic bands; the one near 566 $\mu\mu$ already mentioned, and one between the violet and the red. The second record for J. H. is not qualitatively different from the first.

Hue changes for M. C. W. mass in two regions, the orange-yellow and blue-green. E. S. shows evident practice effects (supported by his verbal comments during the experiment) and his differences grow smaller rather gradually, but show a tendency to mass in the blue-green region also.

Neither M. C. W. nor E. S. attain the number of hue differences calculated by Koenig from Uhthoff's results, (5), namely 165. With practically the same apparatus that was used in this experiment, Dr. Johnson informs the writer that Nutting, in some unpublished experiments, obtained about 200 hue differences. Both J. H. and the controls however were quite without practice in this particular experiment.

[7] This second record was made very hastily, just before J.H. had hurriedly to leave for the train, and it may have been influenced by that fact.

In connection with this experiment, J. H. made an observation which is interesting. It was noticed that he looked longer than usual at the color field with a certain setting of the spectrometers, (upper half of field 551.7 $\mu\mu$, lower half 554.0 $\mu\mu$ and somewhat darker). Asked what the difficulty was he replied: "It is beautiful and reminds me of an ocean sunset view at the equator. There is the sky all glowing red and the green ocean below. I have seen it that way many times." To the experimenter, who took a look at this seascape, there was a greenish yellow "sky" and a somewhat darker dirty yellow "ocean", and no straining of the imagination could make out of this a brilliant marine sunset comparable to those she had actually seen.

Matches between mixtures of two homogeneous lights and a single homogeneous light

The dichromat is usually able to match any spectral color by mixtures in suitable proportions of light from the extremes of the spectrum. A Hilger wave-length spectrometer of the fixed arm, constant deviation type, was set up as an illuminator so that one of the opal glass receiving surfaces of a photometer was illuminated by the wave-length suffering minimum deviation by the prism of the spectrometer and selected at its ocular slit. A high intensity monochromatic illuminator was used to fill the other half of the photometric field. This instrument was equipped with a train of two 60° dense flint glass prisms which were set for minimum deviation of $\lambda = 505$ $\mu\mu$. The spectrum was formed on an acid-blackened brass plate containing three slit openings, the width and positions of which were independently variable. One of the slits was closed and the others placed so as to transmit a band at 672.6 $\mu\mu$ and another at 461.4 $\mu\mu$.[8] A projection lens was used to form an image in these two bands of the face of the first prism and the second receiving surface of the photometer.

The source used for both illuminators, were tungsten filaments in argon-filled bulbs, operated under steady current con-

[8] The nearest fixed points to these hues are the red lithium at 670$\mu\mu$, and the blue helium at 471.3$\mu\mu$.

ditions and giving a color temperature of approximately 3000° Absolute.

The field as observed was circular and subtended a solid visual angle of about 4°. Its mean brightness was high enough for comfortable reading, but was not measured. It was observed in dark surroundings through a circular opening 1 mm. in diameter and 1 cm. from the eye. The pattern was two half-circles each with a trapezoid. In all cases a match was obtained in which the pattern disappeared against the background.

The data secured in the mixing of spectral lights are not so extensive as had been planned, nor is the method of procedure so precise, for an unavoidable three-day absence from Cleveland made extensive work with the color mixing apparatus impossible. The original plan had been to proceed by taking definite proportions of the red and blue lights and find the monochromatic equivalents, working from 100% red + 0% blue to 0% red + 100% blue. What was actually done, however, was to take 10 $\mu\mu$ stops of monochromatic light along the spectrum, and determine empirically the proportions of mixed red and blue beams which would match for J. H. each monochromatic beam. An exact match was insisted upon, however; the pattern in the field (mixed light) was made to match the background (monochromatic) so that the former disappeared. The slit-width of the Hilger spectrometer illuminating the background was kept constant, so the luminosity curve is incorporated in the records. This means that, for the intense colors, wider, therefore more intense, red and blue beams had to be admitted.

An attempt was made to get a rough estimate of the relative stimulating values of the red and blue lights in terms of the same constant. The Hilger spectrometer was set by J. H. at the place where the background illuminated by it showed the least color, namely at 564.5 $\mu\mu$, slit-width 0.5 mm. A direct comparison luminosity match for this was found with red, 667.8 $\mu\mu$, to require a slit-width of 0.5 mm. The same for the blue, 460.3 $\mu\mu$, was 4.4 mm. Translating slit-width into wave-lengths, $\mu\mu$, this means that a beam of red light extending from 673.8 $\mu\mu$

to 667.8 $\mu\mu$ is approximately equal in stimulating value to a blue beam extending from 476.4 $\mu\mu$ to 460.3 $\mu\mu$, since both, with unchanging light source, are apparently equal to the same unchanging monochromatic beam. Since hue discrimination practically does not exist for J. H., it may be assumed that the narrow beams here admitted were of one hue throughout.

TABLE IV

	Red			+ Blue		
Stand.	R. Jaw	L. Jaw	Range	R. Jaw	L. Jaw	Range
$\mu\mu$	$\mu\mu$	$\mu\mu$	$\mu\mu$	$\mu\mu$	$\mu\mu$	$\mu\mu$
460	None			461.4	463.0	1.6
470	None			461.4	463.2	1.8
480	None			461.4	463.3	1.9
490	None			461.4	467.0	5.6
500	None			461.4	469.5	8.1
510	None			461.4	473.0	11.6
520	672.6	672.8	.2	461.4	473.5	12.1
530	672.6	672.9	.3	461.4	473.5	12.1
540	672.6	674.2	1.6	461.4	473.2	11.8
550	672.6	674.5	1.9	461.4	472.5	11.1
560	672.6	675.0	2.4	461.4	472.3	10.9
570	672.6	687.2	14.6	461.4	472.2	10.8
580	672.6	714.0?	41.4	None		
590	672.6	731.0?	58.4	None		
600	672.6	730.0?	57.4	None		
610	672.6	724.0?	51.4	None		
620	672.6	710.0?	37.4	None		
630	672.6	703.0	30.4	None		
640	672.6	702.0	29.4	None		
650	672.6	698.0	25.4	None		
660	672.6	685.0	12.4	None		

In Table IV the best one of three complete sets of records is given. The table is practically self explanatory. 'Range' refers to the width of the monochromatic beams in $\mu\mu$ admitted between the jaws of the slits allowing the selected beams to pass. The wave-lengths at each jaw of each slit are given merely for the sake of completeness. The interrogation marks indicate readings carried beyond the point of accurate calibration.

It is very desirable to plot the results of this experiment in a curve, but since the stimulating values of red and blue are so unlike, that cannot be done directly, nor do the records give for J. H. all the values necessary for making the calculations, and further it is probable that, although every effort was made to keep the physical conditions in the apparatus constant, many variables entered. An attempt was made to plot such a curve

based upon certain calculated results; it is not presented on account of probable unreliability.

Von Kries (6) has emphasized the fact that a mixture of red (670.8 $\mu\mu$) and yellow-green (550 $\mu\mu$) will match for protanopes, deuteranopes and normal 'trichromats' a homogeneous yellow, although the three classes do not agree among themselves as to the proportions of red and green in the mixtures. Von Kries, however, does not keep the hue of his yellow constant, since in the table he gives, the wave-length varied from 569 $\mu\mu$ to 639 $\mu\mu$. Although this test is not an especially critical one, the attempt was made with J. H. to match a homogeneous yellow (587.6 $\mu\mu$ yellow helium) with a mixture of red (667.8 $\mu\mu$, third red helium) and green (501.6 $\mu\mu$, second green helium). Intensity of the yellow was varied by changing the slit-width. The mixed light illuminated the pattern of the field, the monochromatic light the background. A direct comparison luminosity equivalent of yellow 587.6 $\mu\mu$, slit-width 0.5, was found in the green, 501.6 $\mu\mu$, slit-width 1.4 mm.; and in the red, 667.8 $\mu\mu$, slit-width, 3.07 mm.

No attempt has been made to reduce the results to equal stimulating value. In Table V, the amount of each of the two colors in the mixture as empirically determined is stated in terms of the width of the beams in $\mu\mu$'s passed through the slits. The right jaws of the slits were fixed respectively at 667.8 $\mu\mu$ and 501.6 $\mu\mu$.

TABLE V

Yellow 587.6$\mu\mu$ Slt-wdth. mm.	Green 501.6$\mu\mu$ Range $\mu\mu$	+ Red 667.8$\mu\mu$ Range $\mu\mu$
.8	1.5	71.2
.7	1.9	32.2
.6	1.6	17.2
.5	1.4	35.2
.4	1.6	37.2
.3	1.8	20.2
.2	1.5	10.2
.1	.7	4.2

The table shows that J. H. accepts relatively only a small amount of green in the mixture, and that this amount remains

a fairly constant quantity as the luminosity of the yellow is successively reduced. The luminosity adjustment is made in the red. J. H. would rather have had no green in the mixture; he detected its presence and requested its withdrawal when the amount was increased. The yellow at 587.6 $\mu\mu$ is for him distinctly within the red, very near to the point which he selects as the "prettiest and brightest red in the spectrum", namely at about 600 $\mu\mu$, and might easily have been matched by different amounts of red alone. To the normal eye 587.6 $\mu\mu$ is practically an orange yellow.

The achromatic band

It was early discovered there was an apparently colorless band at about the place in the spectrum where the yellow begins faintly to appear for the normal eye. Its position was first rather definitely known when J. H. set an Hilger spectrometer to 564.5 $\mu\mu$ as showing the "least color". Later a slit with bilaterally movable jaws was put in the path of the spectrum at the place of greatest achromaticity, and the slit opened until the edges just marked the limits of this region. The average of five trials, fixed the limits at 568.2 $\mu\mu$ and 564.3 $\mu\mu$, indicating a band about 4 $\mu\mu$ wide, with the center at 566.2 $\mu\mu$.

Later on, a still more exact determination was made by adapting the hue discrimination apparatus. One-half of the field of the polarization photometer was illuminated with light from an Hilger instrument, the other half by a small tungsten automobile headlamp operated at 6 volts. Brightness equality was easily secured by rotating the Nicol prism. The Hilger drum which rotated its prism table was set first by the observer at a point where no color appeared when compared with the approximately white standard half-field, then the drum was turned toward the red until red just began to appear and the reading was taken by the experimenter. The drum was then rotated in the reverse direction until color ("green") began to come in from the opposite side. Five settings for each border limit were secured, which, when averaged, gave 568.38 $\mu\mu$ and 566.52 $\mu\mu$ as the limits. The range is 1.86 $\mu\mu$, and the center is located

at 567.45 $\mu\mu$. This is in very close agreement with the other determinations of the colorless band, and not far different from J. H.'s unaided setting of the Hilger at 564.5 $\mu\mu$ as the place of least color. From the band to the long end, the spectrum was called "red", and in the other direction it was "green" throughout, with no extensive shortening at either end. As has been noted previously however, were the spectrum bent in a circle, there would be another achromatic region between the violet and red.

Miscellaneous

Time did not permit definite experimental study of after-images and simultaneous contrast. Incidental observations were made from time to time and indicate that both these effects occur. In the case of contrast, when one half of the field was white, the other yellow, ("red"), some "green" was detected in the white.

White was repeatedly called white, and aside from obvious induction effects was never confused with a color.

The best, purest and brightest "red" he located at about 600 $\mu\mu$; the best "green" at about 450 $\mu\mu$, (M. C. W. best blue at 440 $\mu\mu$). His "red" is thus really in the orange-yellow and his "green" in the blue.

What effect macular pigmentation has in this case is not known. His whole retina, including his macula, is rather densely pigmented, but it is not exceptional in this respect. Parsons says that the effect of macular pigmentation is to displace the achromatic band toward the long end, in this case, into the yellow. Color is probably not intense for J. H. in this region of the spectrum and macular absorption may be rather effective.

At various times, incidental tests were made to determine whether there was a marked difference between the two eyes. No noteworthy difference was found.

This study was begun, and for the most part pursued, without any conviction as to the type of color defect represented by J. H. It was sufficient to know that the case was unusual to stimulate

the investigation. The study is not complete, but it is not certain that J. H. will be available for further work. Color theories are at the present time undergoing modification and new data are needed. The writer, then, presents these as the facts in hand, and submits the description without at this time attempting a diagnosis.

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BINAURAL BEATS

BY

G. W. Stewart

CONTENTS

Introduction

The subject of binaural beats has been one of much interest to the psychologist,[1] not only because the phenomena observed are difficult to explain with entire satisfaction, but also because the explanations involve what are evidently very important questions. The writer became interested in the physical aspects of binaural hearing through his theoretical and experimental studies[2] of the sound intensity and phase relations in the neighborhood of a rigid sphere. His experimental work upon the subject of binaural beats will be presented in appropriate detail in the Physical Review for 1917. The present article will give a very brief account of the results, both experimental and theoretical, with the expectation that those especially interested in this field will refer freely to the detailed reports in the Physical Review. Brevity will necessitate the preparation of this article for the most part as an abstract, and hence omissions of refer-

[1] An excellent review of the experiments with bilateral excitation is given by Rostosky, Beiträge zur Psychologie and Philosophie, I, 1905, pp. 173-273.

[2] G. W. Stewart, Physical Review, (1911), and N. S., (1913), (1914) and (1916).

ences to other investigations and of complete discussions of disputed points, will occur frequently.

Earlier Results

The chief phenomena observed when two tones differing in frequency are led one to each ear are as follows:[3]

(a) Beats occur whether the tones are presented to one ear or one to each ear. This was discovered by Dove,[4] observed subsequently by various experimenters and discovered independently by S. P. Thompson.[5] This phenomenon is observed by all, even by the partially deaf.

(b) With bilateral presentation of the tones, the minimum intensity observed in each beat does not (even with carefully adjusted equal intensities of the two sources) approximate zero. This is mentioned by S. P. Thompson[6] but doubtless was observed very much earlier by others.

(c) When the tones at the ears are alike in intensity and phase, there is a localization in the median plane, the position in this plane varying with the observer and presumably with the apparatus arrangement. Purkyné[7] was the first to record this localization, which has been shown through numerous experiments of other observers, to be a usual accompaniment of binaural perception.

(d) With opposition in phase and equality in intensity, the sound is localized according to S. P. Thompson[8] at the back of the head. To several observers, with apparatus of a form different than that of Thompson's the localization is in the median plane. Doubtless the position in the median plane is influenced by the method of observation.

(e) The sound is localized on the side of the fork leading

[3] Unless otherwise stated, the difference of freqnency is assumed to be small.

[4] Loc. cit.

[5] S. P. Thompson, Phil. Mag. 5, IV, 1877, p. 274.

[6] Loc. cit.

[7] Purkyné Prager Vierteljahrsschrift, Bd. 67, p. 91, 1860.

[8] S. P. Thompson, Phil. Mag. 5, IV, 1877, p. 274, and Phil, Mag. 5, VI, 1878 p. 383.

in phase when the phase difference is approximately 90°. Thus with phase difference the sound wanders from the median plane at 0° to one side at 90°, to the median plane at 180°, to the other side at 270° and again to the median plane at 360°. Perhaps S. P. Thompson[9] first observed the wandering from ear to ear but the periodic displacement of the localization was first most clearly described by Paul Rostosky.[10] Later workers, who were not aware of Rostosky's results, observed this lateral displacement. Lord Rayleigh,[11] experimenting with forks, recognized the lateral effect and its relation to phase difference, and later[12] confirmed these observations with experiments conducted by the use of telephones. L. T. More and H. S. Fry[13] made preliminary experiments which were followed by those of L. T. More.[14] His results showed unmistakably the lateral displacement. More used an apparatus similar to that earlier adopted by C. S. Myers and H. A. Wilson.[15] A single source, branched pipes to the ears and an arrangement whereby the lengths of the pipes could be altered in opposite senses, were used. The experiments of Myers and Wilson also gave full confirmation of the lateral effects. Bowlker's[16] experiments do not seem to furnish reliable evidence. Unfortunately, the workers just mentioned seem not to have been aware of the splendid experiments of Rostosky published in 1902.[17]

(f) There is a limit to the frequency of the tone with which the effect just described (e), can be obtained. L. T. More[18] states that a fork of 512 d. v. was near to his limit of accuracy in the judgment by phase differences, that with 1024 d. v. his judgment became untrustworthy and that with 3000 d. v. he

[9] S. P. Thompson, Phil. Mag. 5, XII, 1881, p. 351.
[10] P. Rostosky, Philosophische Studien, 19, 1902, p. 557.
[11] Lord Rayleigh, Phil. Mag. XIII, 1907, p. 214.
[12] Lord Rayleigh, Phil. Mag. XIII, 1907, pp. 316-319.
[13] L. T. More and H. S. Fry, Phil. Mag. XIII, 1907, p. 452.
[14] L. T. More, Phil. Mag. XVIII, 1909, p. 308.
[15] C. S. Myers and H. A. Wilson, Proc. Roy. Soc. LXXX, 1908, p. 260.
[16] T. J. Bowlker, Phil. Mag. (6) XV, 1908, p. 318.
[17] Loc. cit.
[18] Loc. cit.

had no sense whatever of direction. Lord Rayleigh[19] gives the limiting frequency of his right and left sensations at 768 d. v. C. S. Myers and H. A. Wilson[20] state in general terms that with very high frequencies the lateral effects cannot be obtained.

(g) In addition to the maximum at 0° phase difference, there are also two additional maxima occurring one before and the other after opposition in phase, but distinctly perceptible only when the beats are less frequent than one in two to five seconds. These additional maxima were first studied by Paul Rostosky[21] in 1902, but in the succeeding literature no reference is made to his important contribution. Thus the subsequent discovery of these secondary maxima by the writer was an independent one.

Observations with Mr. F. C. Bruene

The tones were obtained with two tuning forks, the sounds being led to the ears by binaurals inserted[22] in the opening of the external meatus. Phase relations were determined by the Lissajou's figures made by a beam of light reflected from two mirrors, one attached to each fork.

The following results were obtained with the assistance of Mr. F. C. Bruene, a graduate student in the Department of Philosophy and Psychology.

(1) The beats (i. e., maximum at 0° and minimum at 180° phase differences) with the tones of two forks, one applied to each ear, were heard distinctly. Indeed, of twenty-three inexperienced observers listening for three periods of five minutes each, seventeen noticed the beats distinctly enough to report them. All experienced observers can hear them and record them with very satisfactory accuracy.

(2) There existed a distinct wandering of the localization; in front at 0° difference of phase, on the side of the fork with the higher frequency leading in phase from 0° to 180°, and on

[19] Lord Rayleigh, Phil. Mag. XIII, 1907, p. 214.

[20] Loc. cit.

[21] Loc. cit.

[22] See section VII in which there is a discussion of the use of binaurals closing the external meatus.

the side of the slower fork with the higher frequency leading in phase from 180° to 360°, the changes of position being continuous, from front to rear, (or within the head), and from rear to front. This wandering when first recognized might be described as occurring in a circular path. A more accurate description occurs in what follows.

(3) This rotation of the localization just described was quickly and distinctly observed by some hearers; on the other hand all observers did not recognize it, even with an hour or more practice. Eleven of the twenty-three uninstructed and inexperienced observers reported their observation of this "rotation" after fifteen minutes' trial (three trials of five minutes each). Six of these eleven noticed the effect within the first five minutes, and three in the second.

(4) All of the eleven just mentioned, with the possible exception of one or two, noticed that the localization was much more distinct in the half cycle in which the lead of the faster fork varies from —90° to +90°. With experienced observers, the effect is very noticeable.

(5) Extended experiments with four experienced observers showed that the most accurate judgment as to phase difference, made by the localization, occurred when that difference is 0°.

(6) With the experienced observers the change in the localization at 0° phase difference by the production of a very marked inequality in intensities, is slight. The rotation of the localization was modified by a slight circular shift in the direction of the side of the greater excitation. The effect upon the localization at the sides became very marked, that on the side of the greater intensity becoming more prominent and that on the other almost vanishing.

(7) With observers of experience, but who had not yet recognized the secondary maxima,[23] the maximum intensity seemed to occur at 0° phase difference when the beat-period was less than perhaps two seconds. With a greater and gradually increasing period there arose an uncertainty until, with a period

[23] See pp. 35, 37 and 41 of this article and (9) below.

of perhaps five seconds, the judgment of maximum intensity shifted to a phase difference noticeably less than 180°, or approximately to the first occurring secondary maximum.

(8) The rotation of localization with experienced observers occurred as follows: At 0° phase difference the sound was localized as in front and distant, i. e., external to the head. The localization then described in a horizontal plane a path which appeared to be somewhat circular, but in which the apparent distance contracted until the path entered the ear leading in phase. Then at about 180° phase difference the localization passed quickly through the head, into the other ear and from thence around to the front along a path symmetrical to the one just described.

(9) When an experienced observer gave his attention to apparent intensity rather than to localization, there appeared three fairly distinct maxima. One occurred at 0° difference of phase, one at $180° - \delta$ and one at $180° + \delta$, δ being less than 45°. The earlier of the additional maxima at $180° - \delta$ phase difference coincided with the localization in the ear near the higher frequency, and the later one with the localization in the other ear. With a 128 d. v. fork the secondary maxima seemed clearly discernible only if the beat-period exceeded a value of approximately two seconds. The value given is only approximate as the variation in the perception of the secondary maxima is continuous with the changing of the beat-frequency. The significant fact is that the 0° maximum was always present, whereas the secondary ones required a beat-period exceeding at least a second in order to become clearly evident.

(10) The localization in the ears as described seemed to depend upon the perception of the secondary maxima. If they were absent, as at high beat-frequency, the localization seemed confined to the region near the front. In other words the localization in approximately one-half the cycle seemed to be due to the secondary maxima or the causes producing them.

Observations upon the Secondary Maxima

Again the binaurals inserted[24] in the ears were used. The observations were made with the assistance of Dr. Harold Stiles of Iowa State College. As already stated, the secondary maxima are those occurring just before and after the 180° difference in phase, or at phase differences of $180°-\delta$ and $180°+\delta$. The results were obtained by extended observations upon pairs of forks of the following frequencies: 42, 63, 128, 256, 361, and 469 d. v. The length of the beat-period and time between the secondary maxima were measured. From these values $\frac{\delta}{\pi}$ was computed. The results are shown in Tables I, II and III. The number given in parentheses after the values of 2δ in Table I and after the values of $\frac{\delta}{\pi}$ in Tables II and III, is the actual number of observations involved in the value given of 2δ or $\frac{\delta}{\pi}$. The conclusions are as follows:

(11) The difficulty of determining the exact moment of the occurrence of a maximum renders the measurement of δ inaccurate. Observations were, however, made at various times of day and the average results do seem to warrant the two conclusions that follow.

(12) For a given pair of forks δ is independent of the frequency of the beats when this frequency is varied from one to three times. This is shown especially in Table I.

(13) δ does vary with the fork frequency, and this variation is not linear, varying less rapidly than the frequency. This is shown by the averaged values taken from all the tables.

A Theory of Binaural Beats

The phenomena already mentioned are complex and hence we will divide the theoretical discussion into three parts, namely, the primary maxima, the secondary maxima and the localization.

[24] See footnote 22.

TABLE I

Fork of 128 d. v.

Date	Period of beats	2δ in seconds	$\frac{\delta}{\pi}$	$\frac{\delta}{\pi}$
Aug. 3	27.8		.20	
	12.3	2.60(7)		.21
	12.2	3.50(7)		.29
	13.1	2.75(8)		.21
Aug. 10	35.7	7.9 (4)	.22	
	35.5	7.2 (6)	.20	
	36.0	10.3 (9)	.29	
	13.7	2.95(12)		.22
	14.0	3.6 (11)		.26
Aug. 11	35.2	7.9 (4)	.22	
	13.3	2.68(10)		.20
	13.5	3.1 (6)		.23
	Mean		.23	.23

TABLE II

Forks of 42, 63, and 128 d. v.

Fork of 42 d. v. Date	Beat Period	$\frac{\delta}{\pi}$
Aug. 11	20.4	.19(10)
	20.2	.20(8)
Aug. 12	22.6	.18(10)
	23.7	.16(8)
	22.6	.18(9)
	24.5	.15(11)
	21.6	.17(13)
	21.5	.18(10)
	Mean	.17

Fork of 63 d. v. Date	Beat Period	$\frac{\delta}{\pi}$
Aug. 3	10.1	.20(11)
	10.7	.20(13)
	10.8	.18(11)
Aug. 5	10.2	.18(4)
	19.5	.16(6)
	19.0	.14(4)
	Weighted Mean	.18

Fork of 128 d. v. Date	Beat Period	$\frac{\delta}{\pi}$
June 27	23.2	.25(2)
	10.1	.29(8)
June 29	23.9	.24(3)
	31.0	.22(3)
	16.0	.27(4)
July 2	35.8	.22(4)
July 3	27.8	.20(6)
	12.3	.21(7)
	12.2	.29(7)
	13.1	.21(8)
July 10	36.0	.29(9)
	35.5	.20(6)
	35.7	.22(9)
	13.7	.22(12)
	13.7	.25(10)
	14.0	.26(11)
	13.6	.20(7)
July 11	13.3	.20(10)
	13.5	.23(9)
	35.2	.22(4)
	Mean	.23

The Primary Maxima.—If we assume that there is sound conduction across the head by paths not entirely aerial, and that the organ of hearing detects actual intensity, or an effect which depends upon the square of the amplitude of vibration, and if we further assume that there is a blending which results

TABLE III

Forks of 256, 361, and 469 d. v.

Fork of 256 d. v.

Date	Beat Period	$\frac{\delta}{\pi}$
July 13	13.2	.26(9)
" "	12.2	.29(9)
" 14	43.8	.26(4)
" "	27.4	.27(8)
" "	23.4	.21(8)
" "	12.0	.28(11)
" "	10.5	.26(9)
" "	9.25	.27(9)
	Mean	.26

Fork of 361 d. v.

Date	Beat Period	$\frac{\delta}{\pi}$
June 26	15.9	.28(9)
" "	8.2	.29(10)
July 20	17.6	.28(6)
" "	22.3	.26(7)
" "	17.1	.25(8)
" 27	10.7	.27(9)
" "	10.2	.24(15)
" "	11.7	.30(5)
" "	9.1	.28(9)
" "	12.5	.22(7)
Aug. 1	12.7	.30(6)
	Mean	.27

Fork of 469 d. v.

Date	Beat Period	$\frac{\delta}{\pi}$
Aug. 5	4.2	.32(1)
" "	4.0	.37(2)
" "	6.6	.27(4)
" "	5.6	.35(2)
" "	5.0	.38(2)
" "	5.0	.26(3)
" "	7.0	.27(3)
	Mean	.32

in the effects at the two ears appearing as one, we obtain the result that the total sensation must vary with the following expression:

$$2(\alpha^2+\beta^2)+4\alpha\,\beta\cos\epsilon\cos\lambda$$

where α and β are constants, ϵ is the phase by which the faster fork leads the slower, and λ is the difference between the phases introduced in the two paths, one from the source to the nearer ear and the other from the source to the farther ear. Thus ϵ is the only variable in the expression, and hence the total sensation must pass through a maximum and a minimum (not zero), the former corresponding to $\epsilon=0°$ and the latter to $\epsilon=180°$. According to the experimental results just presented, this theory agrees with the fact that the maximum and minimum do occur at the phase difference specified, the fact that the minimum is not zero; and the fact that phase differences for maxima and minima are independent of either fork-frequency or beat-frequency.

The Secondary Maxima.—The only theory proposed for the explanation of the secondary maxima has naturally been that of the only previous observer, P. Rostosky.[25] He contended

[25] P. Rostosky, Philosophie Studien, 1902, 19, p. 557.

that they were caused by the variation in intensity-ratio, but, unfortunately, in the interpretation of his equations, he made an error[26] which when corrected makes his theory contradict experimental facts.

In order to explain not only the secondary maxima, but various other related phenomena, we are compelled to make several assumptions in addition to the ones just stated in the preceding discussion. An inspection of the experimental results will convince that the secondary maxima must have an origin quite independent of the primary maxima. The assumption will therefore be that the organ of hearing is, from a physical point of view, duplex. A pair of "A" instruments is responsible for the primary maxima and a pair of "B" instruments is responsible for the secondary maxima. We accept, then, the existence of the B instruments and in addition make the following assumptions: that the blending with the B is not as perfect as with A instruments, that with the former there is introduced an additional change of phase of 180° in the wave transmitted across the skull and that the B instrument is excited by the skull rather than by the air-membrana tympani route. We find by a theoretical investigation that the intensity in the ear nearer to the faster fork reaches a maximum at $180° - \lambda$ and in the further ear at $180° + \lambda$, where λ is as previously defined. Thus we have explained the existence of the secondary maxima as the recognition of the individual intensities at each B instrument, and λ is identified as the δ observed experimentally. Thus the result stated under (g) is explained. If the B instruments are excited by the skull, which is comparatively massive, a certain length of time would be required to set up any vibratory motion in these B instruments. This is in accord with the experimental fact, as expressed in (9), that the secondary maxima are not in evidence unless the primary beat-period exceeds one or two seconds. The fact that δ is independent of beat-frequency as stated in (12) above, is due to the fact that the retardation λ (in the theory) depends only upon the frequency of vibration and the

[26] See article on "Theory of Binaural Beats," by G. W. Stewart, in the Physical Review for 1917.

conducting paths and not upon the rate at which the phase differences of the sources are changing, or upon the beat-frequency.

The fact that δ is dependent upon the fork-frequency, as stated in (13) above, is in agreement with the theory, for the time retardation along any path is constant and the phase retardation is strictly proportional to the frequency. Furthermore it can be shown[27] that when there are numerous paths between two points, the resulting phase retardation cannot in general be a linear function of the frequency. Thus our theory is in harmony, qualitatively, with the measurements of δ.

The failure to recognize the secondary maxima at the very beginning of one's experience with binaural beats, (7), can be explained by our assumption that the blending of the sensations produced by the B instruments is only partial.

The Localization. Our theory is in accord with the experimental fact that during a portion of the beat-cycle, *i. e.,* in the region $\epsilon = 0°$, the localization is external, and in the portion near the region $\epsilon = 180°$, the localization is internal. For in the former case the excitation (of the A instruments) is by the usual means, air-drum-skin-etc., and in the latter case, the excitation (of the B instruments) is through the skull which, as we know by direct experiments, (8), gives an internal localization.

In order to bring our theory into agreement with all the experiments on localization described above, we must make two additional assumptions, namely, that the localization with the A instruments depends chiefly upon phase difference and that with the B instruments chiefly upon intensities. The former assumption will perhaps not be regarded by all as a reasonable[28] one, but the latter involves a principle generally accepted.

At $\epsilon = 0°$, the resulting phase difference at the pair of A instruments is zero and hence the localization is in the median plane. The fork near to an ear will furnish the greater portion of the amplitude and thus the variation of phase at each A instrument can be assumed to be chiefly determined by the phase

[27] See article on "Theory of Binaural Beats," Physical Review, 1917.

[28] J. Peterson, loc. cit., assumes that the perception of phase differences is cortical in origin.

of the nearer fork. Thus the localization in the region, $\epsilon = 0°$ follows, approximately, the variation of ϵ. And we note that the movement of localization, experimentally, is in accord with this result, for it seems, as in (8), to proceed at a uniform angular velocity.

In the region $\epsilon = 180°$, the excitation of the B instruments is the more prominent and hence controls the sensation experienced. That the localization at or in an ear occurs both at $180° - \delta$ and $180° + \delta$, where δ is less than, say, $45°$ is in accord with theory in that λ (or δ) must be small because of the high velocity of conduction in the bones of the skull. As yet the quantitative agreement, if we accept possible values of the velocity of sound in the bones, is not very good. But the possible conducting paths are numerous and do not actually terminate on the opposite side of the head from the source, and hence a quantitative agreement should not be anticipated.

The fact that δ is less than $45°$ makes the angular velocity of localization apparently much more rapid, as in (8), in the region $\epsilon = 180°$.

The continuation of the localization in the region $\epsilon = 0°$ and the absence of the localization in one ear when the source on that side is made of much less intensity, as in (10), is in accord with the above theory, for in the first case the difference of phase is only slightly dependent upon the equality of intensity, and in the second the maximum of intensity would be blotted out by the partial blending.

The difference in distinctness of the localization, described in (4), and the most accurate judgment of phase difference at $\epsilon = 0°$ are experimental facts which are also in accord with our theory.

These explanations bring our theory of binaural beats into apparent harmony with the experimental facts concerning localization recorded above in (c), (d), (2), (4), (5), (6), (8) and (10).

The fact that the rotation of localization is not found with all observers is doubtless to be explained upon the basis that

the localization is a "second-order effect" as compared with intensity observations. The theory as here presented does not explain why the rotation of localization in the region, $\epsilon = 0°$, is similar with all frequencies, the same difference in phase meaning the same displacement. This phenomenon may have a wholly physical basis, but that is not certain. Full consideration has not yet been given to this point.

Anatomical Considerations

The query arises, does the structure of the ear permit of the assumptions as made? It would seem that no serious objection arises if we assume the instrument A to be the organs of Corti in the ductus cochlearis, and the instrument B to be the analogous organs in the saccule or the utricle. These organs in the cochlea and the saccule and utricle, have a common origin. Herrick[29] states that some physiologists have thought that cochlear and vestibular nerve systems are not wholly distinct and that the sense organs in the saccule may also function as a sound perceptor. This uncertainty concerning the function of the organs in the utricle and saccule on the part of physiologists does not serve as an objection to our theory. Indeed, the fact that the complex phenomena herein described can be accounted for by the assumption of the existence of the B instruments ought to assist in deciding this uncertainty. The fact that there is a great difference in the central connections of the cochlear and vestibular nerves is in harmony with our contention that the instruments B give sensations which do not completely blend and which do not produce external localization.

But are the vestibular and cochlear organs mechanically different? Obviously they are, for the latter are placed upon a membrane, and are apparently excited by the motions of the fluid in the scala media, and the motions of the basilar membrane and the tectorial membrane. The vestibular organs are located at the points where the membranous labyrinth is fastened to the skull itself. Moreover, the movement of the vestibular fluid must be very small for the vestibular canals are closed.

[29] G. J. Herrick, Introduction to Neurology, pp. 201-202.

Thus there would seem to be a mechanical difference which corresponds admirably with our assumption that instrument A is excited via the vibration of the membrana tympani and that instrument B is excited by the skull. The only change in our assumption induced by the structure of the ear would be the possible insertion of the word "chiefly" after each word "excited" occurring in the preceding sentence.

A paper of importance has just come to the writer's notice. D. Richards[30] reports upon experiments with a guinea pig in which there were removed (1) both cochleae, (2) both cochleae, and one vestibule and (3) both labyrinths. His conclusion is that sound stimuli may be regarded as adequate stimuli for the vestibular apparatus, but that no conclusion can be drawn as to any sensation produced in this way. This contribution is entirely favorable to the theory presented in this paper.

Restatement of Theory and Agreement with Experiment

By the assumption of two physical instruments A and B on each side, having a difference in mechanical arrangement, a difference in the method of excitation, a difference in the blending of the sensations of the A pair and B pair of instruments and an independence of sensations produced by the two pairs and the additional assumptions that instruments A, the usual instruments of hearing, can perceive phase differences, and that in the skull conduction to the B instruments there is an additional phase change of 180°, we have a theory that explains fairly satisfactorily the presence of the primary maxima, the secondary maxima, the appreciable length of beat-period requisite for the appearance of the latter, the localization in the entire beat-cycle, the difference in the nature of the localization and in the quality of the sound in the two halves of the beat-cycle, the rapid movement of localization between the secondary maxima, the variation of the separation of the secondary maxima with the fork-frequency, and its independence of beat-frequency, the difference in the effects of unequal intensities upon the localization in the two halves of the beat-cycle, and finally the

[30] D. Richards, Zeitsch. f. Biol. 1916, 66, pp. 579-609.

simultaneous disappearance of the secondary maxima and the localization in the region of 180° phase-difference.

Our theory does not lead us to identify the B instruments, but merely to suggest the possibility of these instruments being located in the utricule and saccule. Anatomical and physiological considerations give evidence in favor of the suggestions.

Inasmuch as all of the experiments recited in the foregoing pages were performed with binaurals that were inserted into the ears, the query naturally arises as to whether or not the phenomena involved are dependent upon this insertion of binaurals. From a physical point of view the partial closing of the external meatus would not be likely to change the characteristics of the phenomena. But the actual experimental evidence of the truth of the similarity of results with inserted binaurals and binaurals free from the head is obtained by a comparison of the results of Rostosky,[31] who used the latter type of binaurals, and of the writer who tried both in all experiments excepting the quantitative measurement of δ. The results of these two investigators are in entire agreement. Hence it is believed that the theory is equally applicable to open-ear-binaurals.

The theory is, of course, incomplete, but it has herein proved to be very successful and hence promising. There are no conspicuous phenomena connected with binaural beats that are not in accord with the theory, but there are minor points that should receive further consideration. For example, S. P. Thompson[32] has found binaural beats occurring when the tones used have frequencies of almost two to one.

In the theory presented it is assumed that the sensation produced depends upon the intensity or upon the square of the amplitude of vibration. But this assumption is open to question. Might not the sensation vary with the absolute value of the amplitude? Might not an assumption be made that will permit of both the primary and secondary maxima without a necessity for the hypothesis of a second organ of hearing? The first question has been answered by a theoretical investigation

[31] Loc. cit.

[32] S. P. Thompson, Phil. mag. (5), IV, 1877, p. 274.

assuming the variation of the sensation to depend upon the absolute value of the amplitude. This investigation shows that the same maxima and minima occur as with the assumption of sensation as a function of intensity. But the second question remains unanswered. The writer merely opines that no assumption as to the variation of the sensation will lead to the possibility of the primary and secondary maxima without the additional assumption of two hearing instruments.

I wish to express my thanks to Mr. F. C. Bruene and to Dr. Harold Stiles for their assistance in different portions of the experimental work herein described.

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CORRELATION OF FACTORS IN MUSICAL TALENT AND TRAINING

BY

CARL E. SEASHORE AND GEORGE H. MOUNT

CONTENTS

INTRODUCTION

We have here undertaken to assemble and interpret, for the technical reader, some of the data available in this laboratory on the interrelations of various factors in musical talent.

For the last fifteen years or more, a mass of data bearing on this subject has been accumulating until it is now unquestionably the largest collection of its kind. Most of these data have been presented, or will be treated in the discussion of individual problems by different investigators; but the presence of this material suggests the possibility of a mass-survey of the various kinds of records in this series for the purpose of finding out something about the inter-relationships of the factors under consideration.

All the records considered have been taken upon University students in the elementary course in psychology, all in or above the sophomore year. The plan has been to take the entire class in order to eliminate the error of selection. The tests have been made under very favorable conditions, either as class tests under accurate control or as individual tests in the special measuring rooms of the psychological laboratory.

Some of the tests have been made for more than ten successive years on from one hundred to three or four hundred different students. Instead of attempting to use all of the data available, we have selected certain groups, or cross sections, which are representative, namely the records taken in the years '07, '09, '11,

and '16. This selection excludes a few relevant tests which were taken only in other years.

Dr. Mount originally undertook this task and completed his doctor's thesis on this subject in 1910, having developed the material given here for the years, 1907 and 1909. The fact that the same investigation was continued by his immediate followers led to the postponement of publication and, during the time that elapsed, certain parts of the tests particularly the section on the distribution of musical talents, have been superceded by more intensive studies along some of these lines, notably, the measurements on pitch by Smith (16), and on singing by Miles (6).

In Dr. Mount's original treatment of this subject, a new method of expressing correlations was developed consisting essentially of a characteristic figure, (K), representing the distribution of records into quartiles or octiles, a figure somewhat analagous to the "r" in the Pearson coefficient. Since this method has not been followed up in later researches, it was thought best to use the coefficient R of the Spearman foot-rule in the present article in order that these data might be more nearly comparable to those contained in the data worked over by his followers.

It would be poor economy for the reader to have Dr. Mount's original interpretation of the correlations presented separately from those of his followers. Therefore, with his courteous consent, the correlation data which he had worked out are here used as a part of the larger body of the same kind of material, and his original thesis will not be published for the reasons given; namely, that other parts of his thesis are superceded by later work now published, that the reading public will welcome the whole report as one unit, and that his original interpretations, in so far as they are substantiated, are embodied in the present interpretation of the material as a whole.

The group of data for the year, 1911, was collected by Dr. Rollin M. Stewart, who put much valuable time and energy into the evaluation of the questionnaire returns and the ranking of students on the questionnaire. He did not find time to work out the correlations and write up the subject himself, and therefore turned over the material to the writer.

The materials for 1916 were assembled from the various in-

vestigations by Mrs. Esther Allen Gaw to whom the writer is also indebted for continued help in the work in her capacity as research assistant in the field of this investigation. The first named of the authors has written this entire article on the material mentioned and takes all responsibility for the interpretations herein presented. He wishes to express his hearty appreciation of the coöperation of many whose names are not mentioned. He, and all the other workers in this field, are under special obligation to Dr. Mabel Clare Williams for her valuable guidance and supervision of these tests.

The terse account of the various tests which follows can be excused only on the ground that in most cases reference is given to published accounts which should be read in detail if the procedure is to be fully understood or repeated.

It would not seem necessary to go into details as to the conditions of the experiments, which would in themselves fill a large volume, because our only interest in the present issue is in the correlation problem; it is sufficient to know that the same procedure was followed for all the members of a given group or year for which the figures have been compared.

Likewise one might go into great detail in the discussion of the merits and demerits of various methods of computing and representing correlations, means of attenuating, weighting, etc., but that we must also forego and proceed by the simplest method. It should be said that the demand for considering attenuations of correlations and other adjustments was reduced to a minimum by the fact that the measurements were made under good laboratory control, and that the number of cases on which each correlation was based is relatively large. In '07 the minimum was about 190, in '09 about 200, in '11 about 225, and in '16 about 225, but each year a much larger number of cases was taken for some of the correlations.

As stated above the Spearman foot-rule was used for '07 and '09 because much of the material could be best handled by the method of ranking. This was also used for the questionnaire returns in '11. Otherwise, in '11 and '16 the coefficient "r" of the Pearson product-moments method was used. This diversity of standard is unfortunate and must be kept in mind in reading.

The Spearman method, larger formula, gives the higher correlation as is shown by comparing the "R" and the "r" in two actual items.

'07 R, .40, p.e. .04 is equivalent to r, .59, p.e. .03

'09 R, .49, p.e. .03 is equivalent to r, .70, p.e. .02

The general formula by which one may be converted into the other in a very rough estimate is either $r = \sin\left(\frac{\pi}{2} R\right)$ which is Spearman's empirical formula, or $r = 2 \cos \frac{\pi}{3} (1-R)-1$ which is Pearson's theoretical formula to give the r of the product-moments method from Spearman's R.

SOURCES OF MATERIAL

1. *Pitch discrimination.*—This measurement was made according to the standard conditions laid down by the writer in the article on the standardization of measurement of pitch discrimination (12). Briefly, the standard was 435 v.d. and ten differential forks ranged from ½ v.d. approximately in a geometric ratio of the second order up to 30 v.d. The tones were sounded through Helmholtz resonators. The "heterogeneous" method, described at length in the above reference (12), was followed, and it was found possible to make from 170 to 200 trials in the one hour.

These tests were made on groups of students from 100 to 200. In '09 this test was followed by individual tests, using the method of constant stimuli at the threshold found in the group test. The records for '09 were accordingly labeled "group tests" and "individual tests" respectively, and the plain "P.D." in Table II, without qualification, denotes the mean between the two.

In '09 a practice series of three periods was also given. The observers were divided into three divisions on the basis of records made in the first test. Division A included all whose threshold was below 4, Division B those whose threshold was between 4 and 10, and Division C those whose threshold was above 10. The three divisions were tested simultaneously but in different rooms by different experimenters. Division A was given fifty trials upon each of the increments 5, 3, 2, and 1 respectively. Division C was tested in the same way upon increments 12, 8, 5, and 3, respectively, and Division C was tested upon increments

30, 17, 13, and 12 respectively. The threshold was determined by applying the Fullerton-Cattell formula (3) with reference to the cases in which the records were between 65 and 85 per cent.; records on other intervals were discarded except when new intervals fell within the limits indicated.

Measurements in 1907 were made by the writer and Dr. Williams, in 1909 by Dr. Mount, in 1911 by Dr. Smith (16) and in 1916 by Mrs. Gaw.

2. *Consonance.*—All the measurements on consonance were made by the method described in Dr. Malmberg's article on the perception of consonance in this volume (5). Briefly, the procedure was this: Malmberg had ranged all possible two-clangs in the octave from c′ to c″, in the order of consonance-dissonance, for the piano. This order was used as a norm and the measurement was made by the method of paired comparison, each two-clang being compared with every other as sounded on a well tuned piano. Errors were then checked by the norm named above, and these errors were weighted according to the magnitude of displacement from the true order according to the norm. Thus if c′d′ was said to be better than c′c″ a demerit of 11 was given. If c′d′ was said to be better than c′g′ a demerit of 9 was given, etc., there being a scale of demerits from 11 to 1 according to the magnitude of the error. The record was kept in terms of the per cent. of weighted credits for right judgments above the one-half which would be right by chance. For full understanding of this method it is necessary to read Malmberg's original statement.

A critical feature of this test is the definition of consonance. It was pointed out to the class that consonance may be regarded from two points of view, that of feeling and that of knowing, and that our point of view was the latter, and consonance was accordingly defined by the following placard:

1. Blending: a seeming to agree, to belong together, to be relatively pure.
2. Smoothness: relative freedom from beats.
3. Purity: thinness of tone.

This was read and interpreted to mean, "When the two tones of a two-clang tend to blend or fuse and produce a relatively smooth and pure resultant, they are said to be consonant."

The '11 records were taken by Dr. Malmberg and the '16 records by Mrs. Gaw. Attention should be called to the fact that this test has since then been modified as reported by Mrs. Gaw in this volume (4).

3. *Intensity discrimination.*—The measurement of discrimination for intensity of sound was made in the light-, sound-, and jar-proof room (7) of the psychological laboratory with the audiometer (10). The interrupted current reduced to 1.8 volts from a 100 v.d. tuning fork was sent through the audiometer. This produced a tone which was clearly audible. The strength of this tone was kept constant.

The '11 tests were made by Mr. W. S. Smiley and the '16 by Miss Marie M. Agnew.

4. *Hearing ability.*—Hearing ability was also measured by means of the audiometer (10) in the light-, sound-, and jar-proof room (7).

The audiometer was used, as above, but without the tuning fork, and the current was reduced to the standard strength according to the audiometer galvanometer. Ten determinations were made of the upper limit and ten of the lower limit of the threshold and the mean between the averages of these two was taken as the characteristic measure of hearing ability.

All known subjective and objective sources of error were under reasonable control. The record was taken for the better ear according to the judgment of the observer.

These tests were made by Mr. W. S. Smiley in '09 and by Miss Marie M. Agnew in '16.

5. *Time-sense.*—The measure of time-sense was the least perceptible difference in the length of two empty intervals marked off by three clicks in a telephone receiver. In 1911 the timing was done by the synchronous motor as described by Ross (9). An adjustable contact lever was attached to the shaft of the motor in such a way that when the motor revolved at the rate of one revolution per second, this contact could be shifted from the standard point, for one of the intervals, making it shorter than one second by a given differential step. The steps employed were 1, 2, 3, 5, 8, 12, and 17 hundredths of a second.

In 1916 the same measurement was made by means of the

Dunlap pendulum (2) which is a very large and accurately built seconds pendulum. In this case the middle click, *i.e.,* the click at the end of the first interval, was placed earlier or later than the standard according to the same differential steps as before. This resulted in the lengthening of one interval and the shortening of the other whereas in the '11 test with the synchronous motor one interval was kept constant and the other shortened. This difference was, however, of no consequence for the present purpose. The pendulum was substituted for the synchronous motor in order to develop a norm for the pendulum which can be used in simple and readily transportable form.

The '11 tests were made by Mr. Ross (9) and the '16 by Miss Pauline Peters.

6. *Motor ability.*—The test of motor ability was the customary procedure of tapping with a telegraph key adjusted with a spring requiring about 50 grams of pressure and moving through a distance of one millimeter. The record was made with a multiple recorder tracing on ticker tape by the method described for the psychergograph (9) by the writer. This multiple recorder is a sort of a "universal apparatus" for the laboratory, convenient for all sorts of prolonged time and signal records. The time line was traced from a 25 v.d. tuning fork. When indelible leads are used and the tape is run under a moist sponge, the moisture of the paper develops the light tracing into a clear and permanent record.

This and the next three tests were made by Miss Pauline Peters in '16.

7. *Free rhythm.*—Free rhythm was measured in the terms of the average error in the free marking of time at the suggested rate of approximately one beat per second. The record was made on the multiple recorder as above.

We have followed custom in calling this rhythm. It is really not a rhythm test but a test of motor-time, or time-action. The same is true of "regulated rhythm" also.

This and the next test on regulated rhythm were made by Miss Margaret Cummings in '11.

8. *Regulated rhythm.*—Regulated rhythm was measured in terms of the average error in keeping time with a click produced

at intervals of one second in a telephone receiver operated through a seconds pendulum. The original object of this experiment was to study personal equations but for the present purpose only the average error was counted.

9. *Rhythmic judgment.*—"Rhythmic judgment" is something of a misnomer. It consisted in marking time with the telegraph key through the multiple recorder (13) as before, the instructions being to mark time, giving in each measure two short and one long intervals. The record was made in terms of the deviation of the long interval from the sum of the two short.

10. *Singing the keynote.*—All measurements on the singing of pitch were made by means of the tonoscope (14). Singing the keynote was measured in terms of the average error in reproducing the standard tone sounded by a tuning fork before a resonator. In most cases the standard was 256 v.d., the women being allowed to sing this pitch and the men an octave lower. This method was followed because the men preferred to take the key from a 256 v.d. fork rather than from a 128 v.d. fork, as a result of association with notes on musical instruments. This measurement was not made in and by itself but was taken from the singing of the keynote in singing intervals, scales or minimal change; but for each year it was the same for the entire group, which is all that is necessary for us to ascertain for the present purpose. It will be observed that the note sung was in the middle register of voice both for men and women.

All the measurements of singing were made by Dr. David Allen Anderson (1) in '09, by Dr. Walter R. Miles (6) in '11, and by Esther Allen Gaw in '16.

11. *Singing the interval.*—In '09 the test consisted of the singing of the three intervals, the major third, the fifth, and the octave after sounding the keynote in each trial, the measure being in terms of the average error, as calculated from the standard as sung rather than as sounded by the keynote.

In '16 this test consisted of the singing of the first two phrases of America, record being made of the average error in the sounding of the notes which occur for the words, *my, 'tis, sweet, of, of, sing.*

12. *Singing the scale.*—Each observer was required to sing

the natural scale within a middle register of the voice, in just intonation.

13. *Voluntary control of pitch.*—This is the measurement of the least producible difference, or the minimal change, in sharp or flat as described by Miles (6).

14. *Singing.*—This ranking is based on the mean between the singing of the key and the minimal change as described above, a single measure being used for the two.

15. *Auditory imagery.*—In '16 the test as outlined in the writer's Manual of Elementary Experiments in Psychology (11, Ch. IX), was supplemented by a one-hour experiment devoted entirely to auditory and motor imagery in which the stimuli were a broken melody, printed words, spoken words, and sounds to be imaginally imitated. The records used for the purpose of correlation were the mean between the Manual test and the record for this special test on auditory imagery. The method of ranking the images described in the Manual was followed and the records were kept in terms of the percentage of points possible.

The '11 tests were made by Dr. D. J. Macdonald, and the '16 tests of this and motor imagery by Miss Marie Agnew.

16. *Motor imagery.*—Motor imagery was measured according to the directions given in the Manual of Elementary Experiments in Psychology (11, Ch. IX), section on motor imagery. This was supplemented by a test on the imaging of a series of tones. The mean between these two records was used as the final grade.

In this test, although the observer was warned to distinguish between motor sensations and motor imagery, it is safe to assume that this was not done successfully; and it is probable that much of what the untrained observer did record as motor imagery may have involved motor sensations.

17. *Tonal memory.*—Tonal memory was measured in terms of the memory span for notes on the piano according to the method developed by Sodergren (17). Briefly, a series of notes in the middle octave of the piano was sounded. Immediately thereafter the same series, with but one change, was sounded and the observer was required to tell which of the notes was different in the second stimulus group. The spans of two notes, three notes,

four notes, five notes, and six notes were used under controlled conditions. The test lasted one hour and the record was made in terms of the percentage of right cases in all the trials. From these records an evaluation was worked out showing to what a given percentage of right cases in the total number of trials would be equivalent in terms of 75 per cent. of right cases on a given span.

This test was made in '16 by Mrs. Gaw.

18. *Supplementary information.*—All persons examined were requested to fill out the following questionnaire:

Name.................................... Age.......
Date................................ Hour...... Class......

Please give as specific and detailed information as possible in regard to your:

I. Musical Training
   1. In public schools
   2. Private vocal lessons (when, where, how long, etc.)
   3. Private instrumental lessons (when, where, how long, etc.)

II. Musical Environment
   1. Instruments in your home, and their use
   2. Musical encouragement at home (trained voices in family)
   3. Opportunities for hearing music of any sort (specific)

III. Musical Expression
   1. Favorite selections you can sing (by ear? by note?)
   2. Favorite selections you can play (by ear? by note? instruments?)
   3. Singing or playing in public (parts, occasions, etc.)

IV. Enjoyment of Music (What Actually Appeals to You)
   1. Vocal (solo, quartette, chorus, opera, popular songs, classics, religious, secular)
   2. Instrumental (solo, symphony, band, etc.)
   3. Characteristic effects of music (mental, physical)

All were charged to make the information as full, specific, and concrete as possible. Often a questionnaire was returned for more accurate information. In many cases this information was supplemented by personal interviews on significant points. Notes were also taken in the various tests placing on record facts which could be used in this section.

All the ratings in the section were made without knowledge of the objective records. The rating is arbitrary, but was, nevertheless, done with extreme care and patience in the effort to sift the facts. Various devices were used for the evaluation of answers in the different years. The record for musical training being made in terms of hours of instruction, public school train-

ing had to be reduced to this unit. Vocal and instrumental lessons were treated with different value.

The most elaborate evaluation is that worked out by Dr. Rollin M. Stewart for '11, but it is not necessary to go into details about this procedure because the same method was followed for all in the entire group for a given year and the title of each item is perhaps sufficiently descriptive.

Mount differentiated instrumental training from vocal training for the purpose of tracing the relationship of each of these two factors separately. The first item on the questionnaire, musical training, is, therefore, based on a composite record of instrumental and vocal training. The same principle of division was followed for "musical expression."

TABLES OF CORRELATION

The records for the four years selected are presented in Tables I to IV. Attention is called to the fact that the first two and a part of the third are in terms of the Spearman R; and that the third, in part, and the fourth wholly are in terms of the Pearson r. Absence of sign indicates a positive correlation.

TABLE I—*Series of 1907*

	P.D.	M.Tr.	I.Tr.	V.Tr.	M.Env.	M.Ex.	I.Ex.	V.Ex.
Musical Training	.31 .04							
Instrum. Training	.27 .04	.94 .00						
Vocal Training	.20 .05	.34 .04	.23 .05					
Musical Env.	.12 .05	.26 .05	.25 .05	.15 .05				
Musical Exp.	.38 .04	.51 .03	.42 .03	.25 .05	.35 .04			
Instrum. Exp.	.28 .04	.65 .02	.68 .02	.12 .05	.38 .04	.77 .02		
Vocal Exp.	.32 .04	.14 .05	.06 .05	.24 .05	.21 .05	.61 .03	.23 .05	
Musical Enj.	.27 .04	.22 .05	.23 .05	—.04 .05	.03 .05	.12 .05	.13 .05	.08 .05

The upper figure in each pair is R of the Spearman foot-rule formula; the lower is the p. e. of R.

	P.D.	P.D. Av.	P.D. Ind.	S.K.	S.I.	S.Sc.	S.	M.T.	I.T.	V.T.	M.Env.	M.Ex.	I.E.	V.E.
P.D.	.64													
Av.	.02													
P. D.	.68	.68												
Ind.	.02	.02												
Singing	.14	.32	.21											
Keynote	.05	.04	.04											
Singing	.15	.28	.19	.45										
Interval	.05	.04	.05	.04										
Singing	.24	.34	.25	.42	.47									
Scale	.04	.04	.04	.04	.03									
Singing	.26	.37	.29	.76	.75	.78								
	.04	.04	.04	.02	.02	.01								
Musical	.16	.19	.10	.14	.23	.18	.18							
Training	.04	.04	.05	.05	.04	.05	.05							
Instr.	.15	.16	.11	.11	.39	.14	.18	.96						
Training	.05	.05	.05	.05	.04	.05	.05	.00						
Vocal	.18	.31	.20	.26	.15	.07	.17	.28	.13					
Training	.04	.04	.04	.04	.05	.05	.05	.04	.05					
Musical	.17	.28	.17	.24	.18	.16	.22	.30	.28	.12				
Env.	.04	.04	.04	.04	.05	.05	.04	.04	.04	.05				
Musical	.31	.42	.36	.23	.24	.31	.32	.48	.47	.26	.30			
Exp.	.04	.04	.04	.04	.04	.04	.04	.03	.03	.04	.04			
Instr.	.22	.29	.29	.15	.21	.25	.32	.60	.66	.14	.34	.79		
Exp.	.04	.04	.04	.05	.04	.04	.04	.03	.02	.05	.04	.01		
Vocal	.22	.31	.27	.24	.28	.34	.34	.07	.00	.54	.11	.56	.18	
Exp.	.04	.05	.04	.04	.04	.04	.04	.05	.05	.03	.05	.03	.04	
Musical	.13	.12	.07	.04	.05	.06	.01	.03	.05	.19	.15	.16	.20	.19
Enj.	.05	.05	.05	.05	.05	.05	.05	.05	.05	.04	.05	.04	.04	.04

The upper figure in each pair is R of the Spearman foot-rule formula; the lower is the p. e. of R.

TABLE III—*Series of 1911*

	P.D.	C.D.	I.D.	F.R.	R.R.	S.	A.I.	M.I.	M.T.	M.Env.
Cons.	.33									
Discr.	.05									
Intensity	.13	—.03								
Discr.	.05	.05								
Free	.27	.03	—.05							
Rhythm	.05	.05	.05							
Regulated	.01	—.11	.18	.04						
Rhythm	.05	.05	.05	.05						
Singing	.12	.04	.14	.26	.07					
(Ave.)	.05	.05	.05	.05	.05					
Auditory	.29	.14	.05	.22	.17	.07				
Imagery	.04	.05	.05	.05	.05	.05				
Motor	.02	—.02	—.05	.06	.11	—.07	.52			
Imagery	.05	.05	.05	.05	.05	.05	.04			
Musical	.21	.14	.03	.06	.03	.09	.21	.04		
Training	.03	.03	.03	.03	.03	.03	.03	.03		
Musical	.13	.03	.01	.09	.07	.05	.13	—.03	.28	
Env.	.03	.03	.03	.03	.03	.03	.03	.03	.03	
Musical	.06	—.02	.01	.04	—.01	.06	.11	.11	.10	.13
Enj.	.03	.03	.03	.03	.03	.03	.03	.03	.03	.03

The upper figure is the r of the Pearson product-moments method; except in the last three lines, for musical training, musical environment, and musical enjoyment, which represent the Spearman R; the lower is its p. e.

TABLE IV—*Series of 1916*

	P.D.	C.D.	I.D.	H.A.	T.S.	M.A.	F.R.	R.R.	R.J.	S.K.	S.I.	V.C.	A.I.	M.I.
Cons. Discr.	.21 .04													
Intensity Discr.	.09 .04	.11 .04												
Hearing Ability	.12 .04	.01 .04	.16 .04											
Time Sense	.17 .04	.07 .05	.15 .04	.03 .04										
Motor Ability	—.02 .05	.03 .04	.01 .04	.24 .04	.01 .04									
Free Rhythm	.18 .04	.03 .04	.17 .04	.005 .04	.08 .04	.09 .04								
Regulated Rhythm	.21 .04	.01 .04	.05 .04	.09 .04	—.01 .04	.12 .04	.14 .04							
Rhythmic Judgment	.22 .04	.30 .04	.001 .04	—.06 .04	—.03 .04	.03 .04	.15 .03	.00 .00						
Singing Keynote	.21 .04	.30 .04	.28 .04	.11 .04	.001 .04	.10 .04	.01 .04	.13 .04	—.04 .04					
Singing Interval	.22 .04	.21 .04	.27 .04	.32 .04	.10 .04	.21 .04	.07 .04	.23 .04	—.05 .04	.61 .03				
Voluntary Control	.38 .04	.01 .04	.10 .04	.001 .04	.01 .04	—.05 .04	.07 .04	.11 .04	.08 .04	.55 .03	.32 .04			
Auditory Imagery	.29 .04	.06 .04	.04 .04	.03 .04	.19 .04	.06 .04	—.01 .04	.001 .04	.08 .04	.24 .04	.40 .04	.29 .04		
Motor Imagery	.07 .04	.01 .04	.01 .04	.22 .04	.13 .04	.10 .04	.07 .04	.08 .04	.07 .04	.19 .04	.21 .04	.08 .04	.42 .04	
Tonal Memory	.52 .03	.23 .04	.26 .04	—.01 .04	.17 .03	.10 .04	.09 .04	.05 .04	.22 .04	.32 .04	.30 .04	.50 .03	.40 .04	.16 .04

The upper figure is the r of the Pearson product-moments method; the lower is its p. e.

COMMENT ON THE CORRELATIONS

Consonance and pitch discrimination: '11, r, .33, p.e., .05; '16, r, .21, p.e., .04.

Although the perception of consonance depends upon pitch discrimination it is not closely limited by that, for it is safe to say that eighty-five per cent. of all persons have sufficiently good pitch discrimination to hear consonance well enough to secure a record above the average in so far as it depends upon pitch discrimination alone. In other words, pitch discrimination is not the only determining factor in consonance; blending, fusion, smoothness, and purity may be perceived to some extent apart from perception of pitch, as such.

Intensity discrimination and pitch discrimination: '11, r, .13, p.e., .05; '16, r, .09, p.e., .04.

These two factors have but little in common. What there is may perhaps be thought of in general as a tendency toward ear-mindedness. Physiologically the two factors are quite independent in normal hearing.

Hearing ability and pitch discrimination: '16, r, .12, p.e., .04.

It is conceivable that defective hearing-ability (acuity) may be due to anatomical and physiological conditions which involve the organ of Corti and would therefore result in defective pitch discrimination; but most of the causes of defective hearing are due to disturbances elsewhere in the ear. The correlation may be due in part to the pathological condition in the ear, and in part to factors in psychological disposition, such as ear-mindedness and power of application.

Time-sense and pitch discrimination: '16, r, .17, p.e., .04.

Pitch and time are rightly regarded as independent variables in hearing. The variability in pitch is due mainly to structural or functional differences in the ear, whereas variability in the perception of time is due to motor and central factors. It may be that a part of the correlation is due to the capacity for improving the cognitive threshold with practice, in cases of relatively low ear-mindedness; *i.e.,* some, who are dull and are not ear-minded, may have gotten low records in both tests because of their general inability to respond to the test.

Motor ability and pitch discrimination: '16, r, -.02, p.e., .05. This tallies well with what we know of the independence of these factors; and the absence of correlation speaks well for the elemental character of these two tests. That is, each is so simple that it does not involve acquired skill or general intelligence on a large scale. The two tests were equally fair to the bright and the dull, the quick and the slow.

Free rhythm and pitch discrimination: '11, r, .27, p.e., .05; '16, r, .18, p.e., .04.

Free rhythm involves all the elements of time-sense plus musical feeling and a certain agility in action; it is the motor aspect of which time-sense is the sensory or central. The tests in free rhythm for '11 were really more reliable than those for '16. The correlation, it is reasonable to suppose, may be traced to the feeling for, or interest in, sounds which is favored by a keen ear for pitch; that is, hearing differences of pitch tends to result in expression of pitch, and all musical expression tends to become rhythmical.

Regulated rhythm and pitch discrimination: '11, r, .01, p.e., .05; '16, r, .21, p.e., .04.

Probably a mean between these two records would represent the facts in the long run. Regulated rhythm is time-sense plus precision of action. The feeling element of free rhythm is not so conspicuous. Regulated rhythm represents a higher cognitive or central affair, with temporal precision in action, and is quite independent of sensory limitations.

Rhythmic judgment and pitch discrimination: '16, r, .22, p.e., .04.

Rhythmic judgment does not involve the feeling element and here again the correlation may be laid to that factor, as in free rhythm.

Singing the keynote and pitch discrimination: '09, R, .14, p.e., .05; also R, .32, p.e., .04; '16, r, .24, p.e., .04.

Miles (6, p. 61) found a correlation between these two factors as follows; for eighty-four men, the correlation between pitch discrimination and the average error in singing the keynote was r, .04, p.e., .07; and for pitch discrimination and the

constant error in singing the keynote it was r, .08, p.e., .07. For one hundred and four women he found, for pitch discrimination and the average error, r, .27, p.e., .06; and, for pitch discrimination and the constant error, r, .11, p.e., .06. These facts of the comparatively low correlation between the hearing of pitch and singing of pitch may be somewhat surprising, but the singing test is far from elemental; and, as in consonance. the average pitch hearing is good enough for excellent pitch singing. Singing, which is motor control, often does not reach the hearing limit and in so far as it does not, there should be no correlation.

On this question of relation between hearing of pitch and singing of pitch in learning to sing, an investigation has been completed by Mr. Carl J. Knock and will be published in the next volume of these *Studies*.

Singing intervals and pitch discrimination: '09, R, .15, p.e., .05; also R, .28, p.e., .04; and R, .19, p.e., .05; '16, r, .32, p.e., .04.

Miles (6, p. 61) found, for forty-eight men, a correlation of R, .33, p.e., .08 for pitch discrimination and average error in singing the interval, and for pitch dicrimination and the constant error in singing the interval, r, .15, p.e., .07. For one hundred and four women the corresponding records were r, .51, p.e., .05, and R, -.07, p.e., .06 respectively. These strong correlations between pitch discrimination and the average error in singing the interval cannot be attributed wholly to the presence of pitch discrimination in the hearing of intervals. The common element, that is, a general sense of pitch must be taken into account. Intervals must be learned; those who have a good ear for pitch are the ones who practice and develop an interest in intervals. The correlation was, therefore, due largely to the fact that those who have a good sense of pitch take time to practice on the intervals.

The records by Miles on correlation of pitch with constant error in singing involve different factors. The constant error is due to certain motives for illusion which become fixed and habitual; by some motive for constant error, a person may have a concept of the interval which is persistently flat or sharp

by a certain amount. The constant error is in part an error of concept and in part an error of memory for that concept. This accounts for the low correlation between pitch discrimination and constant error in the interval found by Miles.

Singing the scale and pitch discrimination: '09, R, .24, p.e.. .04; also R, .34, p.e., .04; and R .25 p.e., .04.

What has been said about the average error in singing of intervals applies equally here.

Voluntary control and pitch discrimination: '16, r, .38, p.e, .04.

Of all types of singing this is the most closely dependent upon pitch discrimination because the problem is to sing the least producible difference. All things taken into account, the correlation here given seems to be characteristic of this relationship. It must be remembered that, without practice, one is not likely to learn the motor control that will actually produce as small a difference as can be heard.

Singing and pitch discrimination: '09, R, .26, p.e., .04; also R, .37, p.e., .04; also R .29, p.e., .04; '11, r, .12, p.e., .05.

These composite records are not of much value for correlation, because different principles are involved in each of the component elements.

Auditory imagery and pitch discrimination: '11, r, .29, p.e., .04; '16, r, .29, p.e., .04.

Where there is a common element which relates pitch discrimination to other sensory and motor processes in singing, it probably comes most frequently through the channel of auditory imagery. The correlations here shown are good. One may question which of the two factors is the basic. Does auditory imagery depend upon pitch discrimination in so far as pitch is concerned, or does the opposite relationship obtain? Auditory imagery, which we ordinarily designate as ear-mindedness is, as it were, a general vehicle for other factors. Without entering into the dispute as to the rôle of images in successive comparisons of tones, we may assert that the image is present and probably favors discrimination. The correlation here shown seems to represent fairly the judgment of most observers

in regard to this relationship according to introspections. Those who have low auditory imagery tend to have poor and uncertain pitch discrimination.

Motor imagery and pitch discrimination: '11, r, .02, p.e., .05; '16, r, .07, p.e., .04.

From the fact that a keen sense of pitch would naturally develop motor responses and images of such responses to sound, one might have expected a larger correlation here. Its absence is conspicuous; it is undoubtedly due in part to the difficulty untrained observers find in identifying motor images.

Tonal memory and pitch discrimination: '16, r, .52, p.e., .03.

Memory is notably a faculty that responds to training. The accurate hearing of tones leads to the development of a natural memory for tones; conversely, indifferent hearing is even more certain to suppress attention to tones. Indirectly also, those who had formed the habit of hearing pitch were favored in the detection of the changed note which was often only a half-tone.

Musical training and pitch discrimination: '07, R, .31, p.e., .04; '09, R, .16, p.e., .04; '11, R, .21, p.e., .03.

a. Instrumental: '07, R, .27, p.e., .04; '09, R, .15, p.e., .05.

b. Vocal: '07, R, .20, p.e., .05; '09, R, .18, p.e., .04.

This item is perhaps the most significant. It raises two questions: Do those who have good sense of pitch seek musical training? And, does musical training improve pitch discrimination?

Observation of the many thousand cases we have, bearing on this point, leads us to the conclusion that those who have a good sense of pitch tend to like music, but only a comparatively small per cent. of these receive training. This fact in itself is perhaps adequate to account for the correlation here shown. The second question has been answered in the negative by Smith (16) and others, in so far as it refers to actual psycho-physic capacity.

It was thought that the relationship might be significantly different for instrumental and vocal music, the theory being that singing, like violin playing, offered special training in the precision of pitch hearing; but the figures do not warrant any such distinction. The explanation of this probably lies in the

fact that very little, if any, voice training is directed toward precision in pitch because the ear of both pupil and teacher is lenient and there is no objective aid employed as a check.

Musical environment and pitch discrimination: '07, R, .12. p.e., .05; '09, R, .17, p.e., .04; '11, R, .13, p.e., .03.

Environment is a part of education. It is also a mark of hereditary relationship. The relatively small correlation here shown for these two important factors supports the view that training in the hearing of pitch is a universal affair of normal hearing—like the perception of distance in vision. It reaches its normal development early in childhood through the ordinary use of sounds. If we may assume that the sense of pitch is an heritable character, that may account practically for the correlation here found.

Musical expression and pitch discrimination: '07, R, .38, p.e., .04; '09, R, .31, p.e., .04.

a. Instrumental: '07, R, .28, p.e., .04; '09, R, .22, p.e., .04.

b. Vocal: '07, R, .32, p.e., .04; '09, R, .22, p.e., .04.

These correlations involve essentially the same principles as in musical training. This concerns only the amount of musical activity. It is evident that the next point of interest should be a study of correlations with quality of achievement in musical expression.

The separation of vocal and instrumental expression seems to throw no light on the situation.

Musical enjoyment and pitch discrimination: '07, R, .27, p.e., .04; '09, R, .13, p.e., .05; '11, R, .06, p.e., .03.

One noteworthy peculiarity of special talent is that its absence is often not noted. One who has poor sense of pitch does not hear his own faulty pitch or the errors of others; he is satisfied and enjoys the many other aspects of music, apparently satisfied that music is to him what it is to others.

Average pitch discrimination in the practice series and the usual pitch discrimination, i.e., the first group test: R, '09, .64, p.e., .02.

Individual test of pitch discrimination by the method of con-

stant stimuli and the usual pitch discrimination of the first group test: R, '09, .68, p.e., .02.

Average of the pitch discrimination in the practice series and the individual test: R, '09, .68, p.e., .02.

In judging these relations it should be born in mind that a single group test cannot be counted on to establish the approximate physiological threshold in much more than half of the cases. Smith says: "Success in making a true measurement on an inexperienced observer in a single sitting varies with the knowledge, keenness, and care of the observer and the many objectively favorable or unfavorable conditions of the test as well as the experimenter; but, everything taken into account, it is safe to say that when an individual test is made under favorable conditions the approximate physiological threshold may be reached in a single sitting of less than an hour in more than half of the cases of adults or children who are bright and are old enough to understand the test. Even in group tests by the heterogeneous method one may reach in an hour the approximate physiological threshold of nearly half of the observers who are old enough and bright enough to observe." (16, p. 100).

It is, therefore, to be expected that the practice series and the individual test should both lead to readjustment. For this reason the correlation here shown is satisfactory as evidence of the stability of a single rough test. The fact that the correlation between the first group test and the individual test (presumably more accurate) is no greater than between the individual test and the average for the practice series, also speaks well for the first group test.

Intensity discrimination and consonance: '11, r, .03, p.e., .05; '16, r, .11, p.e., .04.

These two tests involve, to some extent, the same general power of critical attention to sound. It is difficult to explain the small correlation in '11.

Hearing ability and consonance: '16, r, .01, p.e., .04.

Time-sense and consonance: '16, r, '07, p.e., .05.

In these two cases it is rather remarkable that no significant correlation comes out here on account of the common element

of musical appreciation. The absence of correlation speaks for the elemental character of the tests.

Motor ability and consonance: '16, r, .03, p.e., .04.

No relation.

Free rhythm and consonance: '11, .03, p.e., .05; '16, r, -.03, p.e., .04.

Regulated rhythm and consonance: '11, r, -.11, p.e., .05; '16, r, .01, p.e., .04.

Comment for these two here same as for time-sense.

Rhythmic judgment and consonance: '16, r, .30, p.e., .04.

Singing the keynote and consonance: '16, r, .30, p.e., .04.

Singing intervals and consonance: '16, r, .21, p.e., .04

In the last three items the element of selection undoubtedly enters because those who have good consonance are likely to develop rhythmic judgment and accuracy in singing. It is difficult to identify other factors.

Voluntary control of pitch and consonance: '16, r, .01, p.e., .04.

Singing and consonance: '11, r, .04, p.e., .05.

The element of selection does not enter because these tests involve activities which never enter into music in this form, although it is the best test of pitch control. Voluntary control rests primarily upon pitch discrimination and motor ability. The absence of correlation with general singing ability in '11 is due to the fact that this record is based partly upon voluntary control.

Auditory imagery and consonance: '11, r, .14, p.e., 05; '16, r, .06, p.e., .04.

It seems strange that there is no higher correlation between these two factors, because the appreciation of consonance seems to rest largely upon that sort of mastery of details in tones which is ordinarily thought of as associated with the vivid representation of sounds.

Motor imagery and consonace: '11, r, -.02, p.e., .05; '16, r, .01, p.e., .04.

No correlation.

Tonal memory and consonance: '16, r, .23, p.e., .04.

This undoubted correlation is probably due in part to the

common element of familiarity with tones and interest in them.

Musical training and consonance: '11, R, .14, p.e., .03.

It is a comment on our processes of selection for musical training that there is so small a correlation between the sense of consonance and the opportunities for musical training.

Musical enjoyment and consonance: '11, R, -.02, p.e., .03.

No correlation; which indicates that the sense of consonance, like pitch discrimination, is an inborn capacity manifesting itself in, and developing through, the interest in, and dependence upon, the perception of qualitative differences in the world of sound which constitutes a part of the natural and ever present environment from early childhood, quite apart from its specialized form in music.

Musical enjoyment and consonance: '11, R, -.02, p.e., .03.

No correlation. This is a great puzzle. The evaluation of musical enjoyment is of course rather loose, while the measure of consonance is quantitatively quite accurate. It would be interesting to know to what extent this absence of correlation is due to inability of non-musical people to recognize the fact that they do not enjoy music as musical people do.

Hearing ability and intensity discrimination: '16, r, .16, p.e., 04.

Although these two activities seem similar they are not closely related, because hearing ability varies with the physical condition of the ear whereas, so far as we know, variation in the intensity discrimination for sound is largely due to psychological factors. It is however conceivable that some physical defects of the ear may be of such a nature as to affect both to the extent indicated by this correlation.

Time-sense and intensity discrimination: '16, r, .15, p.e., .04.

The common element here would seem to be the general capacity for sensory observation, if such there be.

Motor ability and intensity discrimination: '16, r, .01, p.e., .04.

Free rhythm and intensity discrimination: '11, r, -.05, p.e., .05; '16, r, .17, p.e., .04.

Regulated rhythm and intensity discrimination: '11, r, .18 p.e., .05; '16, r, .05, p.e., .04.

These three tests are alike in stressing precision.

Rhythmic judgment and intensity discrimination: '16, r, .00, p.e., .04.

Absence of correlation may be due to imperfection in the test of the judgment.

Singing the keynote and intensity discrimination: '16, r, .28, p.e., .04.

Singing intervals and intensity discrimination: '16, r, .27, p.e., .04.

Voluntary control and intensity discrimination: '16, r, .10, p.e., .04.

Singing and intensity discrimination: '11, r, .14, p.e., .05.

These are large correlations in the absence of dependence of one factor upon the other. The demand for auditory precision is the common element.

Auditory imagery and intensity discrimination: '11, r, .05, p.e., .05; '16, r, .04, p.e., .04.

Motor imagery and intensity discrimination: '11, r, -.05, p.e., .05; '16, r, .01, p.e., .04.

No correlation.

Tonal memory and intensity discrimination: '16, r, .26, p.e., .04.

From one point of view, intensity discrimination and memory span belong to the same type of process, differing only in the length of the retention.

Musical training and intensity discrimination: '11, R, .03, p.e., .03.

No correlation.

Musical environment and intensity discrimination: '11, R, .01, p.e., .03.

Musical enjoyment and intensity discrimination: '11, R, .01, p.e., .03.

Time sense and hearing ability: '16, r, .03, p.e., .04.

No significant correlation in these three cases.

Motor ability and hearing ability: '16, r, .24, p.e., .04.

This is a large correlation in view of the absence of specific common elements.

Free rhythm and hearing ability: '16, r, .01, p.e., .04.

Regulated rhythm and hearing ability: '16, r, .09, p.e., .04.

Rhythmic judgment and hearing ability: '16, r, -.06, p.e., .04.

No significant correlation in these two cases.

Singing keynote and hearing ability: '16, r, -.11, p.e., .04.

Singing intervals and hearing ability: '16, r, .32, p.e., .04.

Large correlation in the latter in view of the observed common elements.

Voluntary control and hearing abiltiy: '16, r, .00, p.e., .04.

Auditory imagery and hearing ability: '16, r, .03, p.e., .04.

No significant correlation in these two cases.

Motor imagery and hearing ability: '16, r, .22, p.e., .04.

No reason known for this correlation.

Tonal memory and hearing ability: '16, r, -.01, p.e., .04.

Motor ability and time-sense: '16, r, .01, p.e., .04.

No correlation.

Free rhythm and time-sense: '16, r, .08, p.e., .04.

Regulated rhythm and time-sense: '16, r, -.01, p.e., 04.

Rhythmic judgment and time-sense: '16, r, -.03, p.e., .04.

It is difficult to account for relative absence of correlation in these three cases.

Singing the keynote and time-sense: '16, r, .00, p.e., .04.

Singing intervals and time-sense: '16, r, .10, p.e., .04.

Voluntary control and time-sense: '16, r, .01, p.e., .04.

No noteworthy correlation.

Auditory imagery and time-sense: '16, r, .19, p.e., .04.

Motor imagery and time-sense: '16, r, .13, p.e., .04.

These correlations are significant because in time-sense both auditory imagery and motor imagery are conspicuous, according to introspections.

Tonal memory and time-sense: '16, r, .17, p.e., .03.

Tonal memory and time-sense are both memory tests.

Free rhythm and motor ability: '16, r, .09, p.e., .04.

Regulated rhythm and motor ability: '16, r, .12, p.e., .04.

The common element is general motor control.

Rhythmic judgment and motor ability: '16, r, .03, p.e., .04.

No correlation.

Singing the keynote and motor ability: '16, r, .10, p.e., .04.

Singing the keynote is, from one point of view, a motor control and it is safe to hazard the assumption that, to the extent that singing in key is at its maximum efficiency, the correlation between the motor ability and this capacity would increase. This point however remains to be established by further experiment; it represents a very important problem in vocational guidance as well as in pure psychology, or pedagogy.

Singing intervals and motor ability: '16, r, .21, p.e., .04.

The larger correlation here may indicate that the smaller correlation just noted for singing the keynote is exceptionally small. Theoretically the correlation for singing the keynote should be larger than this because it presents more of a problem of motor control.

Voluntary control and motor ability: '16, r, -.05, p.e., .04.

No correlation. Here again we are puzzled by the absence of correlation because precision in the control of the voice would seem to be related to motor ability in so far as it is independent of the hearing of pitch which is, of course, the largest determining factor.

Auditory imagery and motor ability: '16, r, .06, p.e., .04.

No marked correlation.

Motor imagery and motor ability: '16, r, .10, p.e., .04.

This correlation may be significant in that it may point to the existence of a general tendency which we may call motor-mindedness.

Tonal memory and motor ability: '16, r, .10, p.e., .04.

The explanation of this tendency towards correlation is uncertain.

Regulated rhythm and free rhythm: '11, r, .04, p.e., .05; '16, r, .14, p.e., .04.

It is difficult to account for the absence of correlation in '11, since these two factors involve several elements in common.

Rhythmic judgment and free rhythm: '16, r, .15, p.e., .03.

The relations are similar to those of regulated rhythm.

Singing the keynote and free rhythm: '16, r, .01, p.e., .04.

Singing intervals and free rhythm: '16, r, .07, p.e., .04.

Voluntary control and free rhythm: '16, r, .07, p.e., .04.

No significant correlation.

Singing and free rhythm: '11, r, .26, p.e., .05.

Correlation exceptionally large.

Auditory imagery and free rhythm: '11, r, .22, p.e., .05; '16, r, .01, p.e., .04.

It would seem probable that the correlation for '11 is more indicative of the true relation than the absence of correlation for '16.

Motor imagery and free rhythm: '11, r, .06, p.e., .05; '16, r, .07, p.e., .04.

Absence of correlation here seems strange.

Tonal memory and free rhythm: '16, r, .09, p.e., .04.

Musical environment and free rhythm: '11, R, .09, p.e., .03.

Correlation comparatively small.

Rhythmic judgment and regulated rhythm: '16, r, .00, p.e., .00.

Further refinements of these tests must be made.

Singing the keynote and regulated rhythm: '16, r, .13, p.e., .04.

Singing intervals and regulated rhythm: '16, r, .23, p.e., .04.

Voluntary control and regulated rhythm: '16, r, .11, p.e., .04.

Singing and regulated rhythm: '11, r, .07, p.e., .05.

The common element in these cases seems to be essentially the attitude of precision in action.

Auditory imagery and regulated rhythm: '11, r, .17, p.e., .05; '16, r, .00, p.e., .04.

Motor imagery and regulated rhythm: '11, r, .11, p.e., .05; '16, r, .08, p.e., .04.

The correlation for '11 in auditory imagery would seem to be a truer representative than the absence of correlation in '16; yet the low correlation in both auditory and motor imagery has great significance in the interpretation of the rôle of imagery in action.

Tonal memory and regulated rhythm: '16, r, .05, p.e., .04.

No significant correlation. From one point of view regulated rhythm involves memory; from another, it may be viewed as limited to the sensory span as distinguished from memory span.

Singing and regulated rhythm: '11, r, .07, p.e., .05.

Musical training and regulated rhythm: '11, R, .03, p.e., .03.

Musical environment and regulated rhythm: '11, R, .07, p.e., .03.

Musical enjoyment and regulated rhythm: '11, R, -.01, p.e., .03. The absence of correlation in the last three cases raises a question about the significance of mechanically accurate appreciation of rhythm for musical enjoyment.

Singing the keynote and rhythmic judgment: '16, r, -.04, p.e., .04.

Singing intervals and rhythmic judgment: '16, r, -.05, p.e., .04.

Voluntary control and rhythmic judgment: '16, r, .08, p.e., .04. No significant correlation.

Auditory imagery and rhythmic judgment: '16, r, .08, p.e., .04.

Motor imagery and rhythmic judgment: '16, r, .07, p.e., .04. These low correlations would be reflections upon the efficacy of imagery as an aid to precision if we could regard the test of rhythmic judgment as reliable.

Tonal memory and rhythmic judgment: '16, r, .22, p.e., .04. Memory is a basic factor in rhythmic judgment.

Singing intervals and singing the keynote: '09, R, .45, p.e., .04; '16, r, .61, p.e., .03.

Singing the scale and singing the keynote: '09, R, .42, p.e., .04.

Voluntary control and singing the keynote: '16, r, .55, p.e., .03.

Singing and singing the keynote: '09, r, .76, p.e., .02. Singing the interval, singing the scale, and voluntary control involve the same factors as singing the key, plus the interval concept and memory for this concept. This correlation shows that, as a rule, those who can sing the key at all accurately tend to sing the interval accurately.

Auditory imagery and singing the keynote: '16, r, .24, p.e., .04.

Motor imagery and singing the keynote: '16, r, .19, p.e., .04. These fairly good correlations would seem to indicate that those who are ear-minded or motor-minded have an advantage in that they sing in key.

Tonal memory and singing the keynote: '16, r, .32, p.e., .04. The general sense of pitch is a common element.

Musical training and singing the keynote: '09, R, .14, p.e., .05.

a. *Instrumental:* '09, R, .11, p.e., .05.

b. *Vocal:* '09, R, .26, p.e., .04.

This relatively small correlation in the first two items is indicative of the fact that the immediate reproduction of a tone is perhaps one of the most elemental forms of singing and speaking and therefore responds the least to training. The difference between the correlation in instrumental training and vocal training cannot be laid entirely to the value of training in voice. It is perhaps more due to selection; those who have a naturally good voice control are the ones who train.

Musical environment and singing the keynote: '09, R, .24, p.e., .04.

Training, heredity, and selection are perhaps responsible for the correlation.

Musical expression and singing the keynote: '09, R, .23, p.e., .04.

a. *Instrumental:* '09, R, .15, p.e., .05.

b. *Vocal:* '09, R, .24, p.e., .04.

Explanation same as for instrumental training and vocal training.

Musical enjoyment and singing the keynote: '09, R, .04, p.e., .05.

Absence of significant correlation here is indicative of the fact that few persons are able to detect their error in singing or in the singing of others, and enjoy music as it comes, not knowing what fine distinctions they fail to make.

Singing the scale and singing intervals: '09, R, .47, p.e., .03.

Practically the same factors are involved.

Voluntary control and singing intervals: '16, r, .32, p.e., .04.

The problems in these two factors are more unlike than it would at first seem. Interval depends upon memory concept whereas the least producible difference depends more largely upon pitch discrimination and motor control.

Singing and singing intervals: '09, R, .75, p.e., .02.

This large correlation is due to the fact that singing intervals was one of the measurements of which singing is the average.

Auditory imagery and singing intervals: '16, r, .40, p.e., .04.

This large correlation is very significant. Most observers upon close introspection find that the singing of intervals is not a blind leap but the auditory image is actually present in concrete form before singing.

Motor imagery and singing intervals: '16, r, .21, p.e., .04.

The ratio of this correlation to the preceding one represents approximately the rôle of motor and auditory imagery in the singing of intervals.

Tonal memory and singing intervals: '16, r, .30, p.e., .04.

Tonal memory was measured in terms of memory of intervals.

Musical training and singing intervals: '09, R, .23, p.e., .04.

a. *Instrumental:* '09, R, .39, p.e., .04.

b. *Vocal:* '09, R, .15, p.e., .05.

While vocal training furnishes good exercise in the learning of the interval, the interval is probably learned far more through listening to music than through the voluntary adjustment of intervals as in singing. In other words, we get our intervals largely from the piano and other common instruments in which they are most frequently heard.

Musical environment and singing intervals: '09, R, .18, p.e., .05.

Correlation may be due to training, heredity, and selection.

Musical expression and singing intervals: '09, R, .24, p.e., .04.

a. *Instrumental:* '09, R, .21, p.e., .04.

b. *Vocal:* '09, R, .28, p.e., .04.

Explanation same as for musical training. Vocal expression is of course essentially the singing of intervals. There is, therefore, both training and selection whereas in piano playing, *e.g.*, it is chiefly selection.

Musical enjoyment and singing intervals: '09, R, .05, p.e., .05. No correlation.

Singing and singing the scale: '09, R, .78, p.e., .01.

Musical training and singing the scale: '09, R, .18, p.e., .05.

Musical environment and singing the scale: '09, R, .16, p.e., .05.

Musical expression and singing the scale: '09, R, .31, p.e., .04.

Musical enjoyment and singing the scale: '09, R, .06, p.e., .05.

In singing the scale, the same principle operates as in the singing of the intervals, above.

Auditory imagery and voluntary control: '16, r, .29, p.e., .04.

Introspection shows very clearly that the auditory image is used in this test, often with profound consciousness, because there is a demand for imaging something which has not been heard.

Motor imagery and voluntary control: '16, r, .08, p.e., .04.

It is surprising that this correlation is not larger because introspection shows the motor image to be very conspicuous.

Tonal memory and voluntary control: '16, r, .50, p.e., .03.

This accords with introspections.

Auditory imagery and singing: '11, r. .07, p.e., .05.

Motor imagery and singing: '11, r, -.07, p.e., .05.

These absences of correlation suggest the need of more specialized inquiry into the actual efficiency value of images.

Musical training and singing: '09, R, .18, p.e., .05; '11, R, .09, p.e., .03.

None of the persons examined had enjoyed continuous professional training in singing; the fact that one had had a few more lessons than another is insignificant in comparison with the large differences in native capacity.

Musical environment and singing: '09, R, .22, p.e., .04; '11, R, .05, p.e., .03.

Musical expression and singing: '09, R, .32, p.e., .04.

Musical enjoyment and singing: '09, R, .01, p.e., .05; '11, R, .06, p.e., .03.

Compare these with correlation in singing intervals and voluntary control.

Motor imagery and auditory imagery: '11, r, .52, p.e., .04, '16, r, .42, p.e., .04.

All evidence points strongly toward a good correlation between auditory and motor imagery, much stronger, for example than between motor and visual.

Tonal memory and auditory imagery: '16, r, .40, p.e., .04.

Naturally a good correlation. Tests of memory depend upon

the ability to hold the group of tones in a single memory image for comparison.

Musical training and auditory imagery: '11, R, .21, p.e., .03.

Musical environment and auditory imagery: '11, R, .13, p.e., .03.

Musical enjoyment and auditory imagery: '11, R, .11, p.e., .03.

Auditory imagery responds to auditory environment of which musical training is a special form.

Tonal memory and motor imagery: '16, r, .16, p.e., .04.

Many observers remember in terms of motor processes to an extent which would lead us to expect a larger correlation.

Musical training and motor imagery: '11, R, .04, p.e., .03.

Musical environment and motor imagery: '11, R, -.03, p.e., .03.

Musical enjoyment and motor imagery: '11, R, .11, p.e., .03.

The correlation in the last item is small in view of the fact that the enjoyment of music is interpreted to a considerable extent in motor terms.

Musical environment and musical training: '07, R, .26, p.e., .05; '09, R, .30, p.e., .04; '11, R, .28, p.e., .03.

There is a tendency for those who live in a musical environment to take musical training.

Musical expression and musical training: '07, R, .51, p.e., .03; '09, R, .48, p.e., .03.

While the correlation is good, it shows that musical expression is in large part independent of musical training.

Musical enjoyment and musical training: '07, R, .22, p.e., .05; '09, R, .03, p.e., .05; '11, R, .10, p.e., .03.

If these two factors are regarded as standing in the relation of cause and effect, this correlation is low.

Musical enjoyment and musical environment: '07, R, .03, p.e., .05; '09, R, .15, p.e., .05; '11, R, .13, p.e., .03.

Musical enjoyment and musical expression: '07, R, .12, p.e., .05; '09, R, .16, p.e., .04.

Musical enjoyment and instrumental expression: '07, R, .13, p.e., .05; '09, R, .20, p.e., .04.

Does musical education make a person hypercritical? Does recognition of failure in training tend to lower the estimate of musical enjoyment?

SOME GENERAL CONSIDERATIONS AND CONCLUSIONS

Certain general impressions have been gathered in the many years of work in this field; some of those that have direct bearing on the present situation should be mentioned.

Evolution in testing is a process of simplifying. In the test of consonance, *e.g.,* the first test was made with tuning forks and resonators under very complicated conditions of control. An elaborate system of weighting was devised for various degrees of consonance. All possible intervals within an octave were employed. The test was necessarily long. The definition of consonance was complicated. All these and some other requirements have given away to simple conditions. The test is now made with the piano. Only eleven intervals are selected and employed. The judgments are not weighted. The definition and the conception of consonance are simplified. Each of these steps in the direction of simplifying, except the resorting to the use of the piano, is a step in the direction of increased efficiency. This tendency is characteristic throughout our experience; improvement takes place through progressive simplifying of technique, delineation of problem, and statement of results.

The evolution of a test is a process of isolating and identifying a specific process. This is illustrated in the case of rhythm. Our so-called free rhythm and regulated rhythm are very unsatisfactory measures of rhythm as it appears in music and speech. Rhythm like memory falls into a number of distinct and separable factors. When we get each of these isolated and under control we shall perhaps not have any single test on rhythm as such, certainly not any to designate rhythm as a whole, for rhythm must be split up into its cognitive, affective, and motor components. Thus discrimination for time and intensity are basic. Temporal and intensive precision in action correspond to these on the motor side; but, in addition, mental content, tonal intellect, uninhibited impulse, temperament, subjective rhythm, and musical feeling in various aspects must be taken

into account. Thus, as the testing of rhythm progresses, we shall tease out, from that bewildering tracery of mental pattern which we call rhythm, the component fibres; and, by testing each of these in isolation, we shall be able to learn a number of things in particular about rhythm, just as we are now coming into a position to say a number of things in particular about a larger conception of musical talent as a whole.

Evolution in testing is a progressive mastery of the means of commanding attention and effort under the most favorable conditions. The charge and a personal command of attention and effort in the person tested are most important factors in the test. The conditions must be so set as to take advantage of known principles of attention and effort in the grasping of the situation and responding to it and the charge, whether written or oral, must be not only emphatic but a matter of record as an element in the result; and unfortunately only persons with a certain gift of proficiency in handling others, trained to the intricacies of sources of error, and alert to incidental evidences which may appear, can be trusted to apply mental tests effectively. There is great danger of scrutinizing the physical and methodological aspects of the test and overlooking the important rôle of the personal charge which is necessary to bring into play the mental factor which is to be tested.

Evolution in testing is a progressive freeing from ambiguities. The children or adults who come in to be tested have few of the concepts of the experimenter. Learning how to put one's self in the place of the one to be tested is a power but slowly acquired. The blind complacency on this point of many of those who engage in mental testing is amazing. Progress comes through the slow education of the experimenter in this wonderful art of divining the point of view of the person to be tested, and technically stripping apparatus, method, and directions of sources of confusion. The instructions are not satisfactory merely because couched in words of a common language.

Evolution in testing is in the direction of making the test elemental whenever we are dealing with an innate capacity as distinguished from an acquired ability. This is characterized

by the fact that to the extent that the test is elemental there should be no appreciable improvement with practice, development with age, or variation in general intelligence. Some tests may be made completely elemental. In others this characteristic can only be regarded as a goal which we may strive to approach. Of course the three factors named do not necessarily vary together. A test may be elemental in one respect and, in the others, only approach this in varying degrees. Since we cannot make all of the tests elemental for all three factors, it is important to know for any particular test to what extent the limit indicated by the record is merely cognitive in the sense defined in an earlier report (15), that is, does not represent physiological limit but is inferior on account of ignorance, lack of appreciation, unfitness of the test, and many other factors. Each of the tests here reported has been, as a rule, one hour in length. In this time it is possible to get permanently satisfactory records on the majority in a group, but there will always be a considerable per cent. who cannot be reached in that time. This is of course a grave source of error and is probably the chief source of discrepancy in correlation.

In view of the above considerations we are cautious in attributing significance to the correlations here found. In the period covered the tests have all been in a process of evolution. The correlations are themselves in many instances a criterion of the degree of efficiency in the test. In other cases the absence of correlation is a mark of accuracy in the tests. At best these findings should be regarded as a preliminary survey from which we may take our departure in more intensive critique of each test and its ramifications. We have found this preliminary survey exceedingly suggestive for guidance into further work and it is hoped that it may serve others in this field and in related fields in the same way.

With such reservations and apologies we may now attempt to gather some of the more salient features of interpretation for each test partly on the basis of the quantitative showing of the correlation but in general as a statement of impressions gathered in the experience with the tests. It should not be

regarded as a full summary of what has been stated in detail.

Pitch discrimination—The test of this capacity may be regarded as elemental, that is, the physiological threshold (15) is independent of variation with practice, age, sex, or general intelligence. It must, however, be remembered that in a first rough test, as here used, the approximate physiological threshold is reached only in somewhat more than half of the cases.

This capacity is basic for all perceptions of pitch and for production of pitch in singing and playing, *e.g.*, the violin; also for consonance, auditory imagery, and to some extent for memory. The failure of close correlation with these in the present tests may be attributed to numerous factors among which are the following: the related test does not involve the least perceptible difference in pitch, *e.g.*, singing or playing in pitch when the error is due largely to lack of motor control, or consonance and tonal memory in which keen pitch perception is not essential for a moderate performance; the related test may not be effectively differentiated, *e.g.*, singing an interval in which the chief source of error may lie in an erroneous conception of the interval; in cases of poor records the threshold may be merely a cognitive one and the actual capacity may be used in the related test, *e.g.*, in singing the keynote.

For hearing ability and discrimination for intensity, the common element may lie in both the structural or pathological conditions of the ear; but in time-sense, the various forms of rhythm, and related tests it may often be traced to common central conditions or inadequacy of one of the tests.

The data in the questionnaire are of special interest on these points: musical training furnishes relatively little training in precision of pitch perception; musical environment makes but little difference except in so far as it is selective, because the experience of pitch differences is called into play far more frequently outside of music than in music; ability to perform and enjoy music in a moderate degree is not seriously limited by defective pitch discrimination, except in a comparatively small per cent. of cases; the report of enjoyment of music cannot have taken into account what the reporter has missed by

not hearing, and may therefore represent a wrong conception of real enjoyment, jut as the color blind person has a wrong notion of what a color really is.

The measurement of pitch discrimination is undoubtedly facilitated by the presence of good auditory imagery, especially in a single group test. General intelligence also favors the securing of a good record. But there is no evidence to show that it correlates with actual psycho-physic capacity in pitch discrimination. In other words, lack of ear-mindedness and lack of general intelligence tend to produce unreliable record but they do not seem to effect actual psycho-physic capacity as functioning in familiar perceptions of sound. On the problem of heredity we have no satisfactory data.

Consonance.—Consonance is the most direct test we have on the ability to hear tone-quality. A test is now being developed for the measurement of the sense of timbre by measuring the ability to detect variations in the overtones. That ability is however closely related to the ability to judge the quality of two-clangs as in consonance, which is now the most general test of sensory capacity for musical intellect.

In view of this basic position of consonance as a test of the "ability to hear music" correlation with the returns from the questionnaire are of special interest. There is no evidence to show that those who have a natural sense of consonance are selected for musical training or that musical training and musical environment enhance this capacity; nor does a person's actual ability to hear tone-qualities as here measured seem to enter into his evaluation of his enjoyment of music. Our first move in the face of these facts was to question the validity and meaning of the consonance test. We are convinced of its validity both as to the isolation of factors concerned and fitness of method. The burden of squaring musical procedure with these facts is therefore passed on to the musical profession.

One of the most gratifying findings about this test is the surprising fact that it is elemental in several respects. This capacity is early present in bright children who have a musical ear. It is fairly independent of the vagaries of feeling, and

measures a well defined type of sensory judgment which is developed by the every-day reactions to sound. It is, however, in some respects an intelligence test as no one can make a good record without the ability to sustain attention purposefully in a difficult act of comparison. Lack of ear-mindedness is also a serious deterrent in this test.

Physiologically consonance presupposes a certain degree of pitch discrimination. It involves the same sensory attitude as intensity discrimination though far more sustained. It is undoubtedly selective with reference to the various measures of rhythm in that the sense of rhythm, which is of motor origin, is naturally developed by those who have a keen ear for tone quality. Between time-sense and pitch of singing there is no close relationship; but, for quality of singing and playing we shall of course look for a strong correlation in future tests since the expression of five points of quality rests upon the hearing of them.

Intensity discrimination.—In normal hearing this is a clear-cut test of the intellectual capacity for accuracy in the observation of sound. It is the simplest test in the entire series and is elemental as to effect of training (16), sex, and age. It varies however with intelligence, of which it is a measure in one aspect. In this respect it is a test of the power of concentration of attention in its simplest form. This capacity may, therefore, be regarded as an index to general ability and reliability in sensory observation of sound. This measure therefore has two phases of definite significance in the evaluation of musical talent: it indicates intensity discrimination and it is an index to general ability and reliability in hearing of sound. The latter point is well sustained by the significant correlation our figures show for so many of the factors here measured in which there is no other basis of relationship than that of precision in the observation of sound.

Musically the capacity must be distinguished from a lively and imaginative play of intensity in the appreciation of music. Artistic musical hearing, like painting, rests ultimately, not on rigid sensory impressions, but upon a fertile imagination which

takes the sensory impression as a cue. One who hears musically may be fantastic and fastidious in the intensity differences he attributes to sound and yet be inferior in the actual observation of such differences when they really exist; but, when it comes to singing or playing, this artistic license ceases and the effective performer is limited by his capacity for precision in the hearing of the shades of stress which he aims to convey.

Hearing ability.—This involves two variables, the relative sensitiveness of the physical ear as an organ and, as in intensity discrimination, the general power of attention to auditory perceptions. For the latter it is not so valuable as intensity discrimination because that is only one of the two variables and the other variable is the more important. Indeed the record of intensity discrimination should always be taken into account in the interpretation of hearing-ability as only one who has a good record in intensity discrimination can be counted on to reveal the actual physiological threshold in hearing-ability. The absence of correlation with so many of the tests here shown is therefore a mark of reliability in this test.

Time-sense.—There are three basic sensory aspects of music: pitch, intensity, and time. All other sensory aspects of music, such as timbre, consonance, and rhythm are complexes, in which one or more of these basic factors are elements. Time-sense as here used is the basic measure for the various temporal processes in the perception and expression of music. It complies with the requirements of a basic test in that it is simple and direct and is the most rigidly controllable measure available. Failure to correlate with the three so-called rhythm tests may therefore be regarded as a reflection upon the reliability of these, because time-sense is undoubtedly a basic capacity for all of them. The positive correlation with pitch discriminaion and intensity discrimination is evidence of the presence of that common element which intensity discrimnation measures.

Motor ability.—Motor capacity in music as well as in any other form of diversified action may be analyzed into five fundamental phases, namely, motor ability, precision in time, intensity and direction of movement, simple and complex reaction time, strength and endurance.

The conventional measurement of motor ability by means of the tapping with a key has a specious appearance of accuracy. On account of its basic position in motor control it is extremely desirable that this test should be analyzed and standardized more accurately before being used extensively in diagnosis. The test should be so divided into parts as to measure both speed and regularity. As here used the test involved only speed. But speed and regularity are the common elements which are involved in all tests of motor control. If satisfactorily isolated in this test, they may be regarded as furnishing a two-phased measure of general ability in motor control. Together they are a mark of the general tone or condition of the motor organism. Singing and playing in pitch, reaction-time, rhythmic action, and endurance, each involves these two factors.

The greatest problem in the development of the evaluation of this test now is to determine to what extent general motor capacity as thus measured can be interpreted as an index to the motor control of specific organs, such as the finger, the lips, or the vocal cords.

One of the greatest obstacles in the way of such an evaluation is the fact that we do not know the relation between voluntary use of a capacity and its automatic use. Our tests are all more or less voluntary, conscious reactions, whereas in artistic performances and, indeed, in all skill, both sensory and motor processes are more or less automatic.

Free rhythm.—The term rhythm as used in this and the next test should be discarded, and we should speak of time,—the time of free action, the time of regulated action, in order to recognize, first, the fact that on the motor side these are time tests, parallel to time-sense on the sensory side; second, to dispel the notion that these are primary tests of rhythm, which they are not.

This test is psychologically simple and can be made relatively elemental. It is easier to control than motor ability, yet we are not entirely satisfied with the test as here reported. Both the '11 and the '16 free rhythm and regulated rhythm tests were made by students, comparatively inexperienced in testing, and it

was observed that they often failed in the very important matter of charge and command of mental attitude in the test. This situation emphasizes the fact that we may have perfected technique of physical method, and yet fail for want of personal command of the mental attitude in the observer. The notion that mental tests can be made so automatic as to eliminate the personal equation of the experimenter is absurd. This directs attention to the most urgent need in mental testing today—the need of not only formulating a charge, but of administering the test by a person who has strength of personality to make it effective, and technical insight enough to guard against unforeseen sources of distraction and variation in effort.

Regulated rhythm.—Psychologically, regulated rhythm is more complex than free rhythm and, therefore, is not so valuable. The failure to correlate with free rhythm and rhythmic judgment reflects unfavorably upon this test, because free rhythm represents the ability to play in correct time alone, and regulated rhythm the ability to play synchronously with another whose time is perfect, and rhythmic judgment is merely free rhythm one stage more complicated. These three tests, therefore, call for thorough revision.

We must be extremely cautious of the interpretation of the so-called rhythm tests, and not regard them as tests of rhythm in the sense of motor tendencies in the grouping of sounds by time and stress. The strongest kind of rhythmic person may be very inaccurate both in time and stress so far as the perception and feeling of rhythm is concerned, but when he attempts to convey his feeling to others by means of time he is limited by his power of precision in action which this test should measure.

Rhythmic judgment.—This test must be improved or rejected.

Singing the keynote.—This test is basic for all pitch singing. It is simple and precise, and, therefore, furnishes good opportunity for the accurate determination of many of the psychophysical principles involved in singing.

Most children who have a good ear develop this command

very early so that the first imitations of musical pitch may be true, and may appear to be as automatic as if they had been inherited as such. True pitch comes to a child as early and as naturally as a vowel quality in speech, where the child has a natural ear for music and has sensitive motor control. There are, however, many and extraordinary exceptions to this rule. Occasionally a very bright person apparently cannot tell "tune" and much less sing in tune at all, and yet be educable in the singing of pitch. Such cases seem to be due to a sort of tone ignorance analagous to that obliviousness to odors which many of us display in the presence of good normal sensitiveness to smell.

This test correlates well with the other tests of singing, and may, therefore, be regarded as a general index to voice control of pitch. At its best, it rests upon pitch discrimination. It seems to be favored by auditory and motor imagery, and by tonal memory. The correlation with vocal training, musical environment, and vocal expression is, perhaps, in large part a matter of selection.

All the singing tests can of course be paralleled on an instrument such as the violin.

Singing intervals and singing the scale.—The test of singing intervals may, of course, be a test of any kind of interval, such as a natural scale, chromatic scale, melody, or any one or more isolated intervals, conventional or otherwise. It is a natural, accurate, and very satisfactory test. If it is to be used as a general measure of the control of the interval concept, the singing of the natural scale is perhaps the most satisfactory because the most familiar. The singing of intervals is highly favored by auditory imagery and tonal memory, also, to some extent, by motor imagery. It correlates well with musical training, musical environment, and musical expression. The latter two are probably both selective and effective in their relation to the singing of intervals. No relationship with musical enjoyment is revealed.

Voluntary control of pitch in singing.—This is the complement

on the motor side to pitch discrimination on the sensory. This type of discriminative action gives promise of developing into a rôle possibly as important on the motor side as discrimination is on the sensory. This minimal sharping or flatting in singing can be paralleled in playing with any instrument of adjustable pitch. It answers the demand for a test that shall measure specific capacity in terms quite free from ordinary habit, and equally new to all. In this respect it differs radically from the singing of intervals or the keynote. Methodologically, it presents certain difficulties in securing a single measure in that there is no fixed standard as to the certainty of the change, but it seems that this difficulty can be overcome. This action involves primarily pitch discrimination and a form of motor control; it is favored by auditory imagery, motor imagery, and tonal memory, and it has many common elements with other singing tests.

Auditory imagery.—This is the only capacity in our series which does not lend itself readily to objective measurement. The introspective grading is, however, favored by the advantage of having fixed limits for the series of grades, namely, no image for the lower limit, and as vivid as true perception for the upper.

There are good unworked possibilities for the study of the nature of the true character of the musical make-up of an individual by an intensive individual study of the content, vividness, completeness, persistence, and spontaneity of the auditory imagery, particularly with the aid of some of the more recent forms of association tests.

It still remains to find out what rôle the auditory image really plays in the life of a musician, or in what respect it is essential to the highest appreciation of music. Unquestionably it holds a vital position in music, but the laws of its functioning and non-functioning are exceedingly complex.

In addition to the correlations already noted in preceding sections, we note here a conspicuous correlation with motor imagery and tonal memory. Records not included here show that motor imagery correlates far more closely with auditory imagery than with visual imagery.

Tonal memory.—This test is basic for the various phases of memory. It is clear-cut, rigid, simple, and adequate. If we shall have a single test for musical memory, this one answers the purpose, but a single test for such a complex act as memory must be regarded as a makeshift, since the various elements of memory must be isolated and tested separately, especially if intensive work is to be done.

Musical training.—In considering the returns of the questionnaire we have been cautious throughout to limit the applications to the degree of certainty implied in the character of the information itself. In correlating these facts with concrete measures from the objective tests it is equally important to limit the conclusions guardedly to the factor of talent actually controlled and measured, and to safeguard the conclusions in proportion to the accuracy of the analysis made by the test. Such precaution is necessary, not only for the writer, but it must also be borne in mind by the reader in his interpretation of statements like the following.

As far as the results of the questionnaire and supplementary evidences are reliable, we face a discouraging situation in the relation of the possession of musical talent and the opportunity for musical training. Briefly, we find that there is no satisfactory tendency to select the musically talented for musical training; that musical training does not ordinarily modify musical capacity; and that specific ability which might be developed by special training is not ordinarily enhanced by the training in vogue.

Musical environment.—The most impressive feature of the evidence before us on this point is the fact that the fundamental powers in musical talent such as pitch discrimination, intensity discrimination, time sense, imagery, and to some extent consonance, singing, and tonal memory, are developed quite apart from association with music. Musical training and environment seem to be a specialized form of the use of these capacities which are continuously in action in the appreciation of, and response to, an environment of sounds.

Musical expression.—It cannot be said from our evidence that

it is the musically talented who play and sing for us, although there is some correlation in that direction.

Musical enjoyment.—The real absence of correlation between musical enjoyment, and musical talent and musical training throws new light on the validity of prevailing opinion in regard to the reality and quality of our musical enjoyment. It is conceivable that a person who could hear and feel nothing but the rhythm of a drum beat might get real enjoyment from that and rest undisturbed in the notion that he had enjoyed the full riches of music, because, like the color blind, his system seems complete and he knows not what lies beyond.

In the light of the facts we have reviewed, we may ask the same question for musical theory in general that is proposed in regard to rhythm in the latest book on that subject (8): "Is it any wonder that the student who dips, or, more boldly, dives into the inevitable chapter on rhythm to be found in current musical hand books, rhetorics, treatises about versification, etc., emerges mystified, when so little account is taken of individual differences in what is one of the most 'individually different' of human experiences?"

It is difficult to convey in writing the illuminating insight into, and concrete foundations for musical theory, art, and pedagogy that one obtains in the prolonged use of these tests. But the general fact can be made clear that the psychology of music must be founded and organized upon experimental analysis of musical talent, that the aesthetics of music must be built in a scientific spirit upon such experimental analysis of the operating psychological principles, and that the art of teaching and vocational guidance in music must be based upon established facts about the nature of talent, the nature of the learning processes, and the nature of the aesthetic principles involved.

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THE PERCEPTION OF CONSONANCE AND DISSONANCE

BY

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CONTENTS

The object of this investigation is first to establish the ranking order of the musical intervals within the octave c′c″ with respect to the degree of consonance, and second, to standardize a measurement of the perception of consonance.*

The term consonance has been variously defined, and has been used to convey several meanings. While in general, it

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has been used with reference to the agreement of simultaneous tones, we find each investigator or theorizer emphasizing some one factor in this complex phenomenon to the exclusion of one or two other factors of similar importance. This diversity of definition results in a corresponding diversity in the ranking order of the common musical intervals as to their degree of consonance. The method in this investigation will, therefore, be: first to determine the factors which enter into the perception of consonance, second, with these factors as a basis, to rank the intervals according to their relative degree of consonance or dissonance, third, to evaluate the ability to perceive consonance in terms of this ranking.

HISTORICAL

A brief resumé of the essential facts bearing most directly on this problem may be presented from the points of view of the theories and definitions of consonance, experimental methods, and ranking of intervals.

Definitions and experimental methods

Pythagoras discovered the regularity of the aliquot division of the vibrating string, and thereby gave a numerical value to the notes of the scale. Since then, the regularity of the vibration frequencies of tones has been known and accepted as a basis of consonance. No thought of the manner in which the mind perceives and distinguishes between consonances and dissonances occurred to the ancient Greeks; the perception was taken for granted. The pleasantness of some intervals and the unpleasantness of others were the only criteria that were present to their minds.

This conception prevailed even up to the time of *Leibnitz* (10) who was the first to call attention of scientists to the fact that the mind did not really analyze or perceive the actual number or the numerical regularity of the vibration frequencies in the intervals. Nevertheless, Leibnitz found no other explanation of this complex phenomenon in consciousness. Therefore, he appealed to the subconscious mind for a solution of the problem

and came to the conclusion that the mind unconsciously calculated the ratios of the vibration frequencies.

"Die Musik ist ein verborgenes Rechnen des Geistes, welcher nicht weiss, das es zählt. Denn er tut Vieles mit verwirrten oder unmerklichen Perzeptionen, was er in deutlicher Apperzeption nicht wahrnehmen kann. Die irren, welche meinen, es geschehe nichts in der Seele, dessen sie selbst nicht bewusst sei. Obwohl also die Seele nicht fühlt, dass sie zählt, fühlt sie doch das Ergebnis dieser unmerklichen Zählung, d. h. das aus ihr fliessende Vergnügen bei den Konsonanzen, Missvergnügen bei den Dissonanzen. Denn aus vielen unmerklichen Übereinstimmungen entsteht das Vergnügen."

This unconscious calculation produces a consonant interval when the ratio number does not exceed five. "Wir zählen in der Musik nicht über fünf", says Leibnitz.

Euler (2) agreeing essentially with Leibnitz' explanation, interpreted the feeling of agreeableness of the consonances as due to the ease of perceiving order or coherence in the simpler ratios. He divided the consonances into ten classes, ranking them according to the simplicity of their ratios. Euler was the first scientist to formulate the fundamental law of consonance that "the degree of consonance is in a direct ratio to the magnitude of the common divisor of the vibration frequencies."

Schopenhauer (24) explained the mental processes that accompany the perception of consonances and dissonances in greater detail and regarded music as the highest expression of the divine in the world. It owes its great power to the essential relation which it bears to the human will. Consonances are the result of a rational relation of the vibration frequencies of two notes that can be expressed by small numbers. Their constantly recurring coincidences can be apprehended more readily by consciousness than those whose coincidences are less frequent. The notes, which are the result of this relation, blend. Dissonance is just the reverse of this state, but both consonance and dissonance comprise different degrees, the one shading into the other.

"Music is a means of making rational and irrational relations of numbers comprehensible, not like arithmetic by the help of the concept, but by bringing them to a knowledge which is perfectly, directly and simultaneously sensible. Consonances and dissonances, with their innumerable degrees of difference, portray the movements of the human will in its essential feelings of satisfaction and dissatisfaction."

The distinguishing factor of consonance would accordingly be this feeling of satisfaction portrayed by objectifying the movements of the human will through harmony.

The method of investigation with regard to musical intervals was theoretical, supplemented by but meagre and indeterminate empirical data, up to the time of *Helmholtz* (4), who gave an impetus to experimental investigation through his analysis of sound in the attempt to demonstrate that consonance is dependent upon the coincidences of upper partial tones which result in a relative absence of beats. The important criterion of consonance for Helmholtz is *smoothness*. His definition reads as follows:

"Consonance is a continuous, dissonance, an intermittent sensation of tone. Two consonant tones flow on smoothly, side by side in an undisturbed stream; dissonant tones cut one another up into separate pulses of tone." Consonance is dependent upon "certain determinate ratios between pitch numbers which do not give rise to beats, or only such beats as possess so minimal an intensity as to produce no unpleasant disturbance of the united sound."

On this theory Helmholtz constructed his consonance curve, showing the relative degrees of consonance on the basis of the number of beats possessed by the different intervals as quoted in Table I below.

As early as 1751 *Tartini* (27) called attention to the importance of combination tones, but it was left for Preyer to prepare the way for Krüger's exposition on the influence of difference tones on consonance.

Preyer's (21) experiments with tones, whose overtones and combination tones were excluded, tend to establish the fact that

the feeling of pleasantness and unpleasantness of consonances and dissonances is dependent on the overtones and difference tones of the clang.

"The beats of the overtones and combination tones are," says Preyer, "a further criterion, for through them the smoothness of the sensation is destroyed. Yet they do not suffice to explain dissonances, as these occur without beats. The well recognized consonances must give the least combination tones, and the most unpleasant dissonances, the most; the former, the most coincidences, the latter, the least."

Stumpf is the pioneer in the purely psychological field, as he introduces a new point of departure. Stumpf (30) identifies consonance with tonal fusion:

"The sounding together of two tones approaches sometimes more, sometimes less, the impression of unity, and it is apparent that this is more the case, the more consonant the interval is. Even if we recognize the tones as two and separate from one another, yet they form a totality in the sensation, and this totality appears to us as possessing a greater or less degree of unity."

Thus Stumpf postulates "Verschmelzung" as the distinguishing criterion of the degree of consonance. He admits that other criteria exist, but this one factor is the only one necessary in ranking consonances and dissonances. Concerning this factor Stumpf writes as follows:

"Kann der Unterschied konsonanter und dissonanter Töne weder in unbewussten Funktionen noch in den Gefühlen liegen, so wird man ihn in den Tonempfindungen als solchen zu suchen haben, wo ihn denn auch Helmholtz suchte. Da er nun aber nicht in der begleitenden Obertönen und nicht in den Schwebungen liegen kann, so muss er eben in den beiden Tönen selbst liegen, welche wir konsonant oder dissonant nennen. Es ist, soviel ich sehe, nur ein Merkmal, das sich hier arbeitet: die *Verschmelzung* gleichzeitiger Töne."

As a correlate to his psychological theory, Stumpf proposes a physiological basis in his theory of "specific energy" which gives to each fusion its individual character.

Following out the theory that the better the consonance, the better is the fusion, Stumpf applied the technique of experimental psychology. That most consonant intervals are the most difficult to analyze into their constituent elements, is the basis of his method of analysis; and by this method he attempted to rank the intervals. He tested the perception of consonance with Appunn's Tonmesser and with the tones of the pipe organ, varying the quality of tone further by using the different stops of the organ, the observers being requested to record whether they perceived one or two tones, *i.e.*, whether or not the two objective tones served as one subjective. The number of errors for each two-clang constitute a measurement of the degree of consonance of that interval, and determined the ranking order.

Stumpf (29) found no difference between major thirds and sixths, yet he admits that there may be a fine degree of gradation between major and minor thirds and major and minor sixths. All the dissonances are ranked in one group as possessing the same degrees of dissonance, with the observation that the natural seventh, 4: 7 may be a slightly better fusion than the other dissonances.

Stumpf (27) lays down the following laws of tonal fusion:

1. Fusion depends on the so-called ratio of vibrations.

2. The degrees of fusion are independent of the tonal region within the tonal range.

3. The degree of fusion is independent of the intensity, whether indeed it be the absolute or relative intensity, so long as the tones remain distinguishable.

4. The degree of fusion is not influenced by the addition of a third or fourth tone.

5. Very minimal deviations of the number of vibrations from the ratio create no perceptible difference in the degree of fusion. If the deviation is increased, the fusion in all pairs of tones, except the lowest degrees, passes into this degree without running through the intermediate degrees, if any. The rapidity of this transition is proportionate to the degree of the initial fusion.

6. Fusion remains and retains its degree when both tones do not affect the same ear.

7. Fusion remains in the mere representation of the imagination.

8. If we proceed above the octave, the same degrees of fusion recur with the ratio of vibrations increased one or more octaves. m : n 2x as m : n if m is less than n and x equals a small whole number.

Faist (3), following out Stumpf's method literally, attempted to verify these laws of fusion, in experiments on the pipe organ with the use of its different stops. In a preliminary experiment he employed what he termed "the direct method", *i.e.,* "the method of serial rank, in distinction from the indirect method used by Stumpf. This method involves the ranking of the intervals directly, keeping the whole series in mind and giving them their relative rank. The result of Faist's experiments tended to verify all of the laws postulated by Stumpf, except the third. He found that the relative intensity of the components of the interval did influence the perception of fusion.

Both *Meyer* (14) and *Stumpf* (34) have also attempted to measure consonance by means of reaction time. The consonances were always distinguished from the dissonances but this method showed no consistency in the ranking of the other intervals, and it was rejected by Meyer as unreliable. Meyer also investigated the effect of variations in intensity, and found that the less consonant the intervals are, the greater is the difficulty of recognizing them in their minimal intensity, and that the relative loss of intensity of higher tones synchronous with lower tones has no perceptible influence within the octave. As to the effect of presenting one tone to each ear, for the purpose of excluding the difference tones, Meyer affirms that this exclusion results in a loss of ability to rank the more difficult intervals, although consonances are readily distinguished from dissonances.

Buch (1) criticizes Stumpf for neglecting to regulate the conditions of his experiments. He thinks it unlikely that experiments made with the organ of the "Domkirche in Halle" could be carefully enough regulated to be trustworthy. Accordingly,

Buch constructed a special instrument, using the organ, by means of which he regulated the intensity, duration, and pitch of the intervals. His experiments were made with, and without analysis. He makes a distinction between making a judgment from the point of view of analysis and that of synthesis. He developed a twofold ranking of the intervals, one on the basis of fusion, and the other on the basis of smoothness.

Lipps' (11) explanation of consonance is based on the rhythmic coincidences of the tonal series. The rhythmic coincidences have their correlate in the psychic processes. For our immediate consciousness, consonance appears as an agreement in unity, or, as Lipps expresses it, "eine Zusammengehörigkeit, eine einheitlichkeit". Furthermore, it is an agreement that gives rise to a feeling of satisfaction. "Consonanz ist ein Verhältnis zwischen Tönen in dessen Natur es liegt, Befriedigung zu erzeugen." The most perfect consonance does not give rise to the most satisfaction as it is "empty and monotonous."

Meinong and Witasek (8) introduced the method of "paired comparisons" in determining the ranking of the intervals, as played on the tones of the violin. They state their conclusion briefly as follows:

" . . . zwei Töne um so mehr verschmelzen, (a) je näher ihnen der Klang steht, auf den als Partialtöne bezogen werden können, (b) je grösserer Zahlenwert dem Verhältnis ihrer Schwingungzahlen zukommt."

Their results are interpreted in terms of the Ebbinghaus theory of hearing. Their experiments demonstrate that the method of paired comparisons may be used to good advantage in the testing of the perception of the degree of consonance and the ranking of the intervals. Meyer has questioned the reliability of their results because of the inaccuracy of the violin as to pitch.

Using the method of "paired comparisons" with the tones of Appunn's Tonmesser, tuned to the accuracy of 1 v.d., *Pear* (19) determined the ranking of the intervals in degree of consonance. The observers recorded the intervals compared as equal, plus,

minus, or doubtful in preference, and the ranking order was computed on the basis of the number of "votes" given each interval. He considered fusion, analyzability, pleasantness and unpleasantness, and association, the factors which might enter into the perception of consonance. These factors were explained and illustrated for the observer, and he was instructed to make his judgment on the basis of fusion."

Wundt's explanation (41) shows that consonance is dependent on the confluence of various factors. There are four criteria that are more or less essential conditions of this phenomenon, namely: (1) Purity, the number of primary difference-tones of different orders, which combine to give the consonant chords a distinct or individual character; (2) uniformity, the uniform relation of the intervals to the compass of the scale; (3) the discrimination of consonance by the recognition of the tonal elements, dependent on the direct and indirect relation of clangs; (4) the fusion of tones into a "clang unity" through the dominance of one of the tonal elements—the one which arouses the most intensive associations.

Wundt explains dissonance as a "diffuse tonal fusion." The diffuse nature of dissonance arises, on the one hand, from the physiological condition of tone absorption; on the other, psychologically, through the distinct differentiation of tones arising from the compounding of the interfering difference-tones. Consonance is, therefore, an act of the apperceptive faculty of mind, which synthesizes the tones into a unity. The attention concentrates on the tonal element that carries with it the strongest associations, and brings all the related phenomena to a focus with the same.

Krueger (8) adopts the first criterion of Wundt as the explanation of consonance and dissonance. A two-clang always possesses five difference-tones. The pitch of each is determined by the formula h-l, 2h-l, 3h-l, etc. These difference-tones are related to every other simultaneous tone in the clang exactly in the same manner as the simultaneous primary tones. Thus, coincidences, beats, intermediate tones result from these difference-tones, and influence our perception of consonance. The

best consonances are those that are characterized by the absence of distinct difference-tones. Dissonances are the result of difference-tones in the two-clang which interfere with one another. The degree of dissonance depends on the number of these interfering difference-tones.

Krueger (6) bases his theory on empirical data. His method of experimentation had for its object the determination of the number and pitch of the difference-tones present in the various intervals. In order to exclude the overtones, he used the tuning forks. The observer was requested to record the number of difference-tones in each interval, and to identify the pitch of each on a Tonmesser.

Stumpf (29) has criticized Krueger's results as not showing sufficient consistency to substantiate his theory. As another proof against the theory he states that consonance and dissonance are perceived even when the tones composing the two-clang are presented one to each ear, thereby excluding the possibility of difference-tones.

To sum up the historical review, we have gathered the data into a table by different authorities and methods, showing the order of consonances and dissonances:

This historical summary shows that the factors emphasized in the course of investigating this complex phenomenon have been: the feeling of satisfaction, agreement of tones, smoothness, fusion, and purity, with slight variants of these. We recognize the fact throughout that the order of the intervals tends to correspond to the simplicity of the ratios, expressing this mathematical relationship. That the feeling of satisfaction, or the feeling of pleasantness, is too variable and general a factor to be used as a constant criterion, is evidenced by the fact that it has not entered as a determining factor in any of the experimental investigations.

The most fundamental factor in ranking consonance and dissonance may be termed *blending,* the tendency of tones to merge into a composite tone that shows a more or less distinct agreement of constituent parts in so far as they are perceived

TABLE I—*Historical order of consonances and dissonances*

			No. Intervals............		1:2	2:3	3:4	4:5	3:5	5:6	5:8	5:7	9:5	9:8	15:8	15:16
			Math. ratios*............		c′ c″	c′ g′	c′ f′	c′ e′	c′ a′	c′ e′ᵇ	c′ a′ᵇ	c′ g′ᵇ	c′ b′ᵇ	c′ d′	c′ b′	c′ d′ᵇ
Authors	*Date*	*Instrument*	*Method*	*Criteria*												
Franco of Cologne	12th cent.		Theoretical	Pleasantness	1	2	2	3	4	3	4	5	5	5	5	5
Euler	1739		Mathematical	Pleasantness	1	2	3	4	5	6	7	8	9	10	11	12
Helmholtz	1863	Various	Coincidences of upper partials Beats	Smoothness	1	2	3	5	4	6	7	8	9	8	10	11
Stumpf	1883	Pipe organ (Different stops)	Analysis	Fusion	1	2	3	5	4	6	7	8	9	8	10	11
Faist	1897	Pipe organ (Different stops)	1) Direct 2) Analysis	Fusion Fusion	1	2	3	4	4	4	4	5	5	5	5	5
Meinong and Witasek	1897	Violin	Paired comparisons	Fusion	1	2	3	5	6	9	4	7	8	10		
Lipps	1899		Theoretical	Rhythmic coincidences	1	2	3	4	5	6	7	8	9	10	11	12
Buch	1900	Pipe organ Tonmesser	Analysis	Fusion Smoothness	1	2	3	3	3	4	4	4		5	6	
Krueger	1903	Tuning forks	Analysis	Purity	1	2	3	4	3	6	5	5	4	7		
Pear	1911	Tonmesser	Paired comparisons	Fusion	1	2	3	4	5	7	6		8	9	10	11

* The mathematical ratios are in some cases only approximate, *e. g.* the ratios for c′g′ᵇ and c′b′ᵇ, 5:7 and 9:5. The ratios differing slightly for the different authorities.

as members. This term has been used by some authorities as synonymous with consonance and to express the agreement of tones. The recurrent similarities of which the early scientists spoke and which Schopenhauer and Lipps have emphasized might also be classed under the category of blending. Preyer, Wundt, and Krueger have emphasized the importance of purity as a criterion and it has proved to be a specific mark of the agreement of the component tones.

Authorities agree with respect to the ranking of the octave and the fifth, first, and second, respectively; but, for the remaining intervals, we find disagreements due to the quality of tone, to the variation of the method of investigation, and to the variation in the basis of judgment.

It may never become possible to arrive at absolute agreement in the order of ranking, but it is plain from this brief historical survey that much may be gained in that direction by a clearer conception in regard to the nature of consonance, the analysis of conditions, and specific definition of terms for the purpose of experimental control. This will be the object of our next part.

ANALYSIS AND RANKING OF INTERVALS

The following series of experiments are the result of two years of investigation in the psychological laboratory of the State University of Iowa, extending from the fall of 1911 to the fall of 1913.

Preliminary Study of Apparatus

In a preliminary series of experiments, the reed organ, the pipe organ, blown bottles, the dichord, tuning forks, and the piano were tested with reference to their adaptability and efficiency in producing tones for an accurate measurement of the perception of consonance. Ten experienced observers were given the test, here designated as Series A, and were requested to give introspections with regard to the quality of tone. The recommendations which follow are based on this ranking and the introspections of the observers.

The reed organ was supplied with a set of accurately tuned

reeds, even temperament. The pipe organ pipes were of the open wooden type, producing a soft mellow tone. These were tuned to correspond to a set of forks tuned in just intonation, or forks of even temperament as the case required. The "blown bottles" were selected instead of the Stern tone-variators on account of their being more easily manipulated, *i.e.,* it was possible to have a complete series tuned permanently. This apparatus consisted of ordinary bottles of cylindrical shape, which, when empty, gave approximately the tone of c′, 256 v.d. They were tuned in just intonation by filling with paraffin. The mouthpieces were firmly attached to the bottles. The organ pipes and the "blown bottles" were energized by compressed air under constant pressure.

The dichord (Spearman's) was strung with a heavy piano string (wound wire), and was sounded by stroking with a cello bow. The wound string responds more readily, and the heavy cello bow eliminated the harsh and grating overtones and noises incident to the use of the ordinary wire string and the lighter bow of the violin. To avoid changes in the quality of the tone, the dichord was played in a uniform manner, care being taken to stroke the string evenly near the bridge.

In the early experiment, the forks were sounded mechanically before carefully attuned Helmholtz resonators, but later by a free movement of the hand. In the most successful mechanical devices, the handles of the forks were firmly mounted in rubber casings before the resonators, and were struck by means of hammers, mounted on steel springs. Wooden, cork, rubber and the regular felt piano hammers were tested, and the piano hammer was selected as producing the clearest tone. But, in spite of precautions, the thud incident to striking the fork, proved a distraction and caused impurities and, therefore, presentation by hand proved to be the best method.

The nine intervals, c′c″, c′e′, c′f′, c′g′, c′d′, c′e′, c′a′, c′b′♭, c′d′, were studied as sounded by each of these three instruments in two extensive series, (1) by the method of paired comparisons, and (2) by Stumpf's method of direct analysis; *i.e.,* in the former series, relative consonance was judged for successive

pairs upon each of the factors which may constitute consonance, and in the latter a direct judgment was based on fusion alone.

The reed organ was finally rejected because the tone was somewhat harsh and difficulty was experienced in securing a uniform timbre of the tone throughout the octave. The dichord was found impracticable as, beside showing variations in quality with slight variations of pressure and adjustment in bowing, it requires a change of the bridge for each note and is difficult to manipulate. The blown bottles, while producing a clear tone, presented difficulties in regulation. *Sylvester* (37) demonstrated that the pitch of a blown bottle is very difficult to control but we found that variations in the relative intensity of sound and the timbre were far more difficult to control. The inevitable difference in timbre made the tones stand apart in the two-clang in such a way as to be prohibitive.

The instruments available for the test were found to be the piano, the tuning forks, and the pipe organ.

The test of consonance with a rich quality of tone can be given more expeditiously by the piano than by the dichord. The piano offers an advantage in its availability and in the perfection it has reached as a musical instrument. It also has the advantage of being familiar and agreeable. Most of the sources of error, which may arise in the use of the piano, can be effectively guarded against. As three strings are sounded for each tone, each of these must be accurately tuned so that no beats arise in the single tone. The same end may be gained by damping two of the strings, thus leaving only one to vibrate. An accurate piano tuner can, however, eliminate the beats that are present in the single tones of the ordinary piano. To eliminate many of the impurities that may arise from resonance, the soft pedal should be used continuously in the test.

The organ pipes present the difficulty of regulating the wind pressure so as to keep it constant. As with the blown bottle, the variations in intensity and in the adjustment of lips causes considerable variations in the pitch. Uniformity of pressure and accurate adjustment is accomplished in the regular pipe organ by a rather complex mechanism, which it is difficult to

improve upon in the laboratory. It is, therefore, best to use a good organ where access to one can be had. The pipe organ of the Methodist Episcopal Church of Iowa City which we used. is a modern two-manual organ, which was in good condition. The most favorable stop for this experiment was found to be the "stopped diapason" which gives a tone intermediate in richness between the rich tones of the piano and the pure tone of the tuning fork. The tones of the organ are, however, not so clear-cut as the piano and tuning fork tones. Therefore, the observer has not the same certainty in his judgments of the organ tones as of the piano or tuning fork tones. Simultaneity in sounding the tones was regulated by opening and closing the stop after the keys of the interval had been pressed down.

The tuning forks are the most reliable in both pitch and timbre. If presented by hand, in a uniform manner, they give clear and distinct tones, which are especially well adapted for a test of consonance. The tuning forks used in these tests were accurately tuned to the tempered scale. The frequencies as recorded on the tonoscope registered: c'—258.6, d'^{b}—274, d'—290, e'^{b} — 308, e' — 326, f' — 246, g'^{b} — 364, g' — 387, a'^{b} — 410.14, a' — 435.9, b'^{b} — 460.8. b' — 487.8, c'' — 517.2. Precautions were taken to secure pure tones by sounding the forks in a uniform manner before tuned Helmholtz resonators. The twelve resonators were mounted on the rim of a wheel and the c' set on the center so that by giving a turn to the wheel any one of these in the rim could be swung into position horizontally to the right of the c'.

In the preliminary experimentation on the quality of the tone, two general facts were fairly well demonstrated. First, it was shown that the ranking of consonance will vary slightly for different qualities of tone. This is expressed quantitatively in later experiments. Second, when by the use of two sets of tuning forks, the just intonation was compared with the tempered intonation, no difference in ranking of the intervals large enough to affect the order resulted from the difference in temperament. This conclusion agrees with Faist's (3) statement with regard to minimal variations in vibration frequencies.

Criteria of consonance and dissonance

It is clear from the historical survey, and it was demonstrated in the preliminary experiments, that the fundamental reason for the great divergence in the ranking by experts and the consequent disparagement of the ranking of consonance and dissonance has been due to the failure to take common ground in the definition of these terms. Our first step was, therefore, to put the various claims of factors involved to a test in a long and painstaking series of analytical tests in which the various possible factors of criteria were isolated and discussed critically under control. This preliminary inquiry resulted in the recognition of the following factors:

For consonance:

1. Blending—a seeming to belong together, to agree.
2. Smoothness—relative freedom from beats.
3. Fusion—a tendency to merge into a single tone, unanalyzable.
4. Purity—resultant analagous to pure tone. (See Wundt.)*

For dissonance:

1. Disagreement—incompatibility.
2. Roughness—harshness, unevenness or intermittence.
3. Disparateness—separateness or seeming to stand apart—analyzable, "twoness".
4. Richness—resultant analogous to rich tone.

In terms of these factors we may then define consonance as follows: *When the two tones of a two-clang tend to blend or fuse and produce a relatively smooth and pure resultant, they are said to be consonant.* Dissonance is the reciprocal of this. "Agreeableness" which has played an important rôle in the popular conception and in the theory is here conspicuous by its absence. The perception of consonance as above defined there-

* Restfulness—a feeling of completeness, finality or satisfaction, with its opposite disquietude—a feeling of incompleteness, needing to be resolved, was first adopted as a fifth criterion, but it soon developed that it must be dropped as it is a variable criterion directly due to progression and association, which must be excluded.

fore becomes a cognitive act of discrimination rather than a mere feeling of agreeableness.

Method of procedure

The above definition, with its analysis into blending, smoothness, fusion and purity displayed on a chart, was read and discussed, giving all of the observers a consistent understanding of the definition.

The observers were instructed to judge each "two-clang" as an aesthetic object by itself, without respect to the effect of progression, meaning, association, or mood, and to make their judgments on each of the above given criteria in turn in separate series of experiments for each of the four criteria.

The experiment was made "with knowledge", and was conducted on the plan of an informal seminar, allowing a discussion of each judgment, but each observer recording his own final judgment. The intervals were sounded simultaneously, with a duration of approximately two seconds, and were repeated as often as requested by any observer—sometimes as many as fifteen or twenty times.

In determining the constant factors that enter into the perception of consonance and the ranking of the musical intervals, one must rely on the introspection and judgment of experienced observers of different types. For this experiment, therefore, eight observers were carefully selected on the basis of their training and fitness for the work.*

Even with the definition of consonance and the control of procedure here adopted, there remain many points of doubt and individual differences in opinion among observers as to the order of consonances. Since it was necessary to arrive at one

* They were the following: Professor C. E. Seashore, Assistant Professor Mabel C. Williams, Professor Edward Schaub, and Professor Robert Fullerton, Dr. Alma D. Schaub, Dr. Thomas Vance, Mr. Hazelette and the writer. Professors Seashore, E. Schaub, and Williams and Fullerton were members of the faculty, the last named being the head of the Department of Vocal Music. Dr. Thomas F. Vance was an advanced student in Psychology; Dr. Alma Devries Schaub had taken her Ph.D. in Psychology; and Mr. Hazelette was a graduate student in Physics and an experienced flute player.

particular order which might be considered a norm, the observers adopted the plan of sitting together in the experiment, proceeding very slowly, and discussing all cases of doubt or difficulty, analyzing the situation, varying the conditions of stimulation, and refining observations. This proved a very great advantage since each of the eight observers, of different types of training, offered to one another criticisms and suggestions for points of view and in this way distinctions were developed and errors of observation were eradicated which might otherwise have passed unnoticed.

Statement of results

As a group of trained observers, we found no difficulty in four-fifths of the sixty-six cases of paired comparisons at the beginning, and, after discussion, the differences of opinion centered about a still smaller number of the cases. Since each observer recorded for himself, we secured eight individual sets of ranking, although all of these were materially modified by the enlightment which came through mutual criticism and the repetition of trials. The record showed for each observer the ranking of the intervals for piano, tuning-fork, and pipe organ, and on each of the four factors, blending, smoothness, fusion, and purity separately. This series we may designate as *Series B*. The results will be stated in Table III after the results of the final test, *Series C,* have been stated in Table II.

It was the original intention to accept the average of these records (Table III) as a norm, but the discussion and mutual criticism was so stimulating and interesting that all the observers agreed to sit again and continue by the same method until all should agree and a unanimous verdict could be handed in as in the case of a jury. This was done with the piano and tuning-forks separately in what is here designated as Experiment Series C. The pipe organ was left out to shorten the labor, in view of the fact that the observers were of one mind that the piano and the tuning-forks were the best instruments available for the rich tones and the pure tones respectively.

The final returns in this series (C) are condensed in Table II,

where the record is kept for piano and organ for each of the eleven intervals on blending, smoothness, fusion, and purity in terms of the number of times a given interval was preferred. Since there were twelve intervals, the one that was preferred to every other would have a record of 11, meaning that it was preferred to eleven other intervals, the next one would be preferred to ten other intervals, etc. The actual rank is, therefore, approximately the inverse order of these numbers as is indicated by the Roman numerals.

The intervals are given in this table in the order in which they rank for the piano in the average.

It is manifestly out of the question to combine the ranking on fusion with the ranking of the other three criteria since the former results in a peculiar classification of its own. The other three criteria, however, seem to work together, prove mutually supplementary, and result in a fairly similar order of ranking. (See Table II). These are, therefore, brought together through the average in the last column of Table II, (Fig. 8), which may be regarded as the goal of the experiments in this Part on the order of ranking.

The data of this table are illustrated in Figures 1-8, which are self-explanatory and will aid materially in the interpretation of the table.

Table II is the principal table of facts to be considered here, but Table III is inserted for two purposes: (1) to furnish the tentative data for the pipe organ tones, and (2) to show to what extent the order of rank in Series C deviates from that of Series B (Table II).

The rank for the organ tone stated in Table III would probably not have changed very much if this tone quality had been carried through Series C as was done with the piano and tuning-forks. The change of relative rank for the piano and the tuning-forks from Series B to Series C may be seen by a comparison of these two tables, those in Table III representing the average of independent judgments of the eight observers and Table II the unanimous verdict reached later by the same observers.

While there are normal variations in the figures which denote

		Blending		*Smoothness*		*Fusion*		*Purity*		*Total* { Blending Smoothness Purity	
	P.	11	I	11	I	11	I	11	I	11.0	I
$c'c''$	F.	11	I	11	I	11	I	11	I	11.0	I
$c'g'$	P.	10	II	9	III	8	V	9	II	9.3	II
	F.	10	II	9	III	6	III	10	II	9.7	II
$c'a'$	P.	6	VI	10	II	3	VIII	7	IV	7.7	III
	F.	7	VI	10	II	2	VII	8	III	8.3	III
$c'e'$	P.	9	III	7	V	6	V	5	V	7.0	IV
	F.	8	IV	7	IV	7	IV	5	V	6.7	V
$c'f'$	P.	7	V	6	VI	5	VI	8	III	7.0	V
	F.	9	III	7	IV	7	IV	5	V	7.0	IV
$c'a'^{b}$	P.	5	VII	8	IV	3	VII	5	V	6.0	VI
	F.	6	VI	7	IV	3	VI	6	IV	6.3	VI
$c'e'^{b}$	P.	8	IV	5	VII	8	IV	2	VIII	5.0	VII
	F.	5	VII	3	VII	7	IV	3	VI	3.7	VII
$c'g'^{b}$	P.	4	VIII	4	VIII	4	VII	3	VII	3.7	VIII
	F.	4	VIII	5	V	4	V	3	VII	4.0	VIII
$c'b'^{b}$	P.	3	IX	3	IX	1	IX	4	VI	3.3	IX
	F.	3	IX	4	VI	1	VIII	3	VII	3.3	IX
$c'd'$	P.	2	X	1	XI	9	III	2	VIII	1.7	X
	F.	2	X	1	IX	8	III	1	IX	1.3	XI
$c'b'$	P.	1	XI	2	X	0	X	1	IX	1.3	XI
	F.	1	XI	2	VIII	0	IX	2	VIII	1.7	X
$c'd'^{b}$	P.	0	XII	0	XII	10	II	0	XI	0.0	XII
	F.	0	XII	0	X	10	II	0	X	0.0	XII

TABLE III—*Rank of consonances and dissonances* (*Series B*)

		Blending		*Smoothness*		*Fusion*		*Purity*		*Total* {Blending, Smoothness, Purity}	
		av.	m. v.	av.	m. v.	av.	m. v.	av.	m. v.	av.	m. v.
	P.	10.9	.2	11.0	.0	10.5	.7	11.0	.0	10.9	.2
$c'\,c''$	F.	11.0	.0	11.0	.0	11.0	.0	11.0	.0	11.0	.0
	O.	11.0	.0	11.0	.0	10.8	.3	11.0	.0	10.9	.1
$c'\,g'$	P.	9.0	1.0	9.0	1.0	5.6	.9	9.3	.9	8.2	.9
	F.	9.9	.2	9.4	.5	6.3	1.6	9.6	.5	8.8	.7
	O.	9.6	.6	9.4	.8	6.6	.9	9.3	.6	8.7	.7
$c'\,a'$	P.	6.8	.5	7.5	.8	3.0	.5	7.3	.6	6.2	.6
	F.	6.0	.7	7.1	.7	2.8	.8	7.5	1.0	5.9	.8
	O.	6.0	1.0	6.8	1.3	2.8	.6	7.0	1.0	5.7	.7
$c'\,e'$	P.	8.8	.6	7.5	1.1	7.1	.4	7.3	.9	7.7	.8
	F.	8.4	.5	7.5	.6	7.0	1.0	7.0	1.0	7.5	.8
	O.	9.0	.3	7.1	1.2	6.5	1.3	8.0	.8	7.7	.9
$c'\,f'$	P.	7.6	1.0	6.8	.8	4.9	.7	7.4	1.3	6.8	.9
	F.	7.9	.2	6.0	.8	6.0	.3	6.5	.8	6.6	.5
	O.	7.9	.5	7.3	1.1	5.1	.7	6.9	.9	6.8	.8
$c'\,a'^{b}$	P.	6.1	1.2	6.5	1.0	3.1	.7	6.4	1.3	5.5	1.0
	F.	5.5	.8	6.5	.9	2.8	.4	6.6	.8	5.4	.7
	O.	4.6	.5	5.3	.9	3.1	.7	5.9	.4	4.7	.6
$c'\,e'^{b}$	P.	6.1	.4	4.8	1.0	6.1	1.4	4.1	.7	5.4	.8
	F.	5.9	.2	2.5	.8	7.5	.6	4.3	.6	5.1	.6
	O.	6.9	.4	5.0	.8	7.0	1.0	4.4	1.0	5.8	.8
$c'\,g'^{b}$	P.	4.0	.0	4.1	.2	3.5	.8	4.3	.9	3.9	.5
	F.	4.9	.7	4.4	.9	4.1	.7	3.8	.4	4.8	.7
	O.	4.8	.8	4.8	.6	4.1	.9	4.0	.4	4.5	.7
$c'\,b'^{b}$	P.	2.6	.5	2.9	.2	1.0	.0	3.6	1.3	2.5	.5
	F.	3.0	.0	3.8	1.9	1.0	.0	3.8	1.2	2.9	.8
	O.	3.0	.3	3.1	.9	1.6	.6	3.6	1.7	3.8	1.1
$c'\,d'$	P.	2.4	.7	1.4	.5	6.9	1.7	2.0	.3	3.2	.8
	F.	1.9	.2	1.0	.0	7.0	.5	1.0	.0	1.4	.2
	O.	1.3	.4	1.3	.4	8.1	.7	1.3	.4	3.0	.6
$c'\,b'$	P.	.6	.3	1.5	.5	.3	.0	1.0	.0	.9	.1
	F.	1.1	.2	2.0	.3	.0	.0	2.1	.2	1.3	.2
	O.	2.0	.3	1.8	.6	.3	.2	2.8	1.5	1.7	.3
$c'\,d'^{b}$	P.	.0	.0	.0	.0	9.8	.4	0.0	.0	2.5	.1
	F.	.0	.0	.1	.1	9.8	.4	.1	.1	2.5	.2
	O.	.0	.0	.0	.0	9.3	1.0	.0	.0	2.3	.3

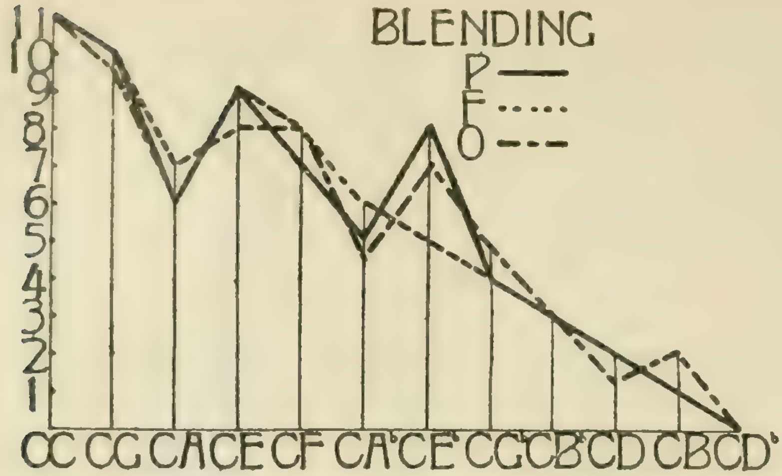

FIG. 1. Blending, for piano, forks, and organ respectively.

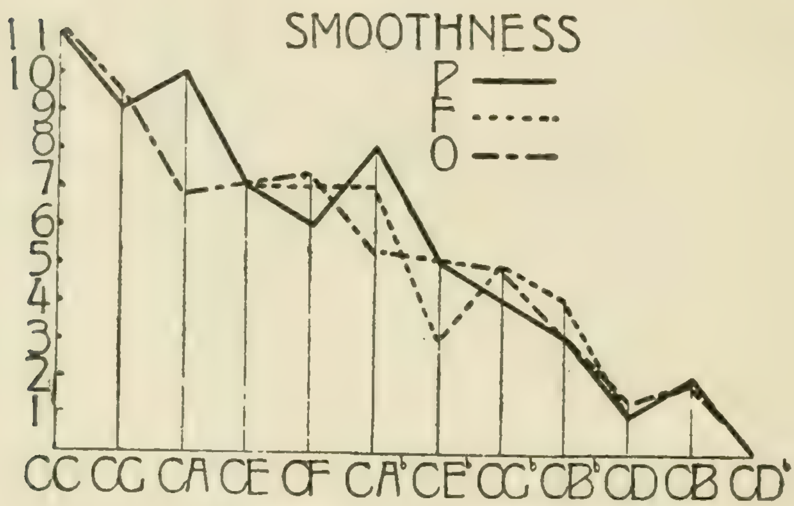

FIG. 2. Smoothness, for the three instruments.

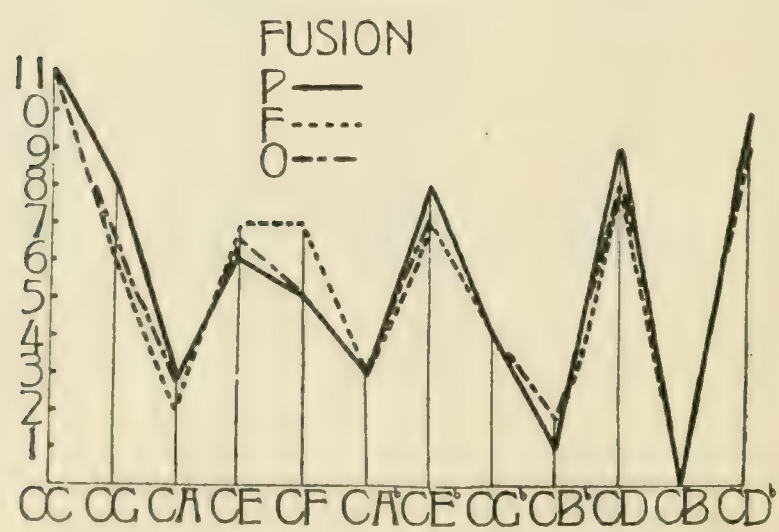

FIG. 3. Fusion, for the three instruments.

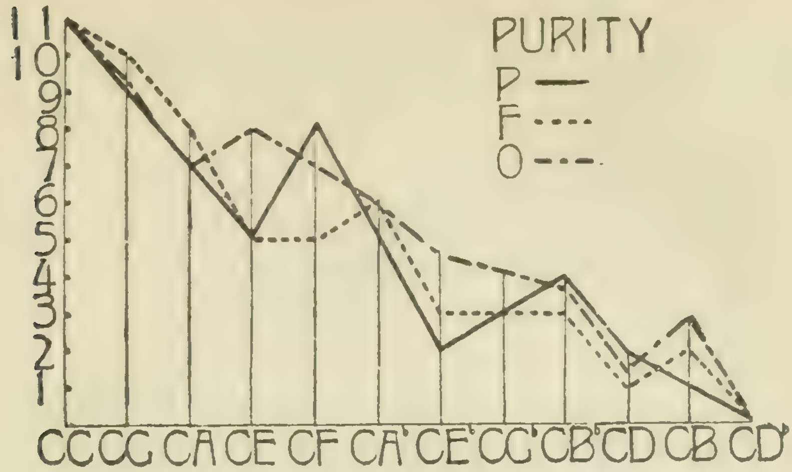

FIG. 4. Purity, for the three instruments.

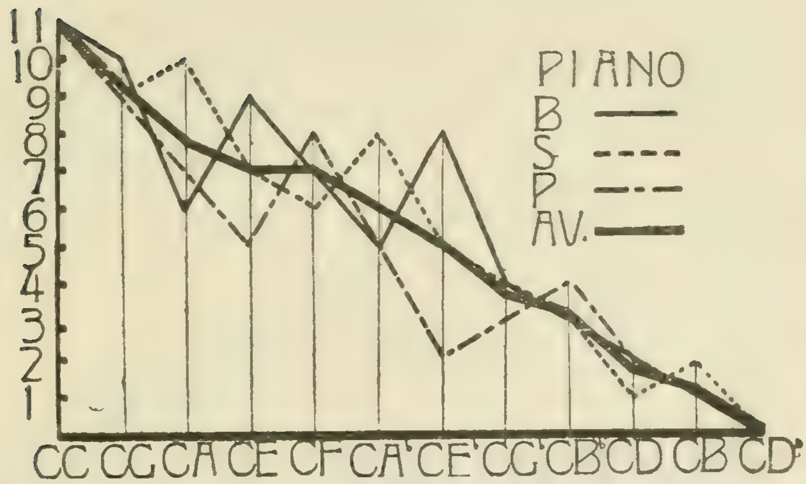

FIG. 5. Blending, smoothness, and purity, with the composite (Av.) from these for the piano.

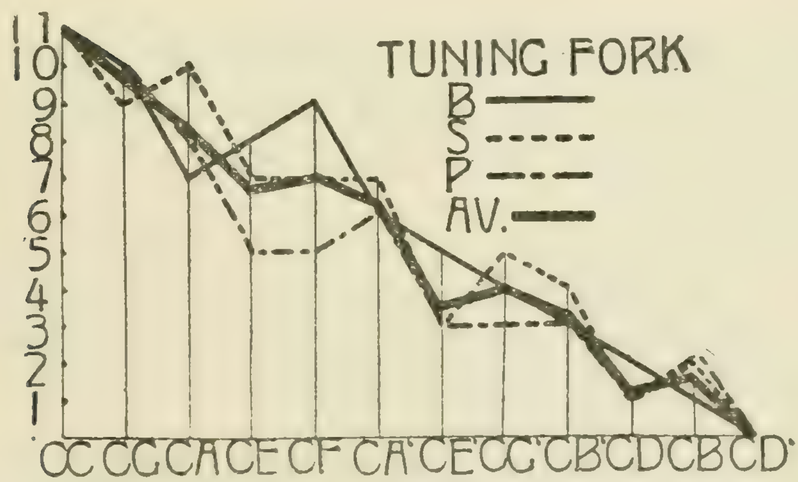

FIG. 6. Blending, smoothness, and purity, with the composite from these for the tuning forks.

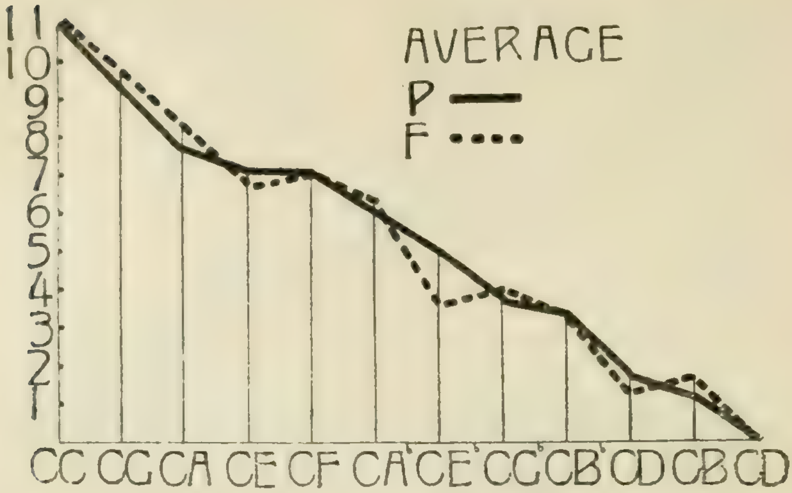

Fig. 7. Comparison curves for piano and tuning forks.

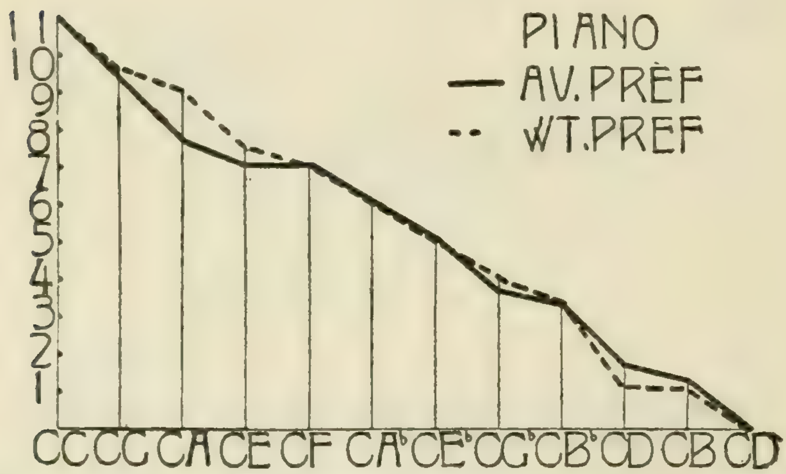

Fig. 8. Comparison of the average preference and the weighted average preference for the piano.

the number of preferences, few of these are enough to alter the order of rank. The nature of the change and the quantitative difference may readily be seen by comparing corresponding figures in the two tables.

The mean variation of the preferences in Series B is given in Table III in order to convey some idea of the variability in individual decisions by different observers. The mean variation may also be regarded as showing, to some extent, the relative difficulty of different intervals.

One further step should be taken: a group of trained observers should proceed as in Series B and C above, except that only

one decision should be rendered for the three criteria on the following basis: *Give the decision on blending alone if the degree of blending is perceptibly different; if not, make the decision on smoothness; and, if there is no difference in either smoothness or blending, base the decision on purity.*

That procedure has the merit of basing the decision on a single criterion—the appropriate one in each case—instead of striking an average of three. The ranking by this method may possibly modify the order denoted by the average in Table II.

To what extent there is likelihood of any effect upon the order of rank by this step may be judged upon analyses of the basis of the decision for each part of intervals as recorded in Series C. In this series the group worked for a unanimous verdict on what constituted the deciding criterion, just as it worked for a unanimous verdict on the order. The final decisions of the group are contained in Table IV.

TABLE IV—*Basis of judgment for individual pairs*

	$c'g'^b$	$c'd'^b$	$c'f'$	$c'd'$	$c'e'$	$c'e'^b$	$c'g'$	$c'c''$	$c'a'^b$	$c'b'$	$c'a'$
$c'd'^b$	s										
$c'f'$	b	s									
$c'd'$	s	s	s								
$c'e'$	b	s	p	s							
$c'e'^b$	b	s	p	s	p						
$c'g'$	b	s	b	s	s	s					
$c'c''$	b	s	b	s	p	p	b				
$c'a'^b$	s	s	b	s	b	s	b	b			
$c'b'$	b	s	b	b	b	b	b	b	b		
$c'a'$	s	s	s	s	s	s	b	b	s	b	
$c'b'^b$	b	s	b	s	b	b	b	b	s	b	s

b—blending; p—purity; s—smoothness. The order of the intervals in this table is the order used in giving the test.

Table IV shows that thirty-one of the pairs are determined primarily by the factor of blending, thirty-one primarily by the factor of smoothness, and only in four pairs does purity enter in as the determining factor. In the comparisons of the dissonant intervals $c'd'$ and $c'd'^b$ with the remaining intervals the element of roughness is the characteristic that determines the judgment in every case, except in the pair $c'b'$ and $c'd'$. $c'c''$ and $c'a'$ depend primarily upon the criterion of blending. In the comparison with $c'e'$ and $c'c''$, purity enters as the deciding factor. In comparison of $c'a'$ with these two intervals,

smoothness gives c′a′ the first place. The ranking of c′a′ is largely determined by the criterion of smoothness. However, in the judgments of the pairs c′a′ and c′b′, and c′a′ and c′b′^{b}, blending becomes the criterion. c′e′ is determined by blending in its comparisons with c′b′^{b}, c′f′, c′a′^{b}, c′b′, c′b′^{b}. In the judgment of the difference of major and minor thirds, purity is the most distinct element which gives c′e′ the better rank. c′f′ in all cases not mentioned above, except in the case of c′e′^{b}, is judged on the blending factor. In the case of c′e′^{b} the purity of the two intervals determine their rank, as c′f′ ranks II in purity while c′e′^{b} ranks III. The minor sixth ranked above the minor third on account of its relatively greater degree of smoothness. In all other cases, not mentioned above, the minor third is judged on the basis of blending. c′b′ is judged on its lack of the factor of blending in all cases, except in its comparison with c′d′^{b}. In the two dissonances c′b′ c′d′, there is difficulty in deciding on account of their apparent difference in character. Both are extremely dissonant, c′b′ ranking lower in blending and c′d′, on the other hand, ranking perceptibly lower in smoothness. As the blending is the more important factor, c′d′ is given the first place. In the pair c′b′^{b} and c′a′^{b}, there is a minimal difference in blending, but c′a′^{b} is perceptibly smoother than c′g′^{b}. In the intervals c′g′^{b} and c′b′^{b} blending determines the judgment. In some cases, such as c′c″ and c′a′, all three criteria coöperate. The superior ability to perceive consonance consists in selecting the factor which most influences the ranking.

Turning then to the specific question before us, we may estimate, on the basis of data in Table IV, what the probable effect would be of a single decision according to the above directions as compared with an average decision for these three criteria.

The analysis is reduced to final figures in Table V in which "average preference" is taken from the last column in Table II for piano and tuning forks respectively;—B., S. and P. denote the number of times the judgment was based upon the factor of blending, smoothness, and purity respectively, for each interval when compared with other intervals according to the exhibit in Table IV.

"Weighted preference" is calculated by weighting the average in accordance with the number of decisions based on each of these factors. Thus, turning to Table IV, we see that for the interval c′g′ the decision may be reached seven times on blending, and four times on smoothness, while purity need not be considered; the preference rank for blending is 10 and for smoothness 9. For this interval, we accordingly get $7 \times 10 + 4 \times 9 \div 11 = 9.6$ as the weighted average.

Section A, of table V (Fig. 8) is based on the piano quality but as a rough approximation we have used the same weighting for tuning forks, Section B of Table V.

TABLE V.—*Comparison of weighted and average preferences*

A. For Piano

Int.	Av. Pref.	Order	Wt. Pref.	Order	B.	S.	P.
$c' c''$	11.0	I	11.0	I	6	2	2
$c' g'$	9.3	II	9.6	II	7	4	
$c' a'$	7.7	III	9.0	III	3	8	
$c' e'$	7.0	IV	7.5	IV	5	4	2
$c' f'$	7.0	V	6.9	V	7	3	1
$c' a'^{b}$	6.0	VI	5.9	VI	5	6	
$c' e'^{b}$	5.0	VII	5.0	VII	3	5	3
$c' g'^{b}$	3.7	VIII	4.0	VIII	7	4	
$c' b'^{b}$	3.3	IX	3.0	IX	7	4	
$c' d'$	1.7	X	1.1	X	1	10	
$c' b'$	1.3	XI	1.1	XI	10	1	
$c' d'^{b}$	0.0	XII	0.0	XII		11	

B. For Tuning Fork

Int.	Av. Pref.	Order	Wt. Pref.	Order
$c' c''$	11.0	I	11.0	I
$c' g'$	9.7	II	9.6	II
$c' a'$	8.3	III	8.3	III
$c' e'$	6.7	V	8.0	V
$c' f'$	7.0	IV	8.1	IV
$c' a'^{b}$	6.3	VI	6.5	VI
$c' e'^{b}$	3.7	VIII	3.5	VIII
$c' g'^{b}$	4.0	VII	4.4	VII
$c' b'^{b}$	3.3	IX	3.4	IX
$c' d'$	1.3	XI	1.1	X
$c' b'$	1.7	X	1.1	XI
$c' d'^{b}$	0.0	XII	0.0	XII

It is rather surprising that for the piano the weighting does not alter the order found for the mere average. For the purpose of a tentative working norm with the piano, tempered scale, (when the decision is made on one of the three factors directed as above, namely: if possible, on blending; if not, on smoothness; and if not on either of these two, on purity), the

order in which the intervals are given in Tables II, III, and V may be regarded as the standard order from the best consonance to the worst dissonance, as expressed in Fig. 9.

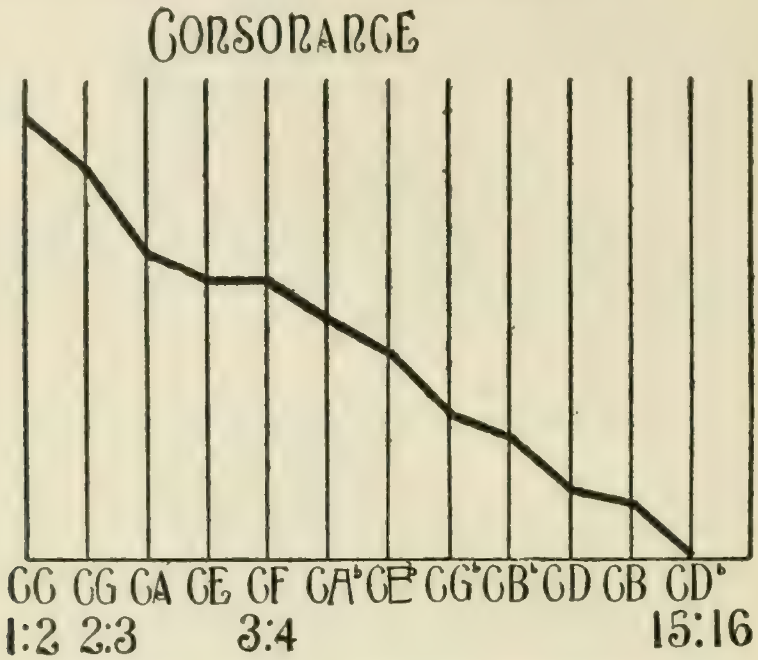

FIG. 9. Norm: the order of consonance-dissonance for the piano.

A rough approximation to the relative degree of certainty in the preference of one interval to another is shown by the numbers which denote the number of times an interval is preferred. This is true both for great differences, *e.g.*, c′c″ and c′d′, as well as for adjacent intervals, *e.g.*, c′c″ and c′g′. The irregularities in the curve, of course, mean just such differences; thus, in Fig. 9 the difference between the consonance of c′c″ and c′g′ is decidedly larger than the difference between c′e′ and c′f′.

Since the primary object of this investigation was to standardize the measurement of the sense of consonance, the main object of the present section of the work was to secure a norm which might be considered a conventional standard for such purposes. All this work on ranking is, therefore, merely accessory to the main object of this investigation. For this reason, the detailed discussion of the introspections and the theoretical interpretation of the empirical records may perhaps best be deferred for a more elaborate investigation.

We have here, at least, a tentative norm established for the first time after recognition of the principal factors isolated under experimental control. Later refinements of experiments may make minor changes in the order, but we have made progress by developing a working principle—the recognition of specific criteria.

MEASUREMENTS OF THE PERCEPTION OF CONSONANCE AS A MUSICAL TALENT

Having determined the constant factors of consonance, the ranking order of the intervals, and the method and apparatus adapted to this test, the writer will in this section, give an account of measurements made under the above prescribed conditions. These measurements were made with three ends in view: first, to secure measurements of individual abilities; second, to establish a norm; and third, to test this measurement under controlled conditions.

Apparatus

The intervals of the octave c′c″ were presented by the method of paired comparison, being played on a piano accurately tuned in the tempered scale. The tones were sounded with moderate loudness, the soft pedal being applied continuously during the test. Each two-clang was sounded with a duration of approximately 2 seconds, with an interval of 1 second between the members of each "pair", and 4 seconds between each repetition. Every pair was sounded at least twice, more frequently three or four times, and in the more difficult comparisons a relatively greater number of times.

Definition and illustration of criteria

Special precautions were taken to impress the fact that mere agreeableness is not the basis of the decision. It was pointed out that a dissonance may be very agreeable for some musical purposes and that the rich musical body of the two-clang, such as the major third, may be the most agreeable and still not the most consonant. It was made clear that the test is a cognitive act of discrimination as opposed to the traditional affective test employed in music.

A placard bearing the definition of consonance was placed before the class, reading as follows:

Consonance is:

(1) *Blending,* a seeming to agree, to belong together.

(2) *Smoothness,* relative freedom from beats.

(3) *Purity,* thinness of tone, absence of richness.

The conductor of the test then explained tersely and concretely how the experiment would proceed, how to record, and how to apply the above-defined criteria in arriving at a decision. The 66 trials, which constitute one complete set, were made in one hour. The intervals used to illustrate the above criteria were sounded in a different octave from the one used in the test. Both the negative and positive aspects of the criteria, *i.e.,* consonance and relative absence of consonance were presented. Thus, without naming the intervals, c′c″—c′b′ and c′e′—c′b′ were given to illustrate blending, the first interval in each pair bringing out the positive, the second, the negative aspect of blending. As illustrations of smoothness, the pairs c′b′—c′d′ and c′a′—c′e′^{b} were played. In both of these pairs judgment is best based on smoothness, the first presenting a large difference, the second a small difference. The element of roughness in the minor third is scarcely perceptible, but it is the deciding factor in the comparison of these two intervals, as the major sixth undoubtedly owes its higher rank to its greater degree of smoothness in this pair. c′c″—c′e′^{b} were selected to illustrate tonal purity. The octave is a relatively pure interval, while the minor third is rich in quality. In order to give a still clearer conception of purity, a criterion somewhat vague in the mind of the average observer, a tuning-fork was sounded before an attuned Helmholtz resonator, which gave a clear pure tone; and this was compared with the rich tone of the piano and violin and with two-clangs. The observers were finally instructed to make their judgments on each pair with respect to the three criteria in the order named and illustrated; namely, giving first preference to blending, second to smoothness, and third to purity. In all cases where they found blending to be the decisive factor, they were instructed to judge on this criterion. In case they

were unable to decide on the basis of blending, they should take the second criterion into account; and, if smoothness was not decisive, the test of purity should be applied. In a large portion of the cases the three criteria coöperated and agreed.

Since the sequence of intervals is a factor that cannot be overlooked, a fixed order of comparisons, which distributed the consonances and dissonances fairly was adopted, as given in Table VI. The body of this table contains three sets of information:

TABLE VI—*The order of trials, the correct preference and the schedule of demerits*

	$c'g'^b$	$c'd'^b$	$c'f'$	$c'd'$	$c'e'$	$c'e'^b$	$c'g'$	$c'a'^b$	$c'b'$	$c'a'$	
$c'd'^b$	1 1 4										
$c'f'$	2 2 3	3 2 7									
$c'd'$	22 1 2	4 2 2	5 1 5								
$c'e'$	23 2 4	24 2 8	6 2 1	7 2 6							
$c'e'^b$	39 2 1	25 2 5	26 1 2	8 2 3	9 1 3						
$c'g'$	40 2 6	41 2 10	27 2 3	28 2 3	10 2 2	11 2 5					
$c'c''$	52 2 7	42 2 11	43 2 4	29 2 9	30 2 3	12 2 6	13 2 1				
$c'a'^b$	53 2 2	54 2 6	44 1 1	45 2 4	31 1 2	32 2 1	14 1 4	15 1 5			
$c'b'$	61 1 3	55 2 1	56 1 6	46 1 1	47 1 7	33 1 4	34 1 9	16 1 10	17 1 5		
$c'a'$	62 2 5	63 2 9	57 2 2	58 2 7	48 2 1	49 2 4	35 1 1	36 1 2	18 2 3	19 2 8	
$c'b'^b$	66 1 1	64 2 3	65 1 4	59 2 1	60 1 5	50 1 2	51 1 7	37 1 8	38 1 3	20 2 2	21 1 6

For each block the top number denotes the order of the trial; the first number below denotes whether the first (top heading) or the second (side heading) is the better; the third number denotes the number of demerits assigned.

In each square the *first* figure denotes the order of trials, the *second* the correct preference, and the *third* the amount of demerits (to be explained later), in case of error in the preference.

The sequence of trials is the conventional order in a series of paired comparisons: each two-clang is named by reading the name at the top and the name at the side in two columns that intersect and form a given square. The two-clang given at the top of a column was always sounded first and the one at the side second. The observer was required to express his preference 1 or 2 according as he preferred the first or the second.

The evaluation of the record is based on the ranking of the intervals established in the preceding part, Table II.

The weighting of demerits

In order to arrive at a method of grading the records, an arbitrary scale of demerits was established. The amount of the demerit for each incorrect judgment was computed on the deviation from the norm. One unit of demerit was given for each step of deviation from the norm. Thus, the observer, in his judgment of the pair c′c″-c′g′ signified by recording 2 that the latter interval is the better consonance, he was given a demerit of 1, as c′g′ is in rank removed 1 step from c′c″. If, on the other hand, in his judgment of the pair c′c″-c′e′♭, he records his choice as 2, thereby placing the minor third as a better consonance than the octave, the error is more significant, and he is given a demerit of 5. The greatest possible demerit for any one judgment is 11 as in the pair c′c″-c′d′♭.

If every answer should be wrong there would be 286 demerits according to Table VI. But this cannot happen because in the long run fifty per cent. of the judgments would be correct by chance, since there are only two possibilities in each case. This reduces the maximum number of probable demerits to 143, which would be the number for one who had *no ability* to appreciate differences in consonance and dissonance and depended entirely upon chance. Such demerit, therefore, is equivalent to 100 ÷ 143, or seven-tenths of one per cent. The records may

then be stated in terms of per cent. of success in conforming to the norm by deducting .7 per cent. from 100 per cent. for each demerit earned. While this weighting is somewhat arbitrary, it does approximate justice to the situation because it is approximately proportional to the magnitude of the error in each case.

Since it would complicate matters to require the observer to follow the order of Table VI in recording, the records were kept in straight columns, the first three containing twenty records each and the fourth six,—one column being left blank between each of these to give space for the marking of error and demerit.

This test was first made on the students in the elementary psychology class in the University of Iowa for the class of '12-'13. After this research had been completed the department furnished similar records for the '13-'14 class; the following year Miss Nesta Williams furnished the records for the '14-'15 class; and Mrs. Esther Allen Gaw contributed the records for the class of '15-'16. Since the publication of the original document has been delayed it is possible to incorporate this large mass of data which have been gathered through the very generous coöperation of the members of the department of Psychology, into one group making 1045 cases.

The norm established.—The general distribution of these cases is shown in Fig. 10, which is based on a number of cases

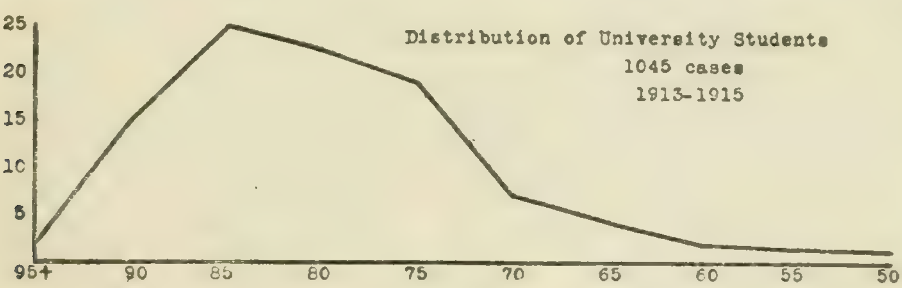

FIG. 10.

sufficiently large taken as a norm. In other words, this curve serves two purposes: it shows how abilities in this act vary and distribute themselves; and it may serve as a norm in terms of which any future record may be interpreted.

Bearing on the order of ranking—The data contained in Part

II bear in themselves a basis for the ranking of intervals which may serve at least as a basis for the criticism of the norm established before. The records for the class of '12-'13 are tabulated to show the total number of preferences for each interval. These are shown in Table VII. In the first column the intervals are given in the order accepted as a norm. (Fig. 9, Table V). The second column contains the per cent. of cases in which each interval was preferred over all with which it was compared. From this the rank of the intervals is indicated in column 3 which shows the deviation of this empirical ranking from the norm.

TABLE VII—*The rank of the intervals in the 1913 class test*

	% of choices	Rank of choices
c′ c″	12.4	2
c′ g′	10.0	5
c′ a′	11.8	3
c′ e′	13.1	1
c′ f′	9.6	6 or 7
c′ a′^{b}	9.6	6 or 7
c′ e′^{b}	10.5	4
c′ g′^{b}	9.8	8
c′ b′^{b}	6.5	9
c′ d′	3.8	10
c′ b′	2.1	11
c′ d′^{b}	1.8	12

A glance at this table shows rather satisfactory agreement between the standard order and the empirical order. The orders VIII, IX, X, XI, and XII are the same in both cases, and with these may be counted VI and VII which happen to tie. The deviations occur, therefore, in the first five intervals, chiefly, *i.e.*, in the consonances; and it will be observed that these deviations are due mainly to the fact that the major third is unduly preferred in the empirical rankings. This can be accounted for by the fact that the factor of agreeableness had not been eliminated satisfactorily in the test of this first year. The musical value of the third as compared with the octave must have influenced the students to some extent so that there was a large number of cases in which the major third was preferred to the octave and the fifth. The dropping of the major third in rank will of course raise the octave and the fifth and secure closer agree-

ment with the norm. The same principle is perhaps also operative in tending to throw the middle range somewhat higher than the norm. The intervals $c'e'^{b}$ and $c'g'^{b}$ and $c'b'^{b}$ are rich musical intervals which may have been favored slightly on the same fallacy as that which threw the third off.

It will probably be found that this sort of error was eliminated by more rigid instructions on this point, emphasizing the fact that the preference is an intellectual judgment rather than an appreciation of musical value in the terms of agreeableness.

With this one exception then, the empirical results on the whole tend to confirm the order established in the norm.

The class of '12-13 was required to fill out a questionnaire on musical training in the form given by Professor Seashore in the report of the Committee on "The Standardization of Pitch Discrimination" (25). This questionnaire is designed to secure a measure of general musical ability, musical training, musical environment, and the expression of musical feelings. For the present purposes, the members of the group were ranked on musical training. The Spearman co-efficient R between this musical training and the record for the consonance test is only $R = .02$, which is very remarkable in view of the fact that one should ordinarily expect those who had had musical training to do better than those who had had none. This absence of correlation is of the greatest significance for the value of this test in that it tends to show that it is fairly independent of training and will, therefore, have diagnostic value.

The correlation between perception of consonance and ability to perform, *i.e.*, to sing or play in music is based upon the same questionnaire returns, gave the Spearman coefficient of $R = .06$ which again is an indication of the effect that the perception of consonance is quite independent of training because it would be fair to assume that the degree of correlation would easily be covered up by the principle of selection by which those who have a good ear for consonance would be likely to acquire musical training.

The correlation between pitch discrimination and the perception of consonance by the Pearson product-moments method

was found to be $r = .18$. This is not so large a correlation as one should really expect and it may be that this is due partly to the fact that pitch discrimination is an immediate sensory experience, whereas the perception of consonance as here tested involves a complex judgment.

The Pearson coefficient for the correlation for consonance and tonal memory is .34 which speaks for the close relationship of these two factors.

Evaluation of the method.—Roughly, the goal of this series of experiments has been reached; we have found a norm for the order of consonances and dissonances and a norm for the distribution of abilities among university students. Both of the fields here opened up must be worked more fully and the two problems are quite distinct. In connection with these experiments, much material has been collected and much is inherent in the records themselves which would throw light on the validity and interpretation of these norms as well as upon the technique of measurement.

But some place must be set for the division of labor; and, since Mrs. Esther Allen Gaw and others have taken up the experimental work in the laboratory from the point at which it was left by the writer in 1914 and propose to carry the analysis of this measurement into finer details, this report must come to a close with the apology that it is only preliminary.

Recommendations Toward a Standard Test

The method of procedure that has been followed, as stated above, can be recommended in giving a test in the perception of consonance, but it is necessary to emphasize certain precautions that must be followed.

In giving the interval, the experimenter must exercise care in the striking of the two keys so as to make the tones equally strong, for the factor of intensity plays a rôle in consonance. The experimenter should practise the combinations and order of pairs until he can play them with ease and without hesitation in an approximately uniform manner.

As has been pointed out by *Seashore* (25), "the tone most

favorable for accurate results" is one of moderate intensity, "the just perfectly, clearly perceptible tone." "It is ordinarily purer than a stronger tone and favors concentration." This precaution in the measurement of pitch discrimination applies as well to the measurement of the perception of consonance. The piano tone is rich and tends to develop many impurities when sounded loudly. These are partially at least if not wholly eliminated through the use of the soft pedal.

Uniformity of duration is important, as the sounding of one tone or note of the two-clang longer than the other proves a distraction and disturbs the observer in his discrimination. It also gives him a clue as to the interval played and changes the nature of the test. The longer duration of one note of the pair tends to change the character of the interval as this note takes a dominant place in the perception, and thus influences the judgment.

The two clangs within the pair should be presented in rapid succession with a constant interval of 1 second, as discrimination of successive stimuli involves the element of memory. A brief interval, therefore, presents the best condition for comparison as "the curve for tonal memory shows that the accuracy of memory falls off very rapidly, immediately after the first second of the interval." (25). A longer interval is necessary between the pairs to eliminate the influence of progression of intervals. Each pair should stand distinctly by itself as a stimulus for comparison and should have no reference to the preceding or succeeding pair. An interval of four seconds accomplishes this end.

The order of presentation of the pairs should be definitely determined by the preparation of a key in which the consonances and dissonances are distributed throughout. This order should also distribute the easy and difficult comparisons, so that the test will be somewhat uniform in difficulty of discrimination throughout sections of the whole test. Such an order prevents undue fatigue at any one point in the test as it conforms in some measure to the double fatigue order, which is a rule of of experimentation in psychological tests. The order also prevents the succession of two-clangs in a definite harmonic series, which must be avoided in the test.

The key given above Table VI was prepared with these precautions in mind, and is well adapted to the measurement of the perception of consonance. If the pairs are to be given in the reverse order, a different key should be made and given separately. The order should not be reversed in the repetitions, as this causes confusion and leads to mistakes in recording.

It has been noted how the element of fatigue may, to some extent, be obviated by the arrangement of the order of presentation. A test of this nature is naturally interesting if conducted in the correct manner, and elicits a continuous conscious effort, which becomes fatiguing if prolonged beyond the endurance of those tested. As has been pointed out by Seashore (25), this fatigue is not a fatigue of the sensory process, but it arises from the concentration of the attention. The attention tends to fluctuate and become distracted when engaged in continuous concentration for any length of time. This is more the case, the greater the conscious effort needed to make an intelligent choice or judgment. The perception of consonance requires this concentration to a very great degree, as it is relatively complex in its nature and requires a keen discrimination and constant application to perceive differences. In this respect, it differs from the mere sensory tests which involve only one factor. As an additional precaution against ennui, or to prevent fatigue, a rest is allowed after each group of ten trials. This divides the one hour recitation time during which the test is given, into three periods, of which the last two, when the element of fatigue is liable to be prevalent, are somewhat shorter than the first period. This tends to equalize the conscious effort, and prevents incorrect judgments due to fatigue.

What has been stated above applies to the sustaining of the attention. In order to secure the attention of the observer, a further precaution is taken in securing the right attitude toward the test, that will stimulate the conscientious application. The experimenter, in giving a class test, must have the energetic and enthusiastic coöperation of all participants. This cannot be overemphasized as an important factor in the measurement of the perception of consonance, which requires such a careful and discriminating application of attention to the details involved.

Summary of Conclusions

Among the conclusions reached in this study the following are prominent:

1. The historical failure to reach an agreement in regard to the rank of consonance and dissonance is due largely to a disagreement as to what constitutes consonance.

2. The perception of consonance is a cognitive process, involving the factors of blending, smoothness, and purity.

3. The order of the ranking of the intervals varies for different qualities of tone. The order has been established for tuning forks, piano, and pipe organ.

4. The uniformity of the distribution curves for different classes tends to show that the factors of the test can be controlled so as to make it well adapted as a class test.

5. A system of weighting errors has been established.

6. The distribution of grades reveals a very great diversity in this capacity among normal observers.

7. The perception of consonance is elemental in a secondary sense in so far as it is based rather on the elemental capacities for pitch discrimination and tonal memory than on acquired musical ability or training.

8. The test in itself is not an adequate measure of musical capacity, but forms one of a series including pitch discrimination, tonal memory, sense of rhythm, tonal imagery, etc. that may be advantageously used for this purpose.

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A REVISION OF THE CONSONANCE TEST

BY

ESTHER ALLEN GAW

The consonance test as described by Malmberg in the foregoing article[1] yielded excellent results, but Professor Seashore and Dr. Malmberg both saw the possibility and need of further refinement of procedure. The writer has continued the experimental study of the problem and wishes to report here certain revisions, notably: the elimination of undesirable intervals, the elimination of the system of demerits, and the simplifying of the definition and concept of consonance. A preliminary norm for children has also been established.

After giving the test in the fall of 1915, the records of the three hundred and eight sophomores enrolled in elementary psychology in the University were analyzed for the purpose of determining how the actual order of preference by those tested compared with the norm, what intervals were exceptionally easy or exceptionally difficult and what constant tendencies to error might be discovered.

In Fig. 1, the sixty-six comparisons are so arranged as to show for each comparison first, the number of demerits assigned (heavy solid curve); second, the per cent. of mistakes (broken curve); and, third, the weighted record (dotted curve). The vertical scales for these three curves are respectiveiy, one demerit for each space in the first, ten per cent. of mistakes for each space in the second, and one hundred weighted demerits in the third, the three scales being designated A, B, and C respectively.

The first curve, then, shows simply how many demerits were assigned to a mistake for each pair. The second shows the per cent. of mistakes for each pair. Since there were three hundred and eight cases and each person made one judgment for each pair, the per cent. of mistakes is found by dividing the number of mistakes for a given pair by 308. And the weighted demerits

[1] Malmberg, C. F. The perception of consonance and dissonance (in this Volume).

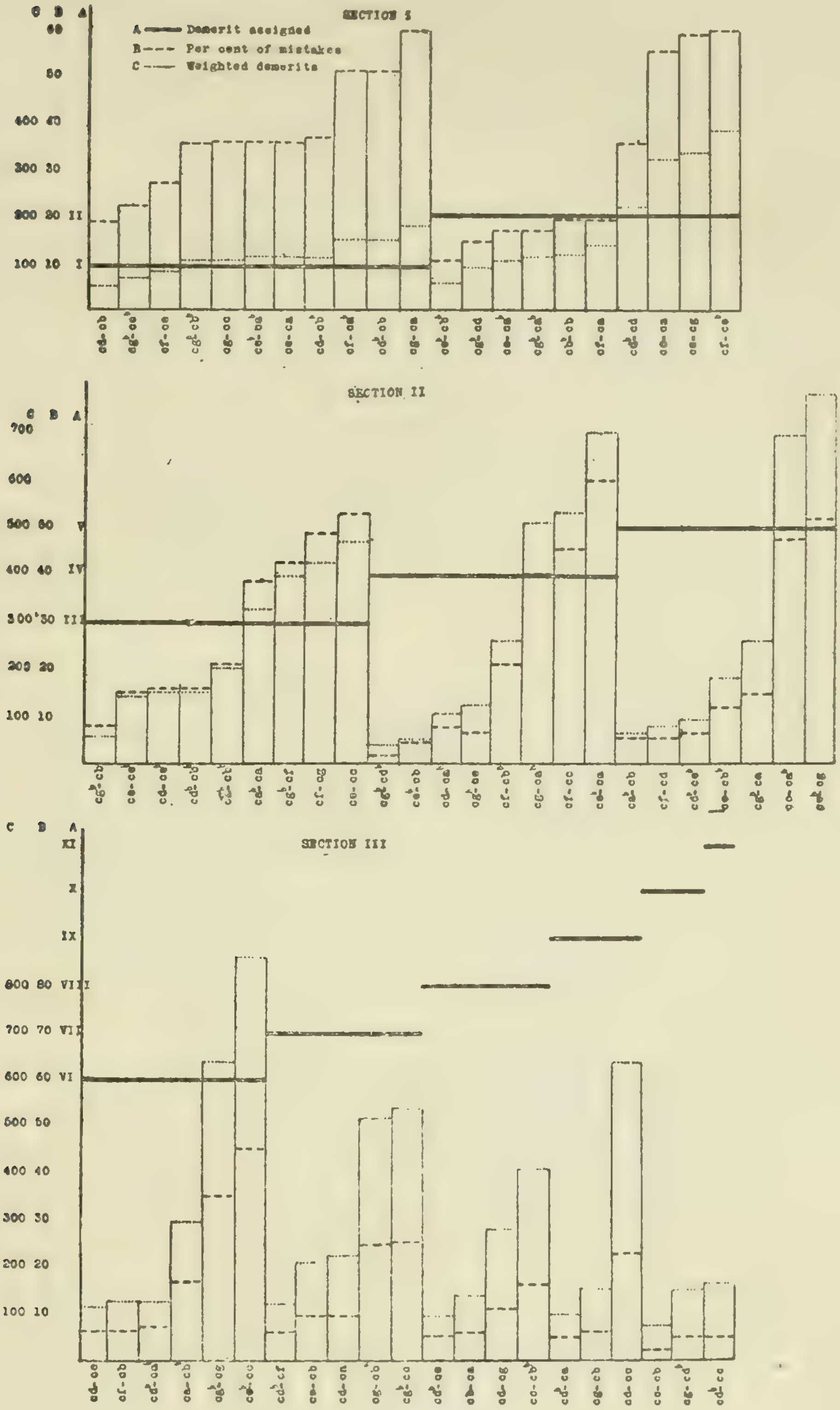
SECTION I
A —— Demerit assigned
B - - - Per cent of mistakes
C ----- Weighted demerits
SECTION II
SECTION III

FIG. 1

of curve c denote the number of mistakes for each pair multiplied by the demerit assigned to that pair.

The curve is divided into three sections merely for the convenience of placing on the page; it should be regarded as continuous.

It will be seen in the curve that, in each group of pairs for each demerit, there are some pairs that show a relatively low and others a relatively high number of mistakes. If we isolate these we shall probably find that these group themselves according to some principle, or principles, which may explain the respective tendencies. For this purpose we may take from those that receive the highest number of mistakes, three for each of groups 1-4 two from each of groups 5-7; and one from each of groups 8 and 9. A corresponding number from each group of those that show the smallest number of mistakes may represent the other extreme.

In examining these we discover certain motives which operate to increase the number of mistakes, if they work in opposition to the difference that is to be perceived, and to reduce the number of mistakes, if they tend to increase the difference. The former may be said to operate negatively, the latter positively.

A. *Richness of clang quality.* By this is meant the relatively rich, complex, and pleasing quality of certain clangs in contrast to the pure, thin, and, in some respects, harmonically less desirable clang quality of other equally good consonances or semi-consonances. Thus, while the octave is the best consonance, the major third is given that rank in these tests. Here there is a conflict between a relatively pure consonance and one relatively rich, because mediocre and poor observers are influenced by this richness in one of the members of a pair.

Throughout our series, then, if a relatively pure and thin clang is to be preferred to a relatively rich clang, that will lead to a corresponding increase in the number of mistakes; whereas, if the clang which is to be preferred is relatively rich and the other relatively pure, that will lead to a corresponding decrease in the number of mistakes. In the former case, this motive operates negatively; in the latter, positively.

B. *Similarity of dissonance or semi-dissonance quality.* It is easier to compare two dissonances of the same kind such as c′d′-c′d′^{b} than to compare two dissonances of different kinds such as c′d′-cb^{b}, regardless of their order of rank, because it is easier to compare two things of the same kind than two things of different kinds. Like motive A, this may operate either negatively or positively. If the comparison is between dissonances of different kinds, it will increase the number of mistakes, whereas if it is a comparison of dissonances of the same kind, it will tend to lessen the mistakes.

While, from one point of view, this is a motive for constant error, it is, from another point of view, a condition for an actual difference, either positive or negative. That is, it makes certain comparisons actually easier or more difficult (according as it acts negatively or positively) than the difference in rank in the norm would indicate.

C. *Conspicuous roughness.* Roughness in the dissonances is by far the easiest mark of dissonance. c′d′, *e.g.,* is always conspicuous for its roughness; and, wherever that interval is involved, it is relatively easier to detect it than to detect a dissonance of the c′b′ type. This again may operate either positively or negatively. If the poorer clang is c′d′, this motive acts positively and makes the judgment easier; whereas, if c′d′ is the better clang, it acts negatively and makes the decision more difficult.

Like motive B this is also a condition of actual difference in the ease or the difficulty of recognizing a given interval in which it operates. In that respect motives B and C differ from motive A, which is merely a motive for constant error; they are also conditions of actual difference.

The cases grouped under these motives are really merely illustrative, because not only are there more instances in which two motives coöperate, but it is a practical certainty that there are numerous cases in which two or more motives conflict and result in an interference which reduces or eliminates the net error. There are also many cases in which more than two motives for constant error are present. Those mentioned are, however,

probably the most important, because they go far to account for the deviation of the empirical order from the true order of rank.

There are undoubtedly other motives present, but for a rough preliminary analysis, we may arrange the intervals which were excessively high or excessively low in mistakes, as indicated in Fig. 1, under the negative or positive aspect respectively of each of these three motives as follows:

After each interval is indicated the per cent. of mistakes of each pair.

A. *Richness of tone quality*

Negative

Excessive Mistakes

$c'g'-c'a'$	(60)
$c'f'-c'e'^{b}$	(60)
$c'e'-c'g'$	(56)
$c'c''-c'a'$	(53)
$c'e'-c'c''$	(50)
$c'g'-c'f'$	(47)
$c'e'^{b}-c'a'$	(57)
$c'f'-c'c''$	(43)
$c'g'-c'a'^{b}$	(40)
$c'g'-c'e'^{b}$	(50)
$c'c''-c'a^{b}$	(45)
$c'e'^{b}-c'c''$	(46)
$c'g'^{b}-c'c''$	(25)
$c'g'-c'b'^{b}$	(24)
$c'c''-c'd'$	(22)
$c'f'-c'a'^{b}$	(49)
$c'f'-c'g'$	(45)
$c'c''-c'b'^{b}$	(16)

Positive

Minimum number mistakes

$c'g'^{b}-c'e'^{b}$	(22)
$c'e'^{b}-c'b'^{b}$	(11)
$c'g'^{b}-c'b'$	(7)
$c'a'^{b}-c'b'$	(4)
$c'e'^{b}-c'b'$	(5)
$c'f'-c'b'$	(6)
$c'e'-c'b'$	(9)

B. *Similarity in tone quality*

Negative

Excessive mistakes

$c'd'^{b}-c'b'$	(51)
$c'g'^{b}-c'g'$	(34)

Positive

Minimum number mistakes

$c'e'-c'a'^{b}$	(17)
$c'e'-c'e'^{b}$	(15)

C. *Roughness*

Positive

Minimum number mistakes

$c'd'-c'b'$	(19)

A-C *Musical Quality and Roughness*

Positive

Minimum number mistakes

$c'g'^{b}-c'd'$	(15)
$c'g'^{b}-c'd'^{b}$	(4)
$c'e'-c'd'$	(6)
$c'd'^{b}-c'e'$	(4)
$c'f'-c'd'^{b}$	(6)
$c'a'-c'd'^{b}$	(4)

If we examine the curves showing the demerit assigned (heavy solid curve, Fig. 1) and the result of weighting the demerits for each pair (dotted curve, Fig. 1) we find that the actual order of difficulty is not inversely proportional to the distance in the order of rank, as is assumed in the system of demerits. This is due primarily to the constant errors which have been noted, of which the chief one is the tendency to be influenced by motive A, musical agreeableness. But, as was pointed out above, motives B and C are also causes or conditions of *actual clearness of difference*. The norm is based on the actual order of consonance-dissonance. These notions will, therefore, operate to make the empirical order of rank legitimately different from that of the norm, because it makes certain pair-differences either more or less clearly perceptible.

Malmberg found that the empirical rank in 1913 did not correspond to the norm. The same was found for these records of 1915 which were taken by the same method. A comparison of the two deviations from the norm is shown in Table I, which gives, first, the order of rank in the norm as established by Malmberg; second, the empirical order as determined by the per cent. of choices in the 1913 records; and, third, the empirical order of rank as established on the basis of the per cent. of choices in 1915 records.

TABLE I

Malmberg norm		*Empirical order of 1912-13*		*Empirical order of 1915*	
		Rank	% of Choices	Rank	% of Choices
1	c′ c″	2	12.4	3	11.4
2	c′ g′	5	10.0	7	10.1
3	c′ a′	3	11.8	2	12.2
4	c′ e′	1	13.1	1	12.7
5	c′ f′	6 or 7	9.6	4	10.8
6	c′ a'^b	6 or 7	9.6	6	10.5
7	c′e'^b	4	10.5	5	10.7
8	c′ g'^b	8	8.8	8	6.7
9	c′ b'^b	9	6.5	9	5.8
10	c′ d′	10	3.8	10	4.1
11	c′ b′	11	2.1	12	2.0
12	c′ d'^b	12	1.8	11	2.1

In this table it will be observed that the principal deviation is among the consonances, but extends somewhat downward

through the semi-consonances,—whereas the dissonances and one semi-consonance maintain their rank according to the norm, except for a very slight reversal of the last two in 1915, which is negligible. In these deviations from the norm we notice the same general principle that was designated above as motive A, to the effect that there is a tendency to give undue preference to the musically rich and harmonically desirable intervals and that, therefore, the relatively pure and thin consonances are forced down in rank. Thus, the octave is forced down to rank two in one group and rank three in the other. The perfect fifth is forced clear down to the fifth rank in one group and to the seventh rank in the other; but the sixth maintains its rank in one group and is raised one point in the other.

The major third shows the most notable change in that it is given first rank in both groups. The fourth practically maintains its balance, falling slightly in one group and rising slightly in the other. The minor sixth maintains its rank, and the minor third is raised three points in rank in one group and two in the other. That is, the most notable advances are the major third and the minor third which are both conspicuous for the very rich tone quality of the two-clang, and the most notable drop is that of the fifth and octave which, though pure, lack that very richness which gives preference to the ranks immediately below.

The greatest source of error in the test is, then, the failure to adhere rigidly to the definition of consonance by allowing musical agreeableness to play a rôle in the decisions. This factor must, therefore, be further controlled in future tests.

The analysis thus far then has shown that, if the system of weighting is to be continued, it should be readjusted empirically on the basis of records in which the above motives for constant error have been eliminated in so far as possible. To this problem we shall return later.

The analysis has also shown that certain intervals are undesirable either because too easy or too difficult, as the result of the operation of causes for constant error. The natural remedy for that is to eliminate these, and limit the material to

those pairs for which these sources of error are relatively absent.

In order to reduce the number of factors to the minimum and to give the widest range of choice from the material available, it was decided to select only eleven pairs—one for each degree of difficulty as represented by the original system of demerits, which is based on the order of rank in the consonance-dissonance norm. There is manifestly a great advantage in using a small number of pairs, other things being equal, because these may then be examined in fuller detail and be better controlled, aside from the fact that they are the best from the larger group of the original test.

The following points were kept in mind (a) Each interval in the octave shall appear at least once. (b) Since Malmberg's order differs from the customary order in harmony in the case of the intervals, c′a′ and c′f′, no pairs shall be chosen which would be affected by that reversal. (c) Since it is very difficult to judge an interval on the criterion of purity alone, and, since the tendency is to judge a rich clang better than a pure clang, because it is more pleasing, all comparisons which rest primarily upon the criterion of purity, or richness, according to Malmberg's classification of the criteria (Table VI) shall be avoided. For example, we do not use the comparison c′c″-c′e′ nor c′g′-c′a′. (d) In addition to the above four restrictions, the extremely difficult and the extremely easy comparisons, according to the showing in Fig. 1, shall be eliminated where consistent with the observance of the other restrictions. (e) These shall be comparisons of consonances with consonances, semi-consonances with semi-consonances, and dissonances with dissonances, and each of these three with each of the other two.

Reference to the Malmberg Table VI, showing the pairs arranged according to demerit, will help in keeping the following choice of intervals clear.

The following is a list of the pairs finally selected, with statement of the reasons for the choice:

Octave and minor second ($c'c''$-$c'd'^{b}$)—*eleven demerits.*

This was the only possible interval with that number of demerits.

Octave and major seventh ($c'c''$-$c'b'^{b}$)—*ten demerits.*

The only other interval with this number of demerits is a comparison of a smooth and pure consonance with a very rough dissonance.

Perfect fifth and major seventh ($c'g'$-$c'b'$)—*nine demerits.*

One of the other possibilities was $c'd'$-$c'c''$ which was discarded because of the abnormal number of mistakes. The other possibility, $c'a'$-$c'd'^{b}$, was discarded because it was a comparison of a pure consonance and a rough dissonance.

Major sixth and major seventh ($c'a'$-$c'b'$)—*eight demerits.*

This seemed the best of the four possibilities, because it is a comparison of a clear consonance with a clear dissonance.

Major sixth and major second ($c'a'$-$c'd'$)—*seven demerits.*

Of the four other possibilities, one contains a very rough dissonance, one contains $c'b'$ which we have already used three times, and the other two mixed comparisons are found to be hard to judge.

Perfect fifth and diminished fifth ($c'g'$-$c'g'^{b}$)—*six demerits.*

Four of the other pairs contain $c'e'$, $c'e'^{b}$, $c'a'$, and $c'a'^{b}$, respectively, which are so often incorrectly judged on account of their rich and pleasing qualities. The other one possibility contains $c'b'$ which cannot be used again. The result is that we have an interval which is relatively more difficult that its rank would indicate.

Major third and minor seventh ($c'e'$-$c'b'^{b}$)—*five demerits.*

There were six other possibilities. Two of these were $c'e'^{b}$-$c'g'$, and $c'a'^{b}$-$c'c''$, each of which is a comparison of a very pure consonance with a rich consonance which has been found too hard to judge considering its high demerit. Two others, $c'f'$-$c'd'$, and $c'g'^{b}$-$c'a'$, are too much of the type of the six pairs already chosen. The other possibility contains $c'b'$, which has already been used three times.

Perfect fifth and minor sixth ($c'g'$-$c'a'^{b}$)—*four demerits.*

This is a type of comparison that ought to be included, that of a pure consonance with a rich consonance, and, since the demerit is not too high, it seems best to use this pair here.

Major third and minor third ($c'e'$-$c'e'^{b}$)—*three demerits.*

This is a comparison of two rich consonances, and therefore relatively easy, but $c'a'$-$c'a'^{b}$, of the same order, would have been a better choice.

Major seventh and minor seventh ($c'b'$-$c'b'^{b}$)—*two demerits.*

This is a comparison of two dissonances, a type which must be included.

Perfect fourth and major third ($c'f'$-$c'e'$)—*one demerit.*

This was chosen in order to include the perfect fourth, but it is really too easy.

To determine in a preliminary way the effect of the use of these eleven pairs alone, the record on these intervals was taken out from the complete record of the sixty-six pairs in the 1915

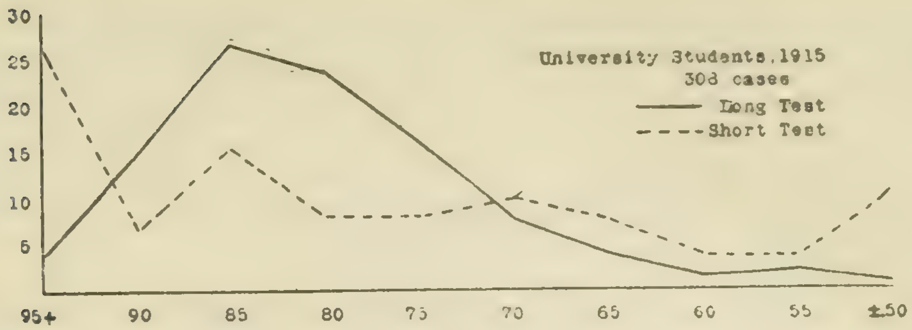

Fig. 2

test. Fig. 2 shows a comparison of the distribution according to the 66 unit test (the original) and the 11 unit test, by the method of weighted demerits. The curve shows that the 11 unit test is very much easier than 66 unit test. This difference in difficulty is, of course, due (1) to the elimination of the extremely difficult pairs, and (2) to the elimination of the principal motives for constant error.

A serious question, therefore, arises as to whether the test in this new form is too easy. It could, of course, be readily changed to any desired degree of difficulty without reintroducing seriously any of the sources of error which have been eliminated, but, in view of the desirability of having the norm for the adults comparable with the norm for children whose records run lower, it was decided to keep the material with the degree of difficulty here found. However, the new method of evaluation introduced below makes the test even with this material more difficult again.

So far the intervals have always been considered as occurring in the key of C. There is a decided advantage in varying the key to avoid the monotony of using the same notes, c′, c″, throughout. And there is no apparent reason why it should not be given in other keys, except perhaps that the tempered scale used on the piano would cause the intervals to have a different consonance value. According to a law developed by Stumpf[2] there should be no difference in the consonance of the various intervals in different keys even in tempered scale.

[2] Stumpf, Carl. Beiträge zur Akustik und Musikwissenschaft, III, Table IX. Leipsic 1898.

But there might be subjective influences which would affect the standing of the intervals in keys other than C. Furthermore, Stumpf did not make as fine distinctions as we are making here. Therefore, it was decided to give the test in the three keys of c, b^b, and g, to determine how they compared. This was done in October, 1916.

To one section of two hundred and twenty-two sophomores the list of eleven pairs was given four times in the key of c, and four times in the key of b^b, and four times in the key of g.

The result is shown graphically in Fig. 3 in terms of the

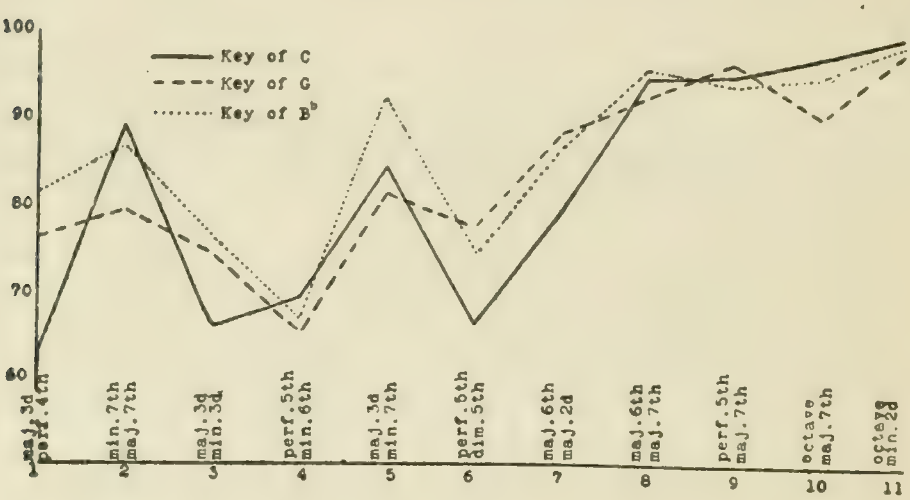

FIG. 3

percentage of correct judgments for each pair in each key. As will be seen, the correct judgments are approximately the same for each of the three keys used, which corroborates under any conditions, and to a serviceable degree, the principle laid down by Stumpf.

Fig. 3 also shows certain fluctuations in the degree of difficulty for different pairs, which are strengthened by the fact that they tend to agree for the three different keys. These may be interpreted in the light of the previous discussion.

The selection of the eleven pairs from the 1915 test yielded, of course, only one judgment for each interval, which is too little to be reliable. In a regular test, the pairs must be repeated as many times as possible to get a large number of trials.

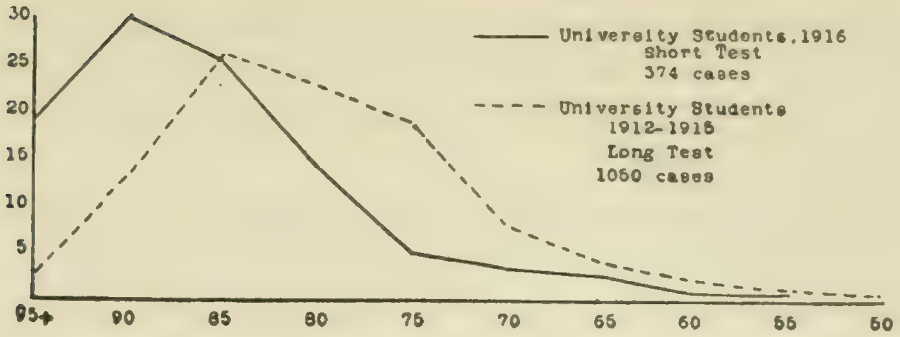

FIG. 4

Fig. 4 shows the distribution of this 1916 group of records as compared with the distribution of Malmberg's 1050 cases in the original form of the test. It will be seen that the difference between these two distributions is analogous to that found in Fig. 2. Fig. 4 is undoubtedly more reliable, because the dotted curve is based upon a larger number of cases, and the solid curve is based upon eighty-eight judgments as compared with eleven in Fig. 2.

If we should regard a selected group of pairs as, *e.g.*, the eleven intervals here selected, each played in three or four keys, as a single unit, it would simplify procedure very much to do

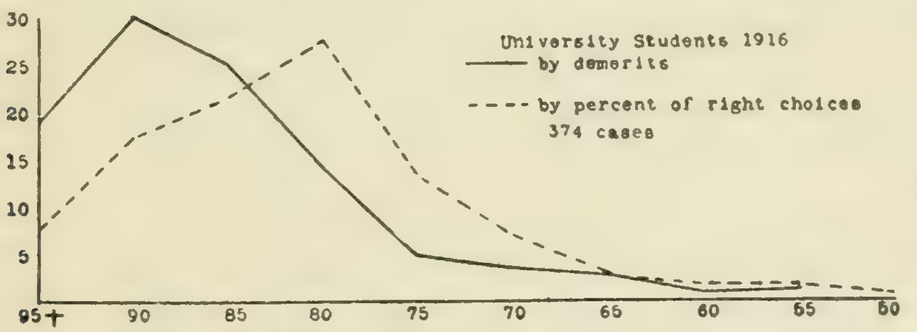

FIG. 5

away with the system of ranking errors by demerits, and use simply the per cent. of right judgments as a measure of ability. This is, of course, the reciprocal of the number of mistakes.

In Fig. 5, this method is compared with the original method of grading by weighted errors. The disparity between the two curves shows that the method of demerits tends to yield a higher ranking than the method of mistakes, as the latter curve falls

below at the better end, it rises correspondingly at the poorer end. This may not indicate any disparity; indeed, it points to an advantage in that the distribution by the former method made the test a trifle too easy.

The main question to consider is whether or not the order of ranking is disturbed seriously by the difference in tabulation. The Spearman coefficient for the correlation of rank between

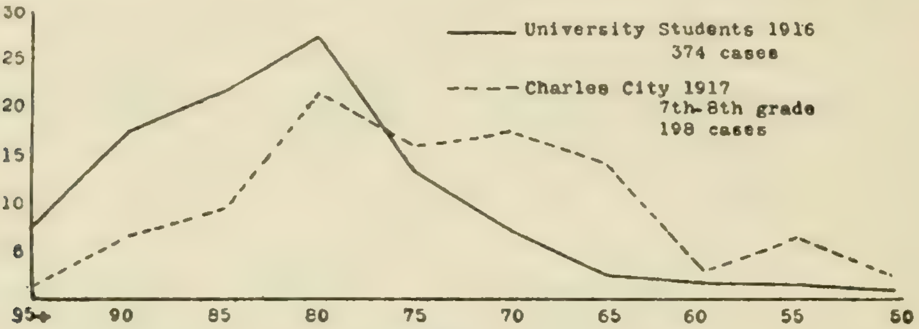

FIG. 6

the two methods for the three hundred and seventy-four cases of University students is R = .99, p.e., .02. This correlation is higher than the actual facts would indicate. But the Spearman coefficient of rank between the two methods for one hundred and ninety-nine seventh and eighth grade children is R = .89. p.e., .03. Since the correlation is very good in both cases we unhesitatingly adopt the new and simpler procedure for future use.

To make instructions simple and specific, the following definitions were written on the blackboard:

A good combination (of two tones) is one which is *smooth,* in which the two tones *blend,* and seem to *agree* and *fuse together* into one.

A bad combination (of two tones) is one which is *rough,* in which the two tones do not *blend,* and do not *seem* to *agree nor fuse together* into one.

Thus far all our efforts to establish norms have been limited to records of adults. All records taken on children have been evaluated temporarily in terms of the adult norms. We are at present engaged in establishing norms for children. Merely as a preliminary suggestion of what changes may be expected for chil-

dren we may use here the records of a group of one hundred and ninety-nine children, which comprise all the children in the seventh and eighth grades in the public schools of Charles City, Iowa.

The comparison of the distribution for these children with the distribution for adults is shown in Fig. 6, in which the curves are based upon the per cent. of correct judgments irrespective of demerits. Here is then a clear-cut picture of the ability of children of this age as compared with adults.

The following table is a comparison of the percentile rank of adults and children according to the per cent. of correct judgments. The first and second columns give the per cent. of correct judgments which an adult and a child respectively would have to make in order to get the percentile rank set opposite in the third column. It will be seen that in order to be average in the perception of consonance, or to get a percentile rank of 50, an adult needs to make 70% of correct judgments, while a seventh or eighth grade child must make 54% of correct judgments.

Adult	*Child*	*% Rank*
100	97	100
86	79	90
82	69	80
77	66	70
75	60	60
70	54	50
66	48	40
61	43	30
54	33	20
46	21	10
9	0	0

The establishment of norms for children and their evaluation for different ages represents a large task in the midst of which we are now engaged. In view of the strenuous nature of the test, one must be impressed with the relatively small difference between the achievement of adults and children. This fact becomes even more impressive upon examination of individual evidences. Thus, many of the children make as good a record as the best of the adults, and the disparity between the showing of children and adults comes largely from the incompetency of the comparatively dull children.

THE COMPARATIVE SENSITIVENESS OF BLIND AND SEEING PERSONS

BY

CARL E. SEASHORE AND T. L. LING[1]

The blind have unquestioned superiority in their ability to use their touch, hearing, and other senses for guidance. From observation of this, the conclusion is generally drawn that the sensitivity of their sense organs is correspondingly heightened.

"Through years of experience in the laboratory, the conviction has gradually grown upon me that a more radical distinction should be made between sensitiveness and ability to use a sense; *i.e.*, between inborn sensory capacity and acquired ability or skill. From time to time I have taken the opportunity of comparing my own sensitiveness in touch and hearing with that of blind persons distinguished for ability in guiding themselves by hearing and touch; and in no case did I find that the blind persons possessed any significant superiority to myself in sensitiveness to touch and hearing, although some of the blind persons experimented upon were noted for their wonderful performances through hearing and touch."[2]

We finally found an opportunity to put the matter to an accurate and crucial test. Sixteen blind students from the Iowa College for the Blind at Vinton, Iowa, and fifteen pupils from the Iowa City High School were selected as cases for comparison. The ages of the blind ranged from sixteen to twenty-six and of the seeing from fourteen to nineteen. In selecting the blind, only those were chosen who had been totally blind for more

[1] The writers acknowledge their great obligation to Superintendent Eaton for courtesies and coöperation at the School for the Blind and to Dr. Mabel C. Williams for taking responsible part in the making of the measurements, and taking the case histories of the blind children.

[2] Seashore, Carl E., Elemental Tests in Psychology, *J. of Educ. Psychol.*, 1916, 7, 81.

than five years, who were generally otherwise both mentally and physically sound, and were now in high school grades. This resulted in the selection of the ablest pupils in the school. The seeing pupils were taken at random, being selected by the principal on the ground of convenience of schedule. On each of these the following six measurements were made.

List of tests

1. *Localization of sound.*—Discrimination for the direction of sound was measured in terms of the angular displacement in the horizontal plane directly in front of the observer. A click in a telephone receiver served as stimulus. The receiver was mounted at one end of a two-meter bar which was pivoted at the middle and carried a pointer running over a scale of degrees at the other end. This apparatus was mounted at such distance in front of the observer that the receiver moved at the distance of one meter from the center of the head. The click was produced by the rapid make and break of the circuit through the mercury key. This whole arrangement proved very accurate and convenient, inasmuch as the angular displacement at this point and in this direction is very small. After a preliminary determination of the threshold one hundred trials were made by the method of constant stimuli.

2. *Discrimination for the intensity of sound.*—This was measured by determining whether the second of two consecutive tones which differed in intensity only, was stronger or weaker than the first. The tones were produced by a one hundred vibration electro-magnetic fork in circuit with the audiometer[3]. A standard current of 1.2 volts was used and No. 30 on the audiometer was taken as the basal unit, the compared tones being either stronger or weaker. This provides a tone clearly audible and yet not unpleasantly strong. The records were kept in terms of the units of the audiometer, which represent approximately equal psycho-physic steps. After preliminary determination of the threshold, one hundred trials were given by the method of constant stimuli.

[3] Seashore, New Psychological Apparatus, *Iowa Studies,* Vol. II, pp. 153-163.

3. *Discrimination for lifted weights.*—A set of bottles forty millimeters in diameter and ninety-five millimeters high was arranged as follows: 3 standards at 80 grams each and 12 compared weights yielding a series—81, 82, 83, 84, 85, 86, 87, 88, 89, 90, 92, and 95 grams. The observers were blindfolded and the standard weights were changed in order to eliminate the temperature error. The weights were arranged with ample space on a small revolving table so that any desired weight could be swung into position without noise. After the preliminary determination of the threshold, one hundred trials were made by the method of constant stimuli.

4. *Discrimination for passive pressure.*—A modified form of the Stratton pressure balance[4] was used. This was built of aluminum which was lighter and therefore more sensitive than the original. By this form of the apparatus it was possible to increase or decrease the standard pressure instantaneously without the slightest jar. The standard pressure was five grams upon the outer side of the third phalanx of the first finger. The pressure location of this was changed slightly for successive trials and the least perceptible increase or decrease in weight was determined, one hundred trials being made by the method of constant stimuli after the preliminary threshold had been determined.

5. *Discrimination for active pressure.*—This was measured by determining under how many thicknesses of paper the position of a wire hair could be located by active exploration with the tip of the first finger. The apparatus consisted of a plate glass disc 10 cm. in diameter, mounted so as to revolve freely. In place of the hair a copper wire, No. 38 B & S, which is about the diameter of the average human hair, was stretched across the plate glass and with ends firmly fastened. Discs of paper cut from Old Veda Bond 17 x 22, twenty pounds to the ream, were placed over this wire, and the blindfolded observer was required to locate the direction of the wire through touch. Starting with ten sheets, five sheets were added for each trial until

[4] Stratton, Über die Wahrnehmung von Druckänderungen bei verschiedenen Geschwindigkeiten, *Philos. Stud.*, Vol. 12, 1896, pp. 525-586.

a failure occurred; then a single sheet was taken off or added until the greatest thickness through which the wire could be located had been determined. The record is therefore given in terms of the number of sheets of paper covering the wire. To one who has not performed this test, the actual results will seem to be very wonderful achievements, as the records show.

In this test much depends upon economic procedure in the hunting. The observers were therefore carefully taught how to proceed: *e.g.*, to run tip of finger from left to right and, if the wire was not found in that stroke, to run it in a line at right angles to the first; the wire being found by such economic procedure, it was comparatively easy to trace it. If it was not found in the first two strokes, these cardinal movements were continued until the observer acknowledged failure.

6. *Tactual space.*—This consists in the determination of the two-piont threshold in the transverse direction on the tip of the first finger and in the transverse direction on the inside of the forearm, 5 cm. above the wrist.

It is unfortunate that no more than one hundred trials could be made in each of these tests, where the method of constant stimuli was employed, but it must be remembered that much time, more than was required for the actual measurement, was spent in making preliminary determination and securing adaptation which is necessary for tests of this sort. Indeed the elemental character of the tests often hinges largely on full knowledge of all factors involved and skill in preparing the observer so as to eliminate sources of subjective and objective variables.

Case histories: 1. The blind

Case 1. Age, 16, female. Blindness began three days after birth. Can only distinguish light from darkness. Attractive, bright, quick, responsive, and breathlessly attentive, but easily fatigued; reads point with either hand at conversational rate; likes typewriting, plays the piano well, and sings; can hem by hand but does not use a sewing machine; taste and smell are good; has no difficulty in finding her own way on the premises.

Case 2. Age, 26, female, single. Cause of blindness *iridocyclitis;* trouble came on gradually from ten to nineteen. Can tell where windows are in a room; healthy and cheerful, but slow; responsive and tried hard in the tests;

reads point slowly with right hand and follows with left; studied typewriting for a while but dropped it; can sew by hand or machine, crochet, knit, tie, and do housework; has learned basketry, weaving, and broom making; has studied piano two years; taste and smell good.

Case 3. Age, 25, male, single. *Congenital cataract.* Can tell light from darkness; taste and smell good; relies much on hearing; is of a nervous temperament, unattractive, but quiet; critical and sure of himself; reads point slowly with left index finger; uses typewriter, studied piano nine years; profession, piano tuning; also makes brooms, ties, and weaves.

Case 4. Age, 19, female. Blindness due to *leucoma adherens* from *blennorrhea neonatorum;* commenced shortly after birth; can locate windows with left eye; can sort laundry by smell; makes much use of hearing; was fidgety and sighed frequently during the test; bright and alert; reads point rapidly with both hands; plays piano well; sews either by hand or machine; skillful in basketry and fancy work.

Case 5. Age, 23, female, single. Blindness due to *sympathetic ophthalmia;* ran a knife into her right eye when a child and failed to get proper treatment; can tell daylight from darkness with her left eye; goes about alone in usual places but never on the street; reads point with both hands at conversational rate; sews, ties, crochets, etc.; tries to do all kinds of house work; plays the piano well; fine young woman, neat and pleasing, quiet manner.

Case 6. Age, 18, male. Blindness due to *phthisis bulbi* from *blennorrhea neonatorum;* pleasing appearance, alert, attractive, but fatigues readily; reads point with left index finger at conversational rate and anticipates with right; uses typewriter well; is musical and sings well; guides himself in the environment without difficulty.

Case 7. Age, 19, male. Blindness due to *phthisis bulbi.* Stuck a shoe awl into right eye when nine years old and rapidly lost sight of both eyes a year later. Guides himself in environment with skill and ease; taste and smell good; basket weaving, broom making; carpet weaving; reads point slowly with left index finger; plays piano and violin a little; likes typewriting.

Case 8. Age, 19, male. Blindness due to chronic *conjunctivitis;* began when six months old. Can locate windows and see the light of the sun but not the sphere itself; taste and smell good; cheerful, reliant, and mentally probably a little above the average; reads point; tunes piano; takes typewriter apart just for the fun of it; has a work shop and whole set of tools at home; built a house seven feet square twenty feet up in a tree two years ago.

Case 9. Age, 19, female. *Atrophy of optic nerves* due to accident at age of fourteen. Intelligent and responsive and tried hard in the tests; uses typewriter; is skillful in basketry and fancy work.

Case 10. Age, 17, male. Blindness due to chronic *iridocyclitis* caused by *spinal meningitis* at six months. Lost sight suddenly at age of twelve; taste and smell good; reads point with either hand at conversational rate; can work in the garden, but not hoe; makes baskets and hammocks.

Case 11. Age 19, female. Blindness due to *sympathetic ophthalmia;* ran scissors into left eye when four years old; right eye failed gradually. Can see light, locate windows, and avoid obstacles; ordinary intelligence; reads

point with left index finger at conversational rate but not with right hand at all; plays the piano; uses typewriter, sews, and does fancy work.

Case 12. Age, 19, male. Blindness due to *maculae corneae from blennorrhea neonatorum.* Goes about the city alone; learns new towns quickly; conscious of using hearing extensively; responsive and attentive; boastful; reads point well; plays pipe organ, piano, and violin; gives concerts.

Case 13. Age, 20, female. Blindness due to *congenital atrophy* of optic nerves. Taste and smell good, but does not use them much; good orientation; nervous; reads point with right index finger, left following; uses typewriter, sews, and does fancy work.

Case 14. Age, 21, female. Blindness due to *phthisis bulbi* from *blennorrhea neonatorum.* Can detect light with the left eye; reads point and braille with either hand but faster with the right; plays piano and sings well; sews and does fancy work, and house work.

Case 15. Age, 25, female, single. Blindness due to *phthisis bulbi* from *blennorrhea neonatorum.* Cannot detect light; taste and smell good; responsive, good application; reads braille better than point; sews and does fancy work and light house work.

Case 16. Age, 20, female. Blindness due to *phthisis bulbi* from *blennorrhea neonatorum.* Can detect light and sometimes colors and outlines of persons; bright, very attentive; reads point with right index finger rapidly and is considered one of the best; uses typewriter, plays piano, and pipe organ, sews, and does house work.

2. *The seeing*

Case 1. Age, 18, male, junior. A good student.
Case 2. Age, 14, male, freshman. A medium student.
Case 3. Age, 17, female, junior. A medium student.
Case 4. Age, 16, male, freshman. A medium student.
Case 5. Age, 21, male, senior. A superior student.
Case 6. Age, 17, male, freshman. A medium student.
Case 7. Age, 17, male, freshman. A medium student.
Case 8. Age, 19, female, junior. A good student.
Case 9. Age, 18, female, senior. A very bright student.
Case 10. Age, 17, male, senior. A medium student.
Case 11. Age, 18, male, senior. A medium student.
Case 12. Age, 18, female, senior. A medium student.
Case 13. Age, 15, male, sophomore. A medium student.
Case 14. Age, 15, male, freshman. A very poor student.
Case 15. Age, 15, male, sophomore. A good student.

TABLE I—*The blind*

Cases	Localization of Sound	Intensity of Sound	Lifted Weight	Pressure	Touch	Tactual Finger	Space Arm
	Degrees	*Units*	*Grams*	*Grams*	*Sheets*	*m. m.*	*m. m.*
1	1.4	1.5	3.1	.64	25	1.5	20
2	3.0	2.0	5.6	.32	32	1.8	
3	5.2	.5	4.0	.63	31	1.3	51
4	1.4	1.3	3.2	.18	21	1.6	30
5	1.3	1.4	4.8	.66	35	1.5	35
6	1.8	.8	4.3	.46	30	1.1	21
7	2.1	2.0		.34	30	.6	27
8	2.0	.8	2.5	1.31	35	1.0	12
9	4.8	1.4	5.2	.66	29	1.1	47
10	2.0	.7	3.0	.38	10	.9	20
11	2.1	1.8	6.6	.38	25	.8	5
12	1.3	.7	1.7	.35	38	1.3	24
13	1.4	1.1	4.0	.22	19	2.3	20
14	1.2	1.2		.51	18	1.3	16
15	1.6	.9	4.0	.70	15	.8	30
16	1.6	.8	4.0	.25	20	2.0	24
Median	*1.6*	*.9*	*4.0*	*.38*	*25*	*1.1*	*24*
Ave..	*2.1*	*1.2*	*4.3*	*.50*	*26*	*1.3*	*25*
M.V.	*.8*	*.4*	*.9*	*.20*	*6.7*	*.4*	*8.6*

TABLE II—*The seeing*

Cases	Localization of Sound	Intensity of Sound	Lifted Weight	Pressure	Touch	Tactual Finger	Space Arm
	Degrees	*Units*	*Grams*	*Grams*	*Sheets*	*m. m.*	*m. m.*
1	1.5	1.0	2.9	.13	41	.6	26
2	4.4	1.6	5.2	.98	14	2.1	19
3	2.0	1.0	2.4	.87	18	1.0	35
4	2.2	1.0	4.6	.50	28	1.7	38
5	2.2	.7	3.0	.35	40	1.6	33
6	1.4	1.6	3.3	.72	47	1.2	26
7	2.8	1.1	4.0	.44	36	1.1	46
8	1.9	.9	2.2	.58	35	2.1	22
9	2.7	1.0	4.1	.80	30	1.6	18
10	1.6	.5	4.2	.40	21	2.0	35
11	1.9	.5	3.1	.73	32	1.0	11
12	1.6	1.2	4.4	.54	45	1.6	23
13	.5	.3	1.7	.43	25	.2	10
14	4.0	1.5	3.1	.44	21	2.0	18
15	2.9	2.2	4.4	.23	20	.5	25
Median	*2.0*	*1.0*	*3.3*	*.44*	*28*	*1.2*	*23*
Ave.	*2.2*	*1.1*	*3.5*	*.54*	*30*	*1.3*	*25*
M.V.	*.7*	*.4*	*.8*	*.18*	*8.6*	*.52*	*7*

Statement of the results

Localization of sound.—In this test the blind are slightly superior to the seeing, especially in terms of the median. The blind vary more than the seeing, the coefficient of variability being 33 and 48 for the blind and the seeing respectively. The

records of the former range from 1.2 to 5.2 degrees while the latter vary from .5 to 4.4. The finest record of all was made by a seeing person.

The intensity of sound.—In this test the blind are slightly superior to the seeing according to the median but inferior by the same amount according to the average. The coefficient of variability is practically the same, namely, 32 for the blind and 33 for the seeing. Here again the best record of all is made by a seeing person, the same one, No. 13, who made the best record in localization of sound.

Lifted weights.—In this test the blind are inferior to the seeing both according to the median and the average. The coefficient of variability is approximately the same, 23 for the blind and 24 for the seeing. Here the finest record made is the same for the blind as for the seeing. But there are more extremely poor records among the blind than among the seeing.

Pressure.—In this test the blind are somewhat superior to the seeing both according to the median and the average. The blind are more variable, the coefficient being 40 for the blind and 33 for the seeing. The records of the blind range from .18 to 1.31 and those of the seeing from .13 to .98.

Touch.—In this test the larger the number the better the record. The facts therefore show that the blind are inferior to the seeing both according to the median and the average. There is but slight difference in the variability although the seeing have four records that are better than the best record of the blind.

Tactual space on the finger.—In this test there is no significant difference in the median and the average. But the variability of the seeing is greater, the coefficient being 40 for the seeing and 27 for the blind. The seeing have two records that are better than the best of the blind.

Tactual space on the arm.—Here again there is no significant difference in the median and the average nor in the coefficient of variability.

The general conclusion then so far as the net results of

the facts are concerned must be this, that, while there are slight differences shown in the Tables, there is no *significant* constant tendency in the records to favor either the blind or the seeing. In other words, *the blind and the seeing under these circumstances are, on the whole, equally sensitive to the direction of sound, intensity of sound, lifted weight, passive pressure, active pressure, and tactual space.*

Interpretation of these findings

These experiments were planned on the assumption that the tests would be elemental; *i.e.,* that the measurements were so simple and natural that training, experience, and special skill would not affect the results. In other words we aimed to test native capacity as distinguished from acquired ability.

From one point of view we may say, therefore, that the outcome of this series of experiments is to prove that the specific tests here used are elemental, because the records do not favor those who have acquired very marked skill and efficiency in these senses. That itself is an important feat in experimental procedure, because it sets up a new standard for psycho-physic tests. Records of psychological and psycho-pedagogical experiments for the past twenty-five years are full of examples showing how capacities like those here mentioned improve with age, intelligence, and practice. If the above conclusion is right we must re-submit those findings to determine to what extent the improvement found was due to the maladaptation of the experiment and the lack of skill of the experimenter rather than to characteristic change in the persons measured.

We cannot too strongly emphasize the necessity of reducing the procedure in an experiment of this kind to its simplest terms so that the record shall be of the actual capacity rather than of the possession of skill in adjusting oneself to a complex situation. There are scores of pitfalls in each one of the above experiments, which, if allowed to enter uncontrolled, would detract from the elemental nature of the test.

If we regard the results then from the converse side, they

prove that the extraordinary skill in guidance through hearing and pressure and other senses in the blind has its basis in apperception rather than in sensation or in the direct and immediate judgment of difference. The person who reads the point print has learned to give meaning to sensory impressions which the seeing person tends to neglect although possessed of the same power as the blind in sensing these differences.

If, then, we say that the blind person who reads point print, almost as we read with our eyes, and guides himself through hearing in many respects almost as we guide ourselves with our eyes, and we regard these performances as representative of the thousands and thousands of situations which occur in life, we arrive at the conclusion that the seeing have countless wonderful capacities lying latent and unrecognized so far as conscious use is concerned. The apparent squandering of sensory capacities may well be compared with the great waste in the struggle for existence by various forms of prolific extravagance in reproduction. We are very richly endowed with capacities of which we employ only relatively few, and these in a very inadequate way. The achievements of Helen Keller, *e.g.*, illustrate this.

But after all, it is difficult to appreciate the full extent to which we actually do depend upon the sort of fine sensitiveness here measured. While most of us do not read with our finger tips or guide ourselves with our ears when walking, we do make constant use of these senses in ordinary activities, most of it unconsciously. It may, therefore, be said, from one point of view that, that while seeing persons are not trained in certain specific acts of skill, they are trained in the use of the sensibilities involved in such skill. Indeed we are born with hierarchies of blind "instinctive skill". We often appeal from the sense of sight to touch and we constantly respond to the environment through hearing without resort to the physical mechanism in terms of which we think.

Technically all these tests involved discrimination as distinguished from sensitiveness or sensibility. The sensory judg-

ment is reduced to its lowest terms and is not far different from perception in its immediacy.

The conclusions of these experiments may then be summed up in the following statements:

First. The blind who are skillful in the use of touch, muscle sense, and hearing are not more sensitive or keen in sensory discrimination than seeing persons when fundamental capacities are tested.

Second. The necessity of making our psycho-physical tests elemental is strikingly indicated by the results of these tests.

Third. Development of the use of a sense consists not in the heightening of sensitivity or sensory discrimination but in the development of complexes and meanings in terms of these.

THE ELEMENTAL CHARACTER OF SENSORY DISCRIMINATION

BY

Carl E. Seashore and Kwei Tan

The problem here reported grew out of that of the preceding article by Seashore and Ling in which it was demonstrated that the blind, though so largely guided by the sense of hearing and touch in their daily activities, are not superior to seeing persons in elemental tests of fundamental capacities in these senses. In that investigation an elemental test was defined as one in which the "measurements were so simple and natural that training, experience, and special skill would not affect the results." This fact of the equality of the blind and the seeing was regarded as proof of the elemental character of the test.

To gather proof from another angle it was decided to take one of these tests, namely, discrimination for the intensity of sound, and submit a number of observers to prolonged practice for the purpose of determining whether or not ability in this test improved with practice. The procedure was exactly the same as reported in the preceding article. The audiometer was used with the 100 v.d. fork, the current being kept at 1.2 volts. The mercury was kept clean, and the physical conditions and the control of the instrument were satisfactory throughout. A single increment was employed for the whole series, namely, the step from 38 to 39, on the audiometer, and the test consisted of one hundred trials for each day under conditions as nearly constant as possible, both subjective and objective. The record was kept in terms of the number of right cases. The observers, designated by letter in the following table, were all students in Psychology who had had some experience in experimental work but no training in this particular test.

In a few cases two or even three hundred trials were taken

during the same day, but the general effort was to get one set of observations a week. The experiments were conducted in the light-sound-and-jar-proof room in the psychological laboratory with both observer and experimenter present in the room. The room was ventilated freely every twenty minutes

Records of these trials are shown in Table 1, in which the numbers given are the number of right decisions in each one hundred trials for each of the chosen observers. The average and median are given only for thirteen periods. It is evident that it would not be fair to continue the average beyond that point for the reason that each observer represented a different degree of ability and the dropping out of, *e.g.*, D. and H. W. at the end of the thirteenth day would distort the average record seriously.

The fluctuation in these figures may appear at first as being rather large, but when one considers that the record is in terms of right and wrong cases, there is really a very satisfactory uniformity on the whole.

Records of introspections and objective observations were kept faithfully, but as these are of an ordinary nature it is hardly worth space to detail them here. Suffice it to say that the variations in the per cent. of right cases is not greater than one would ordinarily find in a single one hundred trials on successive days under very favorable circumstances in any type of discrimination.

TABLE I—*The effect of practice on discrimination for the intensity of sound*

	A.	F.	H.	T.	G.W.	N.W.	C.	D.	Go.	Gr.	H.W.	*Median*	*Ave.*
1.	88	91	94	89	86	65	88	76	86	95	74	88.1	84.7
2.	90	92	92	89	88	64	95	75	88	94	65	89.0	84.7
3.	94	97	92	83	91	69	89	78	90	90	77	89.7	86.3
4.	93	95	80	78	91	87	97	86	80	93	79	87.3	87.1
5.	92	78	76	68	91	89	95	83	89	92	76	88.0	84.4
6.	91	80	73	72	87	89	90	68	94	88	82	86.5	83.0
7.	94	99	75	89	79	87	98	69	89	98	89	90.8	87.8
8.	92	99	90	94	88	91	95	82	94	96	76	91.5	90.6
9.	92	88	93	92	83	89	99	78	89	93	63	90.0	87.1
10.	88	90	95	95	94	85	88	82	82	86	69	87.0	86.7
11.	86	94	91	93	98	75	97	81	83	76	71	84.5	85.9
12.	87	91	78	85	100	79	90	82	87	91	61	86.0	84.6
13.	95	91	89	82	95	79	91	84	85	85	59	86.4	85.0
14.	87	96	84	93	95	90	89		93				
15.	89	96	99	85	94	75	90		80				
16.	88	96	94	96	91	84	90						
17.	90	93	96	95	85	90	93						
18.	83	86	94	91	94	86							
19.	95	96	97	94	96	93							
20.	84	96	96	67	92	90							

Looking then at the average and the median, we see that these run practically constant for the thirteen days. Beyond the thirteenth day each observer's record must be judged by itself, and it will be found that in these there is no marked tendency toward raising or lowering the general average for a given observer. We must, therefore, arrive at the conclusion that, for periods of training represented by this series, there is in this test no marked evidence of improvement with practice. For a single observer one hundred trials are inadequate, by the present method, but by taking a larger number of observers under similar conditions and by treating the results together in the average and the median, we compensate in part for this shortcoming.

Some of the fluctuations must, however, be accounted for by recognizing a certain degree of complexity in the problem. For example, various observers reported from time to time that they observed, or were trying, accessory aids in judging; for example, the sound was visualized and the judgment was made in terms of distance, the fainter sound being farther away. Or the difference was imaged in terms of motor tendencies, the lower sound calling forth, in one type, less effort because it was easy to hear and, in another type, the larger effort as a matter of counteraction. On the whole the introspections do not reveal any case in which the adoption of a particular method seems to have changed capacity permanently from the point of adoption.

Furthermore, the situation was not entirely free from objective disturbances. The observations being taken in the sound-light-and-jar-proof room, exceedingly faint accessory stimuli did prove disturbing. In an earlier experiment of this kind, one observer said that he could hear his backbone creak under these circumstances. This was probably not true, but it illustrates the tendency to be affected by obscure stimuli. There are, of course, momentary disturbances of itching, pain, or pressure, due to the holding of the receiver to the ear. Together with this there is also the subjective factor, the change

in disposition, physiological tone, aggressiveness, etc. from day to day.

Bearing in mind that this test was selected as a sample from among six other tests which were supposed to be elemental, it is reasonable to conclude, in the absence of evidence to the contrary, that the same elemental character could be established for the other tests reported in the foregoing article.

The only one of these on which there is any considerable amount of reliable literature available is that of lifted weights. This test has, perhaps, been studied with more exactness than any other test in the psycho-physical literature. Extraordinary refinements have been made in the discovery of the conditions for change in the capacity. Most of these are in the nature of the laws of illusions. But, as we look back upon literature as a whole, we find that ordinarily a careful observer does not improve with training. He may and usually does go through a cycle of changes which may be traced to the operation of certain motives for illusion. The same would probably be observed in any psycho-physical test that can be subjected to sufficiently rigorous study.

We have, then, in this and the preceding article evidence of the elemental character of the tests from two points of view, namely, that of highly specialized sense training in actual life, in the blind, and that of accurately controlled measurement of training in a comparatively short series.

There is a third line of evidence that must be sought. In fact, these experiments were suggested by the discovery that in pitch discrimination the actual native capacity in sensory discrimination for pitch does not develop with age. Whether that rule applies to these six tests should be worked out with great care upon a series of ages of children from, say eight to sixteen. On all the tests, we have records of children showing clearly that there is improvement in the record, which we may call the "gross record" or "total output", from year to year up to about fifteen. But the challenge set up by this experiment is to repeat those experiments and analyze them to find out to what extent this improvement with age consists

in overcoming the difficulties which are due to the incomplete mastery of the test by the observers. In other words, we may lay down this as an hypothesis for further experiment: In psycho-physic tests of sensitivity or discrimination the conditions must be so simple and so accurately under control that they are equally fair to the skilled and the unskilled, the trained and the untrained, the young and the old. Only then shall we measure sensory discrimination in its true capacity.